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FOR PROMOTING USEFUL KNOWLEDGE

VOLUME 82

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CONTENTS

PUBLICATIONS

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PROCEEDINGS ($3.00 per volume)

VOLUME 79

VOLUME 80

William Bell Dinsmoor. Archæo ogy and Astronomy. pp. 95–173.
January, 1939. $1.00.

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No. 4. SYMPOSIUM: INTRINSIC AND ENVIRONMENTAL FACTORS IN AMERICAN POPULATION GROWTH.

Conrad Taeuber. Agriculture and Current Population Trends. pp. 477–489.

Warren S. Thompson. The Social Environment as a Factor in Population Growth. pp. 491–498.

Frank W. Notestein. Intrinsic Factors in Population Growth. pp. 499–511.

John C. Flanagan. A Study of Psychological Factors Related to Fertility. pp. 513–523.

Clyde V. Kiser. Voluntary and Involuntary Aspects of Childlessness. pp. 525–539.

Mortimer Spiegelman. Mortality in Relation to Widowhood. pp. 541–558.

Waldemar B. Kaempffert. Technological Advance in Relation to Population Trends. pp. 559–568.

Carl Cleveland Taylor. Prospective Development of Cultural Patterns in Rural America and Their Possible Influence on Population Trends. pp. 569–585.

Harry L. Shapiro. Some Reflections of an Anthropologist on the Future of our Population. pp. 587–600.

Alfred J. Lotka. Contact Points of Population Study with Related Branches of Science. pp. 601–626.

February, 1939. $0.75.

VOLUME 81

No. 1. Ralph A. Beebe. Adsorption Calorimetry on Catalytic Materials and an Account of Some Measurements on Chromic Oxide at – 183° C. pp. 1–14.

Nabih Amin Faris. *The Iḥyā' 'Ulūm al-Dīn* of al-Ghazzāli. pp. 15–19.

E. Horne Craigie. Vascularity in the Brains of Tailed Amphibians. pp. 21–27.

Horace G. Richards. Land Mollusks from Corn Island, Nicaragua. pp. 29–36.

H. A. Spoehr and Harold W. Milner. Starch Dissolution and Amylolytic Activity in Leaves. pp. 37–78.

E. R. Fenimore Johnson. The Preservation and Abuses of Motion Picture Film by Scientific Institutions. pp. 79–92.

M. B. Emeneau. The Christian Todas. pp. 93–106.

May, 1939. $0.50.

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DONALD H. MENZEL. Problems of the Solar Atmosphere. pp. 107–124.

CHARLOTTE M. SITTERLY. The Composition of the Sun. pp. 125–133.

RUPERT WILDT. The Constitution of the Planets. pp. 135–152.

S. CHANDRASEKHAR. The Internal Constitution of the Stars. pp. 153–187.

CECILIA PAYNE-GAPOSCHKIN. Variable Stars: A Plan of Study. pp. 189–210.

OTTO STRUVE. Stars with Extended Atmospheres. pp. 211–251. 4 pls.

FRED L. WHIPPLE. On the Physical Characteristics and Origin of the Supernovæ. pp. 253–264.

DORRIT HOFFLEIT. Observations of Supernovæ. pp. 265–276.

THEODORE DUNHAM, JR. The Material of Interstellar Space. pp. 277–293.

HENRY NORRIS RUSSELL. Stellar Energy. pp. 295–307.

June, 1939. $1.00.

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FLETCHER WATSON. Apparent Distribution of Meteoroid Orbits. pp. 481–491.

FLETCHER WATSON. A Study of Telescopic Meteors. pp. 493–504.

J. D. WILLIAMS. Binocular Observations of 718 Meteors. pp. 505–520.

J. D. WILLIAMS. Systematic Biases of Meteor Directions. pp. 521–524.

W. V. HOUSTON. Electrons in Metals. pp. 525–532.

DORRIT HOFFLEIT. On the Necessity for Spectral Sub-classification, with a Discussion of Preliminary Results for 1300 Southern Stars. pp. 533–545.

EDWARD W. BERRY. Geology and Paleontology of Lake Tacarigua, Venezuela. pp. 547–568. 7 pls.

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SYMPOSIUM

ON

THE TOTALITARIAN STATE

FROM THE STANDPOINTS OF HISTORY, POLITICAL SCIENCE, ECONOMICS AND SOCIOLOGY

NOVEMBER 17, 1938

TOTALITARIAN POLITICS

FRITZ MORSTEIN MARX

Associate Professor of Political Science, Queens College

(Read November 17, 1939, in Symposium on The Totalitarian State)

CONSTITUTIONAL government quarried its stone from the rock of reason. Of those whose title to power rested on power, it demanded a justification in terms of the interests of both the individual and the nation. It developed its architecture in conscious distinction from the bulking compactness that gave the state structure of absolutism the appearance of a fortress. It was deliberately civilian, delimiting and diffusing authority so as to insure a free flow of human energies for the benefit of the polity as a whole. It proclaimed man's emancipation from government bondage on the assumption that a liberated society would evolve its own formulae to provide a durable framework of civic allegiance.

Child of the "age of reason," constitutionalism as a movement of ideas was closely akin to those forces which in the eighteenth century carried forward the rationalist attacks upon faith as well as prejudice. Its rise thus coincides with the decline of religion. Its momentum is accelerated by the simultaneous advance of industrialization. Far-reaching dislocations and an unparalleled social mobility soon confront the emergent order. The vast transformations to follow pose a crucial question: Will enlightened consideration of individual self-interest translate itself autonomously into a public morality firm enough to give promise of political stability? The initial period of rapid economic expansion tries to escape the issue by postponement. The twentieth century will insist upon a final answer.

One reply is that of totalitarianism. Essentially, it is a reply in the negative. Its official elaboration must be distinguished from its pragmatic basis. The implicit premises are these. The revolution of rationalism laid the foundation for modern technology, but at the same time undermined the position of the church. Economic man, encouraged to be himself, remained de-

void of authoritative guidance in the quest of community. With intensified class pressure, the vacuum left by lost faith was bound to create psychoses of insecurity so severe as to present a challenge to the political system itself. A temporal Ecclesia of Leviathan would fill out the vacuum,[1] hold Ortega y Gasset's "mass man" in his place, and substitute millennial "order" for the impending chaos.[2] Instead of a problematical freedom and shrinking opportunity, it would offer the absolute security of belief and the guaranty of defined status for each and every faithful. The secular religion of the Ecclesia of Leviathan is totalitarian ideology. It provides anchorage for the "mass mind." It may even move mountains.

I

As a secular religion, totalitarian ideology is rich in allusions to things to come—to come through the ministrations of the political control group symbolized by the Leader, provided that the leader is supported by the "entire people." Any individual defection among the rank and file is consequently not only a defiance of authority but also criminal interference with the people's pursuit of happiness. The actual character of the "good society" to be attained in an unspecified future has remained intriguingly vague. The transfiguration is to be gradual. More important than immediate change is the perpetuity of progress. Between the New Rome and Greater Rome there are innumerable stages of the Almost-As-Great-As-Greater-Rome, to be followed by as many stages leading toward the Still Greater Rome. In the succession of accomplishments, new vistas open up, new potentialities present themselves, new ambitions come alive. Imagination does not always stop before realities.

Only Soviet Russia, in contrast with Fascist Italy and National Socialist Germany, has identified her ultimate objective at least in broad language, although merely by affirming the words of the prophets. Her avowed goal is the "stateless and classless society." In his *Origin of the Family, Private Property, and the State,* Friedrich Engels ventured this prediction:

[1] The ideational evolution is brilliantly traced by Erich Voegelin, *Die Politischen Religionen,* Vienna, 1938.

[2] For a suggestive analysis of the "dread of chaos" as a political factor, see Joachim Schumacher, *Die Angst vor dem Chaos,* Paris, 1937. I have reviewed this little-known book in the *Harvard Educational Review,* vol. 8, 1938, pp. 536–537.

> The society that organizes production anew on the basis of a free and equal association of the producers will put the whole state machine where it will then belong: in the museum of antiquities, side by side with the spinning wheel and the bronze axe.

In the *Anti-Dühring,* he was even more explicit:

> The first act in which the state really comes forward as the representative of society as a whole—the seizure of the means of production in the name of society—is at the same time its last independent act as a state. The interference of a state power in social relations becomes superfluous in one sphere after another, and then becomes dormant of itself. Government over persons is replaced by the administration of things and the direction of the processes of production. The state is not "abolished," *it withers away.*

To be sure, the Soviet state can wither away only after eradicating all remnants of capitalism and class. At present, Russia is still far from providing the observer with a legitimate test of Engels' forecast. She has not yet passed beyond the stage of proletarian dictatorship as the scheduled transition. The contours of the "stateless and classless society" have become no more visible than they were ten years ago. Said a sympathetic critic of the transitional dictatorship: "That process is a long one, and no one can set out the limits of its duration."[3]

In fact, it seems today entirely probable that the transition will freeze into permanency. The "new man with a new psychology" whom Lunacharsky urged the regime to "create" has been the product of the dictatorship and has seen nothing but the dictatorship. He has adjusted himself to the privilege system engendered by "socialist competition"—to the social and economic degradation of being below production standard; to the moderate luxuries enjoyed by the model worker; and to the prerogatives of the upper crust composed of planners, bureaucrats, engineers, and army men. He has also witnessed the metamorphoses of terrorist action. For Trotsky, the "red terror" was still "a weapon utilized against a class doomed to destruction, which does not wish to perish." In subsequent years it became an effective device for disciplining "deviationist wreckers," striking with a ruthlessness that never erred on the side of economy. By comparison with the shaky structure and the haphazard methods of tsarist government, Stalin's dictator-

[3] Harold J. Laski, *Communism,* New York and London, 1927, p. 145.

ship is a streamlined tower of strength. If the "new man with a new psychology" is to fit his present environment, he must appear sadly outmoded in the "stateless and classless society." Yet this should not cause him anxiety. There is a barely hidden emphasis in Paschukanis' comment: "The opportunists cherish pleasant dreams of the progressive disappearance of the proletarian state." [4] When dropping this hint in 1935, its author was among the recognized leaders of Soviet jurisprudence. Late in 1936, the Stalin regime did away with him as unceremoniously as was the practice under the Romanovs. Had not Lenin insisted that no state, not even the proletarian state of transition, could be either a "free state" or a "people's state"? [5]

Totalitarian ideology boasts of its dynamic character. It is long on suggestive phraseology and short on concrete commitments. Thus it has acquired an elasticity of content which may stun the opponent. In Italy, Professor Ugo Spirito has not been prevented from contending for years that Fascism, like Communism, stands essentially in juxtaposition to the capitalist system. In this noble rivalry, victory will fall

> not to that one of the two regimes which will have denied the other, but to that one of the two which will be able to incorporate and surpass the other in a more extreme form.[6]

Such "radical" agitation is not simply the result of oversight on the part of the *Duce*. It rather means, in Salvemini's words, that "at the opportune moment the right-wing Fascists disappear and the left-wing Fascists come to the front." [7] For Germany, similarly, the versatile mind of Dr. Goebbels has publicly stressed the need for "tactical" concessions in patent contradiction to National Socialist "principles." [8] As late as 1934, Professor Carl Schmitt, the notorious weather vane on the roof of the Third Reich, expounded the thesis that between the Soviet state and other political systems there could be no "international community, hence no peace, but merely a truce." [9]

[4] *International Press Correspondence*, vol. 15, no. 12, 1935, p. 344.

[5] V. I. Lenin, *State and Revolution*, New York, 1932, p. 18.

[6] "Individuo e stato nella concezione corporativa," *Atti del secondo convengo di sindicali e corporativi*, Rome, 1932, vol. 1, p. 191. Cf. also H. Arthur Steiner, *Government in Fascist Italy*, New York and London, 1938, pp. 90 ff.

[7] G. Salvemini, *Under the Axe of Fascism*, New York, 1936, p. 372.

[8] See F. Morstein Marx, *Government in the Third Reich*, 2d ed., New York and London, 1937, p. 14.

[9] *Nationalsozialismus und Völkerrecht*, Berlin, 1934, p. 17.

Early in 1939, the same author presented the Hitler regime with an equally polished argument for the international "allocation of global space," buttressed by a Monroe Doctrine addressed to "extraneous" powers—an argument that conceded Russia the position of a "global" neighbor of Germany's *Mitteleuropa.*[10]

Mussolini has found the neatest expression for the flexibility of totalitarian ideology by defining it as a "creed of action." He has in fact gone so far as to state bluntly that the Fascist movement had to start from ideological scratch, that as late as 1919 he himself had not yet evolved any "specific doctrinal attitude."[11] National Socialists too, as Dr. Goebbels has put it succinctly, "acted first and then philosophized." No less illuminating are the rationalizations of free choice offered by Communist theoreticians. Lenin had outlined their position long before he emerged from underground to take charge of the Bolshevist Revolution. In 1907 the loyal disciple had ceased to believe in the infallibility of his masters: "Yes, Marx and Engels erred much and frequently in their estimate of the imminence of revolution."[12] If they erred here, could they not have erred elsewhere? In a similar vein, Lenin denounced Kautsky as acting "like the religious debater in the village: he advances 'quotations' from Marx himself."[13] Perhaps Lenin's historic significance lies in the fact that he never tired of emphasizing the decisive role of tactical mobility. In 1902 he ridiculed the Economists as men who "degrade Social Democracy to the level of trade unionism" by saying that "a plan of tactics contradicts the fundamental spirit of Marxism." This, he urged, "not only means theoretically to vulgarize Marxism, but also practically to *drag the party backward.*"[14] From here it is indeed but one step to Paschukanis' arresting conclusion: "Each ideology dies with the social conditions which have produced it."[15] From here it is also but one step to Molotov's defense of socialist competition:

[10] *Völkerrechtliche Grossraumordnung mit Interventionsverbot für raumfremde Mächte*, Berlin, 1939.

[11] Benito Mussolini, *The Political and Social Doctrine of Fascism*, International Conciliation, no. 306, 1935, p. 5.

[12] V. I. Lenin, *Marx, Engels, Marxism*, New York, 1935, p. 112.

[13] *State and Revolution*, p. 95.

[14] V. I. Lenin, *What Is To Be Done?*, New York, 1929, p. 49.

[15] E. Paschukanis, *Allgemeine Rechtslehre und Marxismus*, Vienna and Berlin, 1929, p. 37.

Only opportunist chatter-boxes can substitute for our policy a petty bourgeois policy of wage leveling, disregarding labor productivity and disregarding a worker's qualifications.

An attitude of lofty disdain to the organization of wages is, in fact, the product of these very petty bourgeois equalitarian survivals. Conscientious workers have a different attitude to this. We must insure that every worker understands that for higher productivity of labor, for work on a more responsible sector, for better qualifications, there must be a higher wage, and then our industry will grow still more rapidly, there will be an abundance of goods, and the whole working class will gain.[16]

Nor should it be surprising to encounter sophistries as superb as Radek's editorial comment on an earlier trade deal between the Third Reich and Soviet Russia. "Comrade Molotov," he declared,

was absolutely right in regarding (the credit) proposal, which the Soviet Government intends to consider in a business-like way, as proof that the situation in fascist Germany is contradictory, as proof that her policy is contradictory."[17]

Needless to say the other party returned the compliment in virtually the same words.

The secular religion of the Ecclesia of Leviathan, though aiming in general terms at a "better life," is hence at the same time, both in form and in substance, at the disposal of the political control group. Even a small body of men, however, especially one that is subject to change in its composition, is unable to assume the character of a sacral symbol. Totalitarian ideology, as the myth of unity, demands the single Leader, the *unitas realis* that alone is capable of representing all in one because it is in itself the perfect representation of the "national spirit." Writes a German ideologue:

The Leader is permeated by the idea; it acts through him. But it is he also who can give this idea the vital form. In him, the national spirit realizes itself and the national will assumes its direction. In him, the people gains visible shape, the people that spans all generations and is therefore never concretely assembled in its totality. He is the representative of the people.

Alien to original Communist doctrine, the leadership concept as an indispensable part of totalitarian ideology has in recent years

[16] *Economic Survey*, published by the USSR Chamber of Commerce, no. 2, 1935, p. 45.

[17] *Moscow Daily News*, January 15, 1935.

made rapid headway in Soviet Russia. At the opening of the Moscow Subway, Kaganovich exclaimed: "Comrade Stalin is a very busy man: he directs the whole country."[18] Voroshilov, master of the Red Army, has expressed the same confidence: "Once Comrade Stalin has taken up the question of transport in real earnest, then, comrades, it will not be a joking matter."[19] Few, however, have registered the change of style more lyrically than State Prosecutor Vyshinsky, now no longer *persona grata:*

> Our great fatherland is joyously flourishing and growing. The fields of innumerable collective farms are rich with a golden harvest. Thousands of new socialist Stakhanov factories and works are pulsating with life. Harmoniously and wonderfully our railways are working for the welfare of our fatherland, and from end to end of the country Krivonoss passenger and freight trains are speeding over the glistening ribbons of steel. Firm as granite stands our Red Army, surrounded with the love of the people, guarding the frontiers of our native land. . . . With great and unsurpassed love, the toilers of the whole world utter the name of the great teacher and leader of the peoples of the USSR—Joseph Vissarionovich Stalin!"[20]

With the Leader elevated to the sacral symbol of national community and national strength, totalitarian ideology capitalizes on the emotional uncertainties of "mass man," man without religion, man afloat in a society that dramatizes human interdependence but fails to articulate the We. The appeal is simply this: "Give me faith, and I will give you in return an eternal goal, the dignity of partisanship in a great undertaking, and the assurance of a higher order." Jubilated a German youth (1933) in a poem celebrating his ascension to the fraternal communality of the Brownshirts:

> Now I know well
> I am no more alone.

On the other hand, the continuity of national advance is systematically underscored. Molotov struck a familiar note when intoning before the Seventh Congress of Soviets:

> Our Party is moving forward with the unfurled banner of Marxism-Leninism, inspiring the fighters for Communism, *our Stalin* is leading the

18 *International Press Correspondence*, vol. 15, no. 24, 1935, p. 646.

19 *Socialism Victorious*, New York, 1934, p. 264.

20 Speech for the prosecution, in the *Case of the Trotskyite-Zinovievite Terrorist Centre*, Moscow, 1936, pp. 120–121.

million-strong masses, and we firmly know that this is the road to our complete victory.[21]

And again, Voroshilov:

The Bolsheviks are called Bolsheviks just because they are never satisfied with what has been achieved but always demand something more and better.[22]

Meanwhile, the march of the cause can be made more conspicuous by occasional glimpses of the backward world outside. Reports Kerzhentsev in his *Bolshevism for Beginners:*

The worker in bourgeois countries is a veritable slave, dragging out a wretched existence, doomed to undernourishment and to life in miserable hovels without any chance of enjoying the benefits of culture in his spare time.[23]

Naturally Kerzhentsev does not stand alone. In another very respectable text the eager Communist reader learns with horror:

The Chinese coolie, feeding himself on a handful of rice, often sleeping in the streets or in parks and clothing himself in rags, is, in the eyes of the capitalists, the most exemplary worker in the world. The more brazen capitalists tell the European workers to take an example from the Chinese coolie, to live as "economically" as he does. This kind of advice has been heard particularly often during recent times.[24]

Under the circumstances, it seems to require a considerable measure of optimism regarding the intellectual promise of Communist civilization to accept the distinction between "charismatic dictatorships" working upon "emotions that are bound up with the particular feelings and modes of thought and the histories and memories of a nation" and "rational dictatorships" that appeal "to reason to master by study and logical understanding the laws underlying the process of history." [25] In the first place, Soviet Russia has veered too far toward an inspirational appeal to qualify as a soberly rational system. Secondly, no political order in the era of "mass man" can afford to be entirely unreceptive to "charismatic" influences. And finally, a *politeia* dedicated to reason alone would hardly endure except as a learned society.

[21] *Economic Survey*, cited in note 16, p. 49.

[22] *Socialism Victorious*, p. 273.

[23] Moscow, 1931, p. 86.

[24] A. Leontiev, *Political Economy*, New York, 1934, p. 119.

[25] Hans Kohn, *Revolutions and Dictatorships*, Cambridge, 1939, pp. 192–193.

II

The Kemalist Constitution introduces the Turkish state as both "etatist" and "revolutionary." The combination of attributes suggests that Leviathan has even devoured the Revolution. Totalitarian politics underlines this fact. The right to violence is claimed by the *cleroi* of the temporal Ecclesia: the organized party membership. It is direct action breaking loose at the hint of hierarchical superiors identified with the political control group. The conspirational nuclei of bygone days have become part of a vast and all-embracing machinery. Their function, however, is not much different from that stressed during the "revolutionary phase" of the contemporary one-party systems.

Credit for having designed a working theory of revolutionary organization must go to Lenin. In brilliance and lucid insight his contribution far excels the adaptations of Mussolini and Hitler. The formula evolved by Lenin was simple. It accentuated the need for planning, expertship and control. For all three, a truly missionary zeal and a clear conception of the mission itself were prerequisite. He had but scorn for the Economists who opposed the formula as an "invention of the doctrinaires, an exaggeration of ideology." [26] He never ceased to ridicule the dreamers who placed their faith in spontaneous proletarian action. As he put it in 1902:

> Yes, our movement is indeed in its infancy, and in order that it may grow up the quicker, it must become infected with intolerance against all those who retard its growth by subservience to spontaneity.[27]
>
> There can be no talk of an independent ideology being developed by the masses of the workers in the process of their movement.[28]

In line with what he called his "habit to reply to attacks, not by defense, but by counter-attacks," [29] Lenin insisted that "the Social Democrat's ideal should not be a trade-union secretary, but a *tribune of the people*." [30] As to practical considerations, he was equally explicit. Looking back upon his previous amateurishness with a "burning sense of shame," [31] he laid down these precepts:

26 *Op. cit.* in note 14, p. 24.
27 *Ibid.*, p. 42.
28 *Ibid.*, pp. 40–41.
29 *Ibid.*, p. 86, note.
30 *Ibid.*, p. 77.
31 *Ibid.*, pp. 118–119.

The struggle against the political police requires special qualities; it can be conducted only by *professional* revolutionists.[32]

A small, compact core, consisting of reliable, experienced and hardened workers, with responsible agents in the principal districts and connected by all the rules of strict secrecy with the organizations of revolutionists, can, with the wide support of the masses and without an elaborate set of rules, perform *all* the functions of a trade-union organization, and perform them, moreover, in a manner Social Democrats desire. Only in this way can we secure the *consolidation* and development of a *Social Democratic* trade-union movement, in spite of the gendarmes.[33]

Only a centralized, militant organization, that consistently carries out a Social Democratic policy, that satisfies, so to speak, all revolutionary instincts and strivings, can safeguard the movement against making thoughtless attacks and prepare it for attacks that hold out the promise of success.[34]

The only serious organizational principle the active workers of our movement can accept is this: Strict secrecy, strict selection of members, and the training of professional revolutionists.[35]

In proposing this type of organization, Lenin sought to overcome the "primitiveness" that had "lowered the prestige of revolutionists in Russia." [36] As his precepts began to take hold, elected committees disappeared. The new mechanism, in the words of the (now discredited) party historian, "was constructed centralistically and from above downward." [37] Lenin, ruthless with opponents,[38] so firmly impressed upon his following the need for unconditional subordination to central decision as to leave an indelible mark upon the Bolshevist Party. Today as before, the freedom of the political control group is consistently emphasized. "Party tactics," we read, "must be elastic, taking into consideration all the special features of the moment and the new problems advanced by life." [39] In the same breath we are informed that "the strictest party discipline is the most important duty of the party members." [40] The basic principle stands out in sharp contrast to the rosy clouds of official verbiage. Thus Article 126 of the Soviet Constitution

[32] *Ibid.*, p. 103.
[33] *Ibid.*, p. 112.
[34] *Ibid.*, p. 128.
[35] *Ibid.*, p. 131.
[36] *Ibid.*, p. 118.
[37] E. Jaroslawski, *Aus der Geschichte der Kommunistischen Partei der Sowietunion*, part I, Moscow, 1931, p. 167.
[38] *Ibid.*, part II, Hamburg and Berlin, 1931, p. 231, note 9.
[39] Kerzhentsev, *Bolshevism for Beginners* (see note 23), p. 22.
[40] *Ibid.*, p. 25.

of 1936 presents the Bolshevist Party as an "organic association" rather than as a medium of group discipline:

> The most active and politically conscious citizens in the ranks of the working class and other strata of the toilers unite in the Communist Party of the USSR, which is the vanguard of the toilers in their struggle to strengthen the socialist system and which represents the leading core of all organizations both public and state.

Let us keep in mind, however, that the Russian Constitution looks both ways, toward a domestic public and toward a potential audience abroad. Confided an inspired editorial in 1935:

> The reform of the constitution of the Soviet Union . . . must have the greatest effect on the advance-guard of the workers, on the Communist Parties. It provides them with a powerful means of agitation. Rightly employed, it must strike fascism at heart.[41]

The heritage of revolutionary form, with its overweighting of the center of command, has continued to predispose the Bolshevist Party toward a contempt for "formal majorities." This contempt is not absent from Lenin's writings. On the eve of the October Revolution, he successfully spurred his organization to bold minority action by arguing: "It would be naive to wait for a 'formal' majority on the side of the Bolsheviks; no revolution ever waits for *this*."[42] In the same vein he persuaded his supporters to discard all doubt in the ultimate victory:

> After the 1905 Revolution, Russia was ruled by 130,000 landowners. They ruled by means of constant force over 150,000,000 people, by pouring unlimited scorn at them, by subjecting the vast majority to hard labor and semi-starvation. And yet we are told that Russia will not be able to be governed by 240,000 members of the Bolshevik Party—governing in the interest of the poor and against the rich.[43]
>
> The triumph of the Russian Revolution, as well as of the world revolution, depends on two or three days' struggle.[44] We are *sure of victory* because the people are on the verge of despair.[45]

Recognizing the psychological moment, Lenin drove relentlessly toward a decision that was essentially his own, a decision not

41 *International Press Correspondence*, vol. 15, no. 7, 1935, p. 190.
42 V. I. Lenin, *Will the Bolsheviks Retain State Power?*, New York, 1932, p. 4.
43 *Ibid.*, p. 25.
44 *Op. cit.* in note 12, p. 170.
45 *Ibid.*, p. 164.

to be affected by the "vulgar bourgeois" distinction between dictatorship and democracy.[46]

Even at this hour, however, his horror of amateur methods dominated his mind. Revolutionary overthrow would require *expertise.* It could not be left to the play of accident. In his instructions for the Communist rising he wrote:

> And in order to treat the insurrection in the Marxist way, in other words as an art, we must at the same time, without losing a minute, organize a *staff* for the insurrectionary detachments, distribute our forces, move the trustworthy regiments to the most important points, invest the Alexandra Theatre, occupy the Peter and Paul Fortress, arrest the general staff and the government, send against the Cadets and the "savage division" such *detachments* as are ready to sacrifice themselves rather than allow the enemy to penetrate into the centres of the city; we must mobilize the armed workers, summon them to the last desperate fight, occupy from the outset the central telegraph office and telephone exchange, instal *our* insurrectionary staff at the central telephone exchange, set up telephone connections with all factories, all regiments, all the points of armed conflict, etc.
>
> All this, indeed, is by way of an example, only to serve as an *illustration* that at the present moment one cannot possibly remain faithful to Marxism, faithful to the revolution, *unless one treats insurrection as an art.*[47]

Precisely the same insistence upon *expertise* is typical of party education in present-day Russia. Says Kaganovich: "It is high time to get rid of the amateur methods and habits and the limited outlook inherited from the pre-revolutionary city governments." [48] Or Kerzhentsev: "So long as we do not form bodies of our own proletarian experts, there will be breaches in the socialist front." [49] Or again Kaganovich:

> The Party demands a concrete approach, a definite aim in theoretical work. In combating the separatism of practice from theory, which is a struggle on two fronts—against commercialization and narrow practicism on the one hand, and against scholasticism and formalism on the other—our Party demands that the cadres of theoreticians make a profound study of Marxist-Leninist theory and tactics, to maintain the closest connection between practice and theory, to combine theoretical study with all the experience and tasks of socialist construction.[50]

[46] *Ibid.*, p. 129.

[47] *Ibid.*, pp. 166–167.

[48] L. M. Kaganovich, *The Socialist Reconstruction of Moscow and Other Cities in the USSR,* Moscow, 1931, p. 75.

[49] *Op. cit.* in note 39, p. 76.

[50] J. Stalin, L. M. Kaganovich and P. Postyshev, *Questions Concerning the History of Bolshevism,* Moscow, 1932, p. 18.

The call for *expertise* addresses itself to all the different sectors of party activity, not the least to agitation and propaganda. Long before Hitler formulated his well-known propaganda strategy in the quiet of a German fortress cell, Plekhanov, one of Lenin's early companions, had enunciated the basic principle by saying: "An agitator presents only one or a few ideas, but he presents them to a mass of people." Lenin's comment is equally significant: "The agitator operates with the *living* world."[51] What he meant he made eminently plain shortly after the October Revolution. Faced with the issue of political indoctrination of the peasantry, he pointed out that the peasants could not be expected to "emancipate" themselves "solely along the straight line of purely Marxist education."[52] To destroy the hold of the church, he offered this advice:

> The clever, vivacious and talented journalism of the old atheists of the eighteenth century, which wittily and openly attacks the dominating clergy, will very often prove to be a thousand times more suitable to rouse the people from religious torpor than the dull and dry repetitions of Marxism with an almost complete lack of skilfully selected facts to serve as an illustration, which predominates in our literature and which (it is no use hiding our sins) frequently distorts Marxism.[53]

Religious faith, as a counterideology, can not be uprooted by mere display of force. It must be undermined slowly and systematically. Rash action is undesirable. Hence members of the Bolshevist Party are exhorted to impose upon their zest intelligent reserve. As a Moscow manual explains:

> When dealing with peasants or workers recently from the village the greatest caution must be exercised. It must be borne in mind that the best means for struggling against religion is the spreading of education. A believer whose religious feelings are affronted will only become still more religious. Thus the forcible closing of a church against the will of the population will merely evoke a desperate, passionate struggle and confirm the dupes of the priest in their faith.[54]

The suggestion is in harmony with the instructions of the Central Committee of the All-Union Communist Party laid down in the circular on the "Struggle with Distortions of the Party

51 *Op. cit.* in note 14, p. 65.
52 *Op. cit.* in note 12, p. 215.
53 *Ibid.*, pp. 215–216.
54 Kerzhentsev, *op. cit.* in note 39, p. 78.

Policy in the Collective Farm Movement,'' [55] which contains the following clauses:

(7) Decisively to stop the practice of closing churches administratively, with a fictitious justification of a voluntary desire on the part of the population. To permit the closing of churches only in the event of a real desire of the overwhelming majority of the peasants, and only with the confirmation of the decisions of the peasant meetings by territorial executive committees. To hold guilty persons to strictest responsibility for mocking the religious feelings of peasant men and women.

(8) To remove from their posts and replace with others those Party workers who are not able or do not desire to carry out a decisive struggle with distortions of the Party policy.

From these illustrations it should be clear that it is essential for the effectiveness of the temporal *cleroi* to be closely integrated into a centrally directed hierarchical order. Here, as elsewhere, the prototype afforded by Lenin's thought has been relied upon by all contemporary one-party systems. In the words of the Founding Father, Bolshevists are ''centralists by conviction and by the program and tactics of their whole party.'' [56] Indeed, he went so far as to assert that ''Federalism arises, as a principle, from the petty-bourgeois views of Anarchism. Marx is a centralist.'' [57] This statement is particularly challenging if held against the federalist aspirations of the USSR. An object lesson is supplied by the nationality policy of the Soviets, Comrade Stalin's specialty, as we may remember. Even a protagonist as enthusiastic as Perchik concedes at the start: ''The national question is subordinated to the basic idea of Bolshevik policy, the struggle for Communism.'' [58] Under the circumstances, it can hardly be otherwise, but it is equally patent that the formula aims at the eradication of nationality cohesion. The new culture toward which Russia's nationalities are to turn is ''national in form and proletarian in content,'' as the official version of 1930 has it. Can we doubt that, in this matter, content is decisive?

However, since the core of the party organism still rests in the Great Russian element, ''scandalous examples of Great Russian chauvinism'' [59] have often interfered with what is called the

[55] *Izvestia*, March 15, 1930.

[56] *Op. cit.* in note 42, p. 29.

[57] *Op. cit.* in note 5, p. 46.

[58] L. Perchik, *How the Soviet Government Solves the National Question*, Moscow, 1932, p. 18.

[59] *Ibid.*, p. 56.

nativization of the Soviet apparatus. In many offices, we are told, members of other nationalities are met with the greeting: "If you don't know Russian, there is no need for you to come around here."[60] In the Mordavian autonomous region, for instance, when the workers tried to present their report on the progress of the harvest in their native tongue, the District Collective Farm Union invariably answered: "We do not understand German (!)."[61] Granted that the causes for such friction can perhaps ultimately be checked, the evidence suggests strongly the centripetal tendencies of the Soviet system. Moreover, while the USSR has unquestionably been more successful than tsarist suppression in meeting the nationality problem, it has done so by liquidating the substance of nationality. From the Bolshevist point of view, of course, the picture is different. Says Stalin: "He who has not understood this peculiarity and 'contradiction' of our transitional period, he who has not understood the dialectics of historical processes, is lost to Marxism."[62]

III

In its quest of the "new man with a new psychology," totalitarianism finds itself confronted with a task of gigantic magnitude, a task that taxes its entire strength, a task that has forced the one-party systems to retain the combat formation developed in order to meet the earlier needs of revolutionary strategy. While planting its mines underneath the Old Regime, the party had to adjust itself to the necessity of being continually under alarm. It operated in an atmosphere and psychology of suspense and emergency. When the Old Regime collapsed, the revolutionary momentum soon carried the party organization to the point where its very program collided headlong with the social *status quo*. Any single step in the execution of the program involved further steps so as to make secure the ground gained.

Placed under the terrific pressure of Old versus New, the party had no alternative to perpetuating the spirit of emergency within its own ranks. Though destroying its competitors on the political scene and establishing a monopoly of representa-

60 *Ibid.*, p. 47.
61 *Ibid.*, p. 33.
62 *Ibid.*, p. 48.

tion for itself, the party has remained *pars* in more than one sense. It has remained *pars* as a separate and distinct organization. It has remained *pars* as the *aristoi:* the "vanguard of the toilers," the "spinal cord" of the Fascist State, the "select order of leadership destined to guarantee the National Socialist State for all times to come." It has also remained *pars* as an entity set off against "the people," in terms of both hierarchical allegiance and ecclesiastic mission.

Relatively speaking, in Fascist Italy the party has come closest to being absorbed into the state, while in Soviet Russia it has come closest to absorbing the state. As to the latter, the explanation lies largely in Lenin's astute comment: "The old (state) apparatus we could neither seize nor set in motion." [63] National Socialist Germany occupies a position midway between the other two totalitarian regimes. Here the party and the state represent sharply distinguished functions, modified by personal unions effected through dual officeholding. The party is the dynamic force, determining the general direction of state activity. Since the state service, however, has not lost its traditional characteristics as a career establishment, it is more than simply a tool at the disposal of the party. The professional judgment of the bureaucracy has continued to affect the shaping of national policy in those areas where the stand of the party is not categorical. For this very reason, the state requires the constant superintendence of the party. Exclaimed Hitler face to face with his loyalists at the Party Congress of 1934: "We command the state!" Effective command cannot emerge from diversity or diffusion.

National Socialism has gone farthest in designing machinery for the conveyor-belt production of minds filled with nothing but the tenets of its faith. Training for the "career of leadership" through and within the party has been dissociated entirely from the general educational system. The detail of the scheme is instructive. At an early age, boys coming from all sections of the population are selected for the Adolf Hitler Schools, of which there are to be eventually some forty, at least one for each *Gau* as the regional unit of party organization. Attendance, including uniform and pocket money, is free. Prime requirement is perfect health.

[63] *Op. cit.* in note 42, p. 22.

There is no free entrance, however. It is the local party authorities alone that may propose candidates. Their nominations must be endorsed by the National Socialist district agencies, intermediate subdivisions of the *Gaue*. After having been admitted, the pupil stands on his own; he is not subjected to the rigmarole of educational discipline such as reports and examinations. He is, on the other hand, expected to prove himself of outstanding "character" and ability; if he fails in this respect, he is under the obligation to "withdraw." Instruction is offered on the basis of a curriculum so closely geared to party needs that it was not even considered desirable to rout the draft through the Ministry of Education, headed by an old National Socialist though it is. Instead, the curriculum was sanctioned by *Reichsleiter* Rosenberg, superideologue of the Third Reich, Baldur von Schirach, steward of the Hitler Youth, and Dr. Robert Ley, Party Organization Chief. The instruction is pointedly anti-intellectual, stressing physical prowess and absolute loyalty. It has not very much in common with education as we know it.[64]

After graduation from the Adolf Hitler Schools, the pupils must take up occupational or professional training. Their graduation certificate opens to them even the universities. In addition, as every other young German, they have to spend half a year in the Labor Service and two subsequent years under the colors. They have to put in also a required minimum of precinct duty within one of the party formations. Seven years after graduation, one fourth of the whole class of 4,000 will be called up as the most promising material for the second stage of education for leadership at the Order Castles (*Ordensburgen*). This stage takes four years, broken down into the same number of stations. The Castle of Crössinsee in Pomerania offers track, riding, and aviation. From there the aspirants shift to the Castle of Vogelsang for a still more rigid set of "courage tests." At the Castle of Sonthofen they are groomed in skiing and mountaineering during the third year. The last year at the Castle of Marienwerder is devoted to the attainment of "full spiritual and political maturity." During each period, the hardened fighters for the National Socialist millennium must

[64] For an analysis of present-day education in the Third Reich, see my contribution to the *Phi Delta Kappan*, vol. 22, 1939, pp. 101 ff.

set aside three months for party service so as to provide themselves with concrete experience in the workings of the organization.

While at the Order Castles, the aspirants are not intended to be exclusively among themselves. Near the location of the four castles, the recreational division of the party, known as *Strength Through Joy,* plans to construct party-owned and party-managed hotels, the privilege of staying to be limited to seven days. The underlying idea as officially enunciated is to supply the rising generation of political leaders with "rich contacts with the people." But it is not "the people" that will frequent these hotels. The opportunity will be available rather to the minority strong in faith, men and women of unquestionable standing with the powers to be. "The masses" will be far away. They will be farther away still from the third stage of the selection process: the closing term at the High School of the Party on the shores of Lake Chiem. Thereafter, Hitler's brawny giants, with mentalities hermetically sealed, will be ready to officiate in the temples of the Teuton denomination. They will know their catechism and nothing else. They will worship the Leader and exorcize the devil of dissent.

In order to do so, however, the templars have to exert themselves beyond their congregation. They must spread out and seek the "people's soul." The "leadership of men," to use a National Socialist phrase, entails the brisk pursuit of the following. The forms it takes are clearly delineated in a recent German editorial:[65]

> The more deeply and intensively the leadership machinery of the Party is to reach out into the people, the more manifold must it become. Those organizations created as fighting units by the National Socialist Workers Party in its "period of belligerency" have long since been placed in the service of political leadership; proportionally enlarged and augmented, they have been differentiated in their functions. The combat units have received special tasks. The National Socialist Labor Cell Organization has become the comprehensive German Labor Front. The women are combined in their separate organizations. The civil servants, the teachers, the judicial personnel are united in National Socialist formations. Where is the German who is not somewhere included in this multiform entity? Meanwhile it has grown far beyond its original scope. More and more new organizations of different character such as the Red Cross, the Union of Former Soldiers (embracing all Germans having

[65] *Frankfurter Zeitung,* July 9, 1939.

served their term in the army), or the gigantic Association of Turners and Athletes have become, over and above a formal *Gleichschaltung,* in their entire structure additional means of the National Socialist leadership of men. The functions of the Party have thereby increased incredibly—and with it the functions of its members. To be a Party comrade means to take upon oneself greater duties; the distinction of membership necessarily assumes the ever more concrete sense of readiness for service. Not only must the Party and its formations exist and operate, but also German life in its entirety and in all its manifestations must be guided from this center. Whoever in Germany occupies a position of leadership will constantly be subjected to the organization, the training, and the disciplinary power of the National Socialist Party. Conversely, he who is a Party comrade will somewhere discharge a function in the name of the Party. This is the meaning of Adolf Hitler's demand: all Germans National Socialists, the best National Socialists into the Party!

This vast assignment affects each German in many different ways. The National Socialist Party itself, through its regional organizations, its districts, its tens of thousands of local groups with their cells and blocks, extends into each individual home. The citizen is thus a recipient of the care of the Party at his working place as much as during his recreational hours and while devoting himself to his personal hobbies. In no age is he dismissed from it: organizations directly or indirectly subordinated to the National Socialist Party accompany him from his youth through his prime in the army and his profession to the quiet evening of his life. Wherever he stands and exercises a function, he is accessible to (the Party's) political leadership, in great matters as in small. Whether the Leader himself indicates the major lines of advance in the arts and crafts or whether the Women's Organization offers individual advice to those responsible for the progress of home economics; whether the Labor Front sends its speakers to plant meetings or whether the Storm Troop Detachment calls our ex-service men into its units: each example is merely an application of the all-inclusive principle of leadership of men, which tolerates individualization only when and where it serves, as in the case of research, the community at large, even here, however, without permitting individual attainment to fall out of the general frame. The supreme leadership does not turn to the nation merely under particular circumstances, for principal decisions or under critical conditions; it is eager to maintain a steady contact. This is the sense of the historically unprecedented, gapless organization of our people. Naturally, there ensue occasionally inconveniences for the individual; after so few years, the organization is certainly not yet perfect. Still, it does not originate from a bureaucratic passion for organizing, but in the last analysis restores that bond among all which is self-evident in the tribal mode of existence, although it is all too easily destroyed under modern conditions of mass living. The new leadership of men aims at transforming the shapeless mass into the organized national community, a powerful instrument not conceived as passive, an instrument which in turn sustains leadership by subordination.

Let us note that the "organized national community" is here presented not simply as the main objective of National Socialist ambition, as the "good life for good men," but simultaneously as an "instrument" of leadership. A psychologically revealing slip of the tongue! For it does not require particular acumen to realize that an instrument is fashioned for use, that it is wielded for a purpose. He alone who wields the instrument is master over the purpose. And as the tool is said to rejoice at being wielded by the competent hands of the true craftsman, so the "organized national community," it appears, celebrates its perfection in the act of subordination. It is clear now that the "national community" must indeed achieve the form of a "gapless organization"; it is clear also that the eventual outcome cannot be but "historically unprecedented." Throughout the ages, man has pursued his search for cooperation without coercion. Totalitarianism formally abandons this search as beyond the point. "Gapless organization!" The most dexterous practitioners of the police state would pale with envy, could they lay their eyes on certain parts of our world.

The tight network of totalitarian party organization not only spans across the realm of politics, but also firmly envelops the social and economic spheres. This in fact is an "organic" necessity. Tremendous pressure for national self-sufficiency is the inevitable concomitant of belligerent creeds. Production, distribution, and even the general structure of consumption are welded into one gigantic system. To keep millions of men and women in line and mindful of their responsibilities in sustaining this system, legions of party functionaries devote themselves to policing individual and group conduct. The neomercantilism of the totalitarian state by far surpasses in scope its classical prototype. A most intriguing page from the annals of National Socialist statecraft is that recording the high-pressure organization of the "forgotten trade"—junk and rags.[66] Under the Four Year Plan, the "war on waste" has become a campaign of national proportions. Junk dealers, in general among the least organized of all entrepreneurs, have been elevated to the plane of a model industry. Governmentally sponsored organization has meant for them many things of stark novelty, above

[66] Two of the best case studies of particular areas of the totalitarian economy are Karl Brandt, *The German Fat Plan and its Economic Setting*, Stanford, 1938, and Henry S. Miller, *Price Control in Fascist Italy*, New York, 1939.

all the imposition of far-reaching trade obligations. Instead of leaving them to the inspiration of the profit motive, they have been charged with the express duty of regular collection of the available waste material, irrespective of their own preferences. They must keep detailed inventory books in order to provide the authorities with opportunities for inspection.

The organized junk pickers deliver their goods to organized middle men or directly to the organized wholesale traders. The latter, responding to the encouragement of a provident government, have specialized themselves for one or the other of the main branches—rags, paper, bones, rubber, glass, metal, and celluloid. All of these activities, however, were not fully grouped in a perfect order until the overdue coordinator appeared. In April, 1938, a National Commissioner for Waste Materials was appointed. Under his impetus, the existing enterprises were forged into one single economic sector. Volunteer activities were authorized for the purpose of filling gaps in the nation-wide collection scheme. Commercial pickers were subjected to a rigorous police control. They were combined in official collection districts; the individual picker must call upon each household in his beat once a month. He is forced to take along all waste materials, regardless of their value. Encroachments upon areas outside his beat are prohibited. The practical effect was to create marketing zones; here, at last, came the reward, for the establishment of marketing zones tended to stabilize the individual enterprises.

The path of progress, however, was not free from thorns. Local differentiations in the trade structure had to be ironed out; Leipzig, for instance, had 1,200 junkmen, while in Königsberg there were less than a dozen. Further difficulties arose from the elimination of all Jewish dealers just at the time when the demands on the trade were greatly increasing. These dislocations were followed by the weeding out of skilled personnel and their transfer to other occupations, as part of the government policy of allocating all available labor according to the needs of the national economy at large. But organization emerged triumphant. School children, in cooperation with local governments, were assigned to the task of collecting each week from door to door the bones that formerly had gone into the refuse pail; between collections, the bones are to be stored neatly by

all German housewives. The Hitler Youth was drafted for the purpose of gathering in the same manner less odorous household wastes. In due course of time the National Commissioner had reorganized the entire garbage collection throughout the nation; since then, even derelict tin cans are routed back into the process of production. The sharp throttling of imports and the accelerated pace of industry constantly raised the demand for domestic waste products. As a consequence, the authorities had to fix maximum prices for virtually every one of them.

Government control of inventories was implemented by regulations concerning the distribution and the sorting of waste products. Step by step the junk trade was transformed into a full-fledged agency of economic policy. The different branches of the trade assumed forms closely resembling syndicates. To open a new enterprise requires government permission. Traders are no longer entitled to time their sales at their own discretion; nor is it left to them to select their customers. Sorting categories have been standardized by regulation. Trade practices, codified with care, have become uniform all over the country. Success, or rather what National Socialists view as such, has not been lacking for this vast undertaking. Midway in 1939, the National Commissioner for Waste Materials took pride in announcing that no less than 22 per cent of the domestic supply of textile raw materials was derived from the collection of rags. Rags, however, is the Commissioner's prime exhibit, apart from the equally orthodox line of scrap metal. Not all is well with the other lines. In the first place, with the entire nation turned to junk collecting, the avenues of commercial distribution were soon blocked by innumerable supply jams. The jams had hardly been broken, when Germany's versatile planners were stunned by the discovery that the economy was simply not geared to reabsorbing all waste products. Moreover, to force these on the market in large quantities caused disruptions in the trade with domestic raw materials occupying a competitive position. Hence the junk piles have mounted without any immediate prospect of complete utilization.

Bones, for instance, although now used for no less than 80 different products, find their way into industry as yet only to a limited extent, in 1939 slightly above one third of the total

supply. This is particularly striking in view of the fact that as early as August, 1937, extensive measures were adopted to secure a systematic utilization of bones. The use of bones for fuel was prohibited. Commercial enterprises such as stockyards, restaurants, and junk businesses were placed under obligation to store their supplies and to keep them available for sale. Bones were not to be used industrially unless stripped of all fat substance down to a margin of one per cent. Only a limited quota could be utilized for fodder and fertilizer prior to the extraction of the glue content. In the market, however, the new products had no easy going. They clashed with mineral fats, artificial glue, and fodder and fertilizer derived from fishbone. Far-reaching marketing regulations became imperative. To increase efficiency, the existing 3,000 bone mills, mostly of very limited capacity, had to be reduced to about 500. The same method was applied to the bone industry. The latter is at present virtually concentrated in no more than 10 glue factories and some 20 extraction works. It was a rough process, and the final solution of the many problems involved is still distant.

Needless to say that all these obstacles have merely intensified the fervor of the supervisory agencies. Elaborate reallocation schedules for all available raw materials are in progress. Plants are being designed for economical junk processing. Economical? What is economical in a "closed economy" such as that of the Third Reich? If we were to calculate costs, we would have to include the outlay for an extensive supervisory machinery; the exasperations involved in revamping a trade composed almost exclusively of smallest units formerly operating on a highly individualistic basis; disproportionately large transportation costs; the perfectly natural human resistance toward a discipline that means constant bother with little things in themselves most unappealing if not repulsive; the exertion of thousands of minor National Socialist operatives who must keep their eyes on kitchens and attics. We can well imagine how pleased the German housewife must be by the attention she is bound to receive from the swastika-adorned *Blockwart* dropping in to see whether the exemplary life is faithfully practiced.

IV

Among the first war measures of the Third Reich was the following decree issued by Minister Rudolf Hess, Substitute for the Leader in Party Affairs:[67]

> In a great number of requests reaching my office, Party comrades ask me to release them from their present function so that they may join the armed forces in the service of their people.
>
> I must refuse to grant any of these requests. Although the desire expressed in them is easily understood, I must insist that individual desire is not decisive but only the common interest.
>
> This demands that important Party posts must remain occupied by the best qualified personalities.
>
> Every Party comrade fulfills his duty at the place assigned to him until a new decree determines for him a new assignment.

The decree is significant from more than one point of view. In the first place, it demonstrates the fact that even at a moment when the fate of the polity itself is at stake the temporal *cleroi* cannot identify themselves with the common man to the point of disbanding their hierarchical order and taking a place in the actual firing line. On the contrary, with exigencies mounting, they must draw together more closely and specialize more ardently in their missionary work: to keep up the spirit of abandon and sacrifice among "the masses," to ferret out the manifestations of active and passive resistance, and to make the wheels of preparedness hum a higher tone of speed. There are other concerns too. Hitler's guard detachments, the black-uniformed SS (*Schutzstaffel*), have been known even in previous years to orient their combat tactics in unpublicized maneuvres toward potential domestic disturbances. The increased strain on totalitarian regimes that results inevitably from warfare, the complex task of maintaining the people's morale in the face of disaster, the necessity of feeding the fires of faith so that they rise visibly toward the skies: all these factors translate themselves into an intensification of party work.

At the same time, however, the party functionaries are confronted with a widening gulf that separates them from the concrete experience and the sufferings imposed upon the proverbial man in the street. The decree also aims in this direction. It poses a justification that may look thin but that at least sug-

[67] *Frankfurter Zeitung*, September 3, 1939.

gests an element of sacrifice on the part of the *cleroi:* for the common good they must forego the personal satisfaction of joining their regiments, a satisfaction which is assumed in the decree, although it may appear as a laughable pretense in the eyes of those hundreds of thousands who had to part with their families on mobilization day. It is true, thus far Germany's man power has not been severely taxed, and the actual extent of army enlistment does not even remotely approximate that required during the First World War. Should similar needs arise, the effects of the Hess prescription might sharpen the dichotomy of party and people to such a degree as to necessitate a public *démarche.*

There can be no doubt that this dichotomy is real. It is so in every one-party system. It is most strikingly evident in Soviet Russia, most strikingly because here the identification of the toilers with their own "vanguard" ought to be obvious. Had the duration of the transitional dictatorship of the proletariat been shorter than it proved to be, no serious problem might have arisen. Throughout his lifetime, Lenin held out the promise of a society truly collective both in its motivation and in its forms. In his own words:

> Striving for Socialism, we are convinced that it will develop into Communism; that, side by side with this, there will vanish all need for force, for the *subjection* of one man to another, and of one part of the population to another, since people will *grow accustomed* to observing the elementary conditions of social existence *without force* and *without subjection.*[68]
>
> The state will be able to wither away completely when society has realized the rule: "From each according to his ability, to each according to his needs," *i.e.,* when people have become accustomed to observe the fundamental rules of social life, and their labor is so productive that they voluntarily work *according to their ability.*[69]

These suggestions, aside from a number of variations on the same theme, represent Lenin's version of the collectivistic myth —the term used here in the Platonic sense. They reflect the optimism of the apostle rather than the acuteness of the revolutionary theoretician. They contain nothing to indicate a definite and methodical approach to the problem of transition such as was typical of Lenin's thinking on all other questions. Per-

[68] *Op. cit.* in note 5, p. 68.
[69] *Ibid.*, pp. 79–80.

haps he considered the issue itself remote; perhaps he knew of no specific solution; or, pondering the classics, he was doubly anxious to avoid an unnecessary commitment. Conceivably, also, he had too clear a grasp of the very function of the myth to minimize its appeal by concreteness. But he was quite concrete in one respect. "Until the 'higher' phase of Communism arrives," he declared, "the Socialists demand the *strictest* control, *by society and by the state,* of the quantity of labor and the quantity of consumption." [70]

Up to now, control has centered in the Bolshevist organization. Since this control stands out as a *fait accompli* for every Soviet citizen, it is hardly surprising, particularly after the sweeping party purges beginning 1933, that the Stalinist cadres and the body of the people, the latter identified as one class, maintain at best a formal union. Theoretically, the union should be more than that, as is plain from authoritative comment:

> The party is the head and brain of the class. It is therefore wrong to try to counter class by party (*sic*). The party is a part of the class, and its more organized and disciplined part. Wherein lies the strength of the party? In that it has a clear and exact program of all its demands. In that it sees clearly the ultimate interests of its class and knows how to place them above the interests of the moment. In that it correctly lays down the ways in which the struggle must be waged. In that it is firmly consolidated and is based upon iron discipline. In that it is closely bound up with the masses. The party must thoroughly understand the needs and interests of the masses. If it becomes isolated from them it cannot reflect their interests. Such isolation is fraught with danger for the party. The party will be useless if it merely hangs in the rear of the class. It must lead the class.[71]

Consciousness of the danger of isolation is to be distinguished, however, from the necessities of organizational separateness. Thus the party member "has no right to tell non-party workers about internal party questions, so long as nothing has as yet been published about them by the party itself." [72] Organizational separateness may also invite those administrative vices which Lenin laid to the "general conditions of our truly Russian (even though it be Soviet) bureaucracy." [73] How serious such aberrations may become can be gleaned from Krylenko's exhortation:

[70] *Ibid.*, p. 80.
[71] Kerzhentsev, *op. cit.* in note 39, pp. 18–19.
[72] *Ibid.*, pp. 27–28.
[73] *Op. cit.* in note 12, p. 215.

> The workers are . . . entitled to demand that they be treated like human beings, in keeping with their human and civil dignity; they will not tolerate and strenuously object to any act of neglect, whenever they are treated in an indifferent, bureaucratic manner, whenever they have to combat red tape.[74]

Or, in the words of the circular on the "Struggle with Distortions of the Party Policy in the Collective Farm Movement": [75]

> Cases are observed of exceptionally rough, disorderly and criminal treatment of the population by some of the minor officials, who are sometimes victims of provocation on the part of masked counter-revolutionary elements.

These and similar admissions, of course, spring primarily from the need for party discipline. The use of violence in pursuit of the established line of Soviet policy is in itself never criminal. Snorted Molotov four years ago: [76]

> Is it possible to imagine anything more shameful than the howl of certain bourgeois and socialist organs of the press, which they raise in connection with the shooting of a few dozen whiteguard terrorists?

Indeed, to a large extent the relationship between the party and the population as a whole is that of the watchdog and the herd. So it is also in Fascist Italy and National Socialist Germany. The law of the land is laid down by the party command; it is binding upon everyone. The unconditional subordination of the party membership to its hierarchical superiors finds its counterpart in the general code of civic behavior. Thus for Russia, "Socialist competition demands of each worker in the industry iron labor discipline, system in work, a lively interest in the industry as a whole." [77] Those deficient in this respect are treated as degenerates, as the *exemplum horribile:* "There are of course still any amount of careerists and grasping workers for whom the most important point is to get good pay, and who care not a rap for the welfare of the factory, or of the whole of industry." [78] The implications are elaborated most persuasively in an earlier manifesto of the Central Committee of the All-Union Communist Party: [79]

[74] N. Krylenko, *Revolutionary Law,* Moscow and Leningrad, 1933, p. 21.
[75] See note 55.
[76] *Loc. cit.* in note 16, p. 21.
[77] Kerzhentsev, *op. cit.* in note 39, p. 50.
[78] *Ibid.*, p. 80.
[79] *Rabochaya Moskva,* September 4, 1930.

> The infiltration into the Party of any kind of opportunist sentiments, undermining the fighting capacity of the proletariat in its developing offensive against the capitalistic elements, must meet a decisive Bolshevik repulse. This is the basic condition of the victorious progress of Socialism and, guaranteeing Bolshevik rates of Socialist construction, is therefore a prerequisite for the overcoming of the difficulties which stand in the way of this construction. The surmounting of the difficulties which confront us depends above all on our work, upon our ability to carry on the logical and uncompromising struggle with looseness, disorganization and bureaucratism in our organizations, and especially upon our capacity to mobilize the working class, which is a source of quickly growing and inexhaustible revolutionary energy, for the cause of the struggle for Socialism.

In the meantime, the "mobilization of the working class" requires further "tactical" concessions. For instance, "it has . . . to be borne in mind that it is difficult to raise productivity without preserving the material interest of the worker. There must be a combination of the worker's personal interests and his interest in the general raising of productivity."[80] In the meantime, too, the Bolshevist Party will have to contend with revivals of plain humanity such as that evidenced in a questionnaire filled out by a girl member of the Young Communists' League in Leningrad. This touching document, spotlighted as a clear instance of civic depravity, recorded the following information:[81]

> Q. How do you spend your leisure time at home? A. Any old way.
> Q. What literature do you read? A. None.
> Q. Do you attend any courses? A. No.
> Q. Do you attend any house meetings? A. No.
> Q. What would you like to study? A. The guitar.

V

Totalitarianism and constitutionally safeguarded individual rights are incompatible. As the temporal Ecclesia of Leviathan, it does not so much destroy liberty as it denies liberty any conceptual basis. There can be no freedom from faith. There can be no exemption from the postulates of the secular religion. There can be no immunity from the ministrations of its *cleroi*. In Mussolini's phrase, repeated and worked over many times

[80] Kerzhentsev, *op. cit.* in note 39, p. 51.

[81] *Ibid.*, p. 83. For a somewhat enthusiastic account of contemporary Russian education, see Beatrice King, *Changing Man*, London, 1936.

since it was first uttered in 1925: "Everything within the State, nothing outside the State, nothing against the State." Or, even clearer in its meaning: "For Fascism, the State is the Absolute, in relation to which all individuals and groups are relative."[82] The Fascist State, German *Volkstum* as the communality of an endless chain of generations, the "stateless and classless society" of tomorrow: each figure of thought symbolizes an ultimate and central value whose pursuit, collectively and individually, is proclaimed to be identical with the pursuit of happiness. Individual deviation is not liberty but license.

To cope with license is one of the functions of the party. Yet the dichotomy of party and people would present serious obstacles were it not for the effectiveness of specific devices by which totalitarian absolutes are constantly translated into concrete directives for individual behavior. In their entirety, these specific devices are known as the corporative order. The term bears the stamp of Mussolini's laboratory. But the organizational forms and practices which the term connotes are duplicated in National Socialist Germany and are part and parcel of Russia's proletarian dictatorship. *Corporativismo* has produced a veritable flood of literature.[83] We must confine ourselves to the essentials.

Reduced to its essentials, the corporative order represents an effort to interlock the entire occupational and social structure with the centrally controlled political hierarchy. Man the social animal becomes man the organized termite. This transfiguration is intended to yield four major results. First, every single passage in the human ant hill becomes a transmission line for political imperatives to be respected by every single ant. Second, each interest and each ensuing pressure within the polity is forced to rationalize its objectives in terms of the approved faith and thus to concede on principle the priority of the common interest as defined by the political control group. Third, all problems whose solution admits of practical alternatives, each equally admissable within the confines of ideological absolutes, can be submitted to sober debate among the spokesmen of different interest groups; in other words, here the consultative resources of the whole nation are placed at the disposal of the

[82] Benito Mussolini, *Fascism*, Rome, 1935, p. 27.

[83] A helpful compilation of the Fascist writings on the subject is to be found in Steiner, *op. cit.* in note 6, pp. 149–150.

regime. And fourth, in the organized enlistment of such consultation the corporative order acquires a modicum of representative features. Fascist Italy has gone farthest in giving constitutional expression to the quasi-representative character of her corporative institutions.[84]

The potentialities of *corporativismo* are no less manifest elsewhere. Late in June, 1939, for instance, *Reichsamtsleiter* Dr. Hupfauer of the German Labor Front, in charge of "competitive stimulation of plant performance," delivered himself of a lengthy edict concerning the "promotion and conservation of productive energies." Its main points deserve quotation:

> From among the many opportunities for correct guidance and proper care of the toilers and their productive energies, several of the most important tasks are herewith selected and placed before the German plant communities for the third year of organized competitive stimulation.
>
> 1. Awake all talent, develop all special capacities, and utilize these capacities at the most appropriate place. No enterprise is allowed to eschew expenditures in providing able employees with additional professional training. The National Vocational Contest offers each toiler the possibility of demonstrating clearly his personal competence, and at the same time supplies management with an accurate picture of the true productive capacities of the plant personnel. A prudent plant leader and manager must pay greatest attention to the proper allocation of the productive energies at his command.
>
> 2. Avoid all unnecessary depreciation of energies and take appropriate steps toward restoring them constantly. The best means for preventing any unnecessary consumption and depreciation of energies is their allocation with an eye toward the personal fitness, intellectual and physical, of each toiler for his individual working place. In addition, energy conservation results from the measures taken to satisfy industrial safety requirements; these must be applied in all plants with a diligence exceeding the standards prescribed by law. Similarly, all those measures which we include under the recognized term *Beauty in Labor* (an organized activity devoted to shop beautification) serve the purposes of energy conservation, promotion of health, and stimulation of cooperative attitudes. None of these measures, however, will change the fact that great exertion, mental and physical, always leads to a great consumption of energies. Our efforts toward securing the greatest total economic yield will meet with success only if care is taken in providing for a restoration of the productive energies after periods of work. The enterprises combined in the National Vocational Contest must follow in this respect the excellent example set by the 30,000 plants which have

[84] Cf. H. Arthur Steiner, "Fascist Italy's New Legislative System," *American Political Science Review*, vol. 33, 1939, pp. 456 ff.

insured a healthy nutrition for their working forces partly in plant cafeterias and partly by providing facilities for warming up meals prepared at home. Plant athletics is a foremost means of physical restoration and of strengthening the will to achievement; the same is true of a real recreational furlough, which in particular must be granted workers in the lowest wage groups.

3. Think of Germany's future! The Leader demands of our generation accomplishments of unique magnitude, accomplishments which are to safeguard Germany's great future. The goal requires the mobilization of all toilers, young and old, men and women. Juvenile workers and women are to be kept away from heavy work or working places which do not accord with their psychic and physical peculiarities. Pregnant women and mothers of small children must receive special protection and intensive care. Such a constructive social attitude, however, does not produce its full effects unless all forces are utilized in order to meet the greatest need of German labor: satisfactory housing conditions. A dwelling can be called satisfactory only if it accommodates in particular the worker with a larger family. All plants must therefore take steps to provide healthy and adequate housing facilities for their workers.

These tasks concern not management alone, but the whole enterprise, plant leader and the entire working force. The contest for highest performance will result in economic and social progress only if it is supported by the spirit of true work community.

It should be obvious that the "spirit of true work community" invoked in this ukase is held out as an ideological aim. The "work community" is presented as the microcosm of the united nation, a nation bent on cooperative attainment, a nation that has risen beyond the conflict of interests and class division. The Fascist and National Socialist version of classlessness occupies a position no less central than that held by its equivalent in the Communist myth. Viewed in terms of man's economic motivation alone, this version may appear a pretense more brazen than its Soviet counterpart. Man's motivation, however, is by no means exclusively economic. He is subject to the stimulation of irrationality as much as of rationality. Totalitarian ideology, in its very character as a secular religion, makes its appeal primarily to emotional yearnings, yearnings which are often devoid of an economic basis and which as often lack any foundation in reason. More than once has Hitler articulated this phenomenon by pronouncing the Third Reich before his party cohorts "not the product of reason, but of your faith." Totalitarian ideology does not enunciate the equality of paychecks; it does dramatize the equality of status, an equality

shared by all those whose labor merges into the stream of cooperative endeavor as superintended by the political control group.

Both Fascism and National Socialism—the former more slowly, the latter more rapidly—have entrenched themselves to the extent of monopolizing the final decision over the flow and allocation of investment. With that, they have obtained an unchallengeable hold on the owner class. The Soviets, faced with a different situation, could start out at this very stage; they simply fell heir to a bankrupt economy, which they held in receivership since the days of the October Revolution. Pumping systematically the worker's savings into the emergent collective order, they have remained the masters in their house without ever encountering domestic difficulties. A run on the Soviet banks would be as inconceivable as a run on Comrade Stalin himself. Nor could it happen in Berlin or Rome, where support of the regime in power is just as categorically equated with support of the *Volkgeist* itself.

The corporative formula supplies the political control group with an unlimited opportunity for transferring part of the burden of responsibility to identifiable interests whose positive response to final decisions is indispensable for compliance. Hence all of these identifiable interests, in so far as they occupy a significant place in the social system, must be brought into the fold. By such inclusion, of course, each interest loses automatically its dynamic freedom of action. Being incorporated into the new structure, it is effectively governmentalized. Its operations acquire a public character. It can be held accountable. It is laid open to close supervision. Yet supervision does not require complete officialization of the interest group. Its sphere is not simply taken over by the state or party bureaucracy itself. As to the entrepreneur group, for instance, the issue, under Fascist auspices, is not that of public ownership and management versus private ownership and management. On the contrary, by "protecting" private property and initiative, contemporary Italy and Germany leave the entrepreneur saddled with his full managerial responsibility. In fact, his responsibility has grown considerably, for it extends today to meticulous observance of political directives. Private ownership is placed on probation, *quamdiu se bene gesserint.* The entre-

preneur owns and manages his business during "good behavior." The behavior demanded of him is fraught with political connotations.[85]

Corporativismo does not merely aim at the entrepreneur group and the owner class. It is no less concerned with labor. Perhaps nothing reveals more pointedly the dilemma of capitalism than the systematic endeavor made under the swastika and the lictor's emblem to clothe the worker with "the dignity he deserves." Ever since the Italian syndicalist movement merged with the Fascist organization, lip service has been rendered to the parity of the worker with his employer—to be sure, a parity not of income but of social ranking, and a parity of social ranking more often referred to in official oratory than realized in the operation of the economy. The same applies to the Third Reich. The days are over when Hitler was available to expound his Utopia before select gatherings of industrialists. Today, when impelled to reach out beyond the army of his loyalists, he confines his appearances to factory halls to "honor German labor." These gestures are by no means matters of free choice. The temporal Ecclesia of Leviathan can exist only if it is universal in a very concrete sense. As a result, totalitarianism is inevitably driven toward maintaining a vast array of vital social services. The nationwide recreational schemes of Italy's *Dopolavoro* and Germany's *Kraft durch Freude,* both the work of the party, exemplify this tendency. The corporative order draws the logical conclusion by according labor a partnership in the evolution of social and economic policy. It goes without saying, however, that the partnership is conditioned upon political religiosity.[86]

From the point of view of effective industrial management, the New Rome and its National Socialist counterpart have had better sailing than Soviet Russia. Italy and Germany left the managerial group technically in possession. The subsequent transformation toward a continually increasing coordination of the entire economy progressed from stage to stage without presenting a clear-cut challenge to the property system. While

85 For a glimpse of "the fun of being in business" in National Socialist Germany, see Morstein Marx, *op. cit.* in note 8, pp. 157 ff. Cf. also James K. Pollock, *The Government of Greater Germany,* New York, 1938, pp. 152 ff.; William G. Welk, *Fascist Economic Policy,* Cambridge, 1938.

86 Cf. Taylor Cole, "The Evolution of the German Labor Front," *Political Science Quarterly,* vol. 52, 1937, pp. 532 ff.

the groans of the entrepreneurs have at times become distinctly audible under the impact of mounting political impositions, the gradual character of the transition itself cushioned the effects upon private management. Moreover, the development coincided with at least moderate prosperity, and the inherent risks of a competitive economy were limited through the very operation of corporative planning and integration. Thus the regime was able to mobilize for its own purposes the technical competence of the entrepreneur class. For them, compliance and cooperation became the only means of retaining title to their business. Those who failed to see the point were invited to ponder the threat of virtual confiscation or to repent in a concentration camp.

Lenin had no such opportunities. He was faced with the task of reconstruction from the very foundation walls. He knew what experimentation would entail. Yet he knew also that "only he who does nothing never errs."[87] His realistic approach was, in his own words, "to begin, to get stuck, to begin anew, and that ten times if it must be, but to reach the goal."[88] Under the circumstances, the most pressing need was the training of technical personnel imbued with the Soviet spirit. The new technical personnel, however, soon enveloped itself in the web of organization theory, which it treated with the same veneration to which Marxism laid claim. One of the results was the plague of what is called in Russia functionalism—a combination of staff overexpansion and excessive separation of line activities grouped strictly according to their functional character. Hence each line function, though a considerable number of them may be involved in the conduct of one single public enterprise, is subject to its own central control agency. The outcome has been floodlighted by Kaganovich as follows:

> Under the functional system, which gives rise to absence of individual responsibility, it is impossible to provide for distinct operative leadership, it is impossible to secure strict personal responsibility, and hence, it is impossible to secure systematic and careful supervision over the fulfilment of decisions. Under the functional system and a too confused structure of organization the middle and higher links of the organization become overstaffed. One day the workers are transferred to the place

[87] *Op. cit.* in note 12, p. 219.
[88] Jaroslawski, *op. cit.* in note 37, part II, p. 239.

of production and another day they are brought back again to the overstaffed offices.[89]

Endless correspondence goes on between the management, the functional sections, and the shops, which hampers the shop manager in his work. More than that, very often purely formal relations exist between the chief of the department and his apparatus.[90]

Thus the production plans for a group of textile mills were scrutinized in five central departments and no less than 46 other agencies; in the end, we learn, "the mills received 19 different sets of instructions, every one of which contradicted the others."[91] As an experienced Soviet worker remarked, "It can't be helped, all this writing comes from their sitting there nibbling their pencils."[92]

The war on functionalism has not yet come to a close. In the meantime the individual manager—confronted with conflicting orders, political vigilante formations in his plant, and the threat of disgrace and even extermination—is hardly in a position to assume vigorous leadership. Moreover, placed in a higher income group, he stands off against the mass of his workers and thus shares the precarious position occupied by the technician class. Is it surprising that Soviet spokesmen have had reason to lecture their followers on the need for "fraternal support"[93] of the engineering profession whose members "often meet with scant welcome"?[94] Is it surprising that "constructive self-criticism" no less often turns against the loquacious toiler who fails to toil? The type Stalin had in mind when reporting a conversation with "an incorrigible chatterbox, a very respected comrade":[95]

I: How are you getting on with the sowing?
He: With the sowing, Comrade Stalin? We have mobilized ourselves.
I: Well, and what then?
He: We have put the question bluntly.
I: And what next?
He: There is a turn, Comrade Stalin; soon there will be a turn.
I: But still?
He: We can observe some progress.

89 *Socialism Victorious,* cited in note 19, p. 121.
90 *Ibid.,* p. 131.
91 *Ibid.,* p. 142.
92 *Ibid.,* p. 132.
93 G. K. Orjonikidze, *ibid.,* p. 625.
94 Kerzhentsev, *op. cit.* in note 39, p. 7.
95 *Socialism Victorious,* p. 84.

I: But for all that, how are you getting on with the sowing?
He: Nothing has come of the sowing as yet, Comrade Stalin.

VI

Viewed from an outside vantage point, totalitarian politics may appear quixotic and bizarre, or monstrous and maniacal. We are entitled to our own frame of values, but reliance on these alone affords us little help in understanding the basic phenomenon posed by our subject. Let us remind ourselves that for the upright Soviet citizen the United States is at best, to use Stalin's phrase,[96] "the citadel of capitalism." The National Socialist knows our country merely as the "anarchy of greed," and official Italy has catalogued it safely as the "decaying liberal order." For the purpose of cool analysis, there is no point in meeting invective by invective. We are here to observe and to reflect.

Several conclusions emerge from our reexamination. In the first place, the temporal Ecclesia of Leviathan is by its very character bent upon securing the universality of its absolutes. Faced with tangible obstacles, it may for a time acquiesce, but it is bound to resume the pursuit of these absolutes whenever the resistance disintegrates. Being engaged in a sacred mission, it is unable to respect the distinction between persuasion and brutal force. Its torture chambers are as sanctified as its temples.

Second, totalitarianism is essentially anti-intellectual. It honors the mind only in support of the soul, and it insists upon complete surrender of the soul itself. Both soul and mind must be protected from the wiles of the Evil One. Thus, among the books outlawed as "incompatible with the Fascist spirit" by Italy's Literature Supervision Office are not only Nick Carter mysteries and Emil Ludwig's Lincoln biography, but also works of Machiavelli, whose *Prince,* we may recall, gave Mussolini in 1924 the inspiration for an academic thesis. The leadership principle admits of no rational basis; its sole foundation is faith. On the other hand, the single leader as the human manifestation of the body spiritual and the substitute for deity is an integral necessity for the operation of the secular religion. The fratri-

[96] Stalin before the Sixteenth Party Congress; Kerzhentsev, *op. cit.* in note 39, p. 88.

cide within the Bolshevist Party since 1933 is no longer mysterious when placed in this perspective. The Old Communists, soaked in Marxist rationality, failed to adjust themselves to Stalin's strategic self-elevation—a move that reflects clearly his grasp of the imponderables of totalitarian statecraft. Indeed, he could have derived encouragement from Lenin himself who even in his day declared the "*independent* elaboration of Marx's theory . . . especially necessary for Russian socialists."[97] After fifteen years of Soviet progress, practical experience had made a dangerous farce of Lenin's optimistic prediction that "under Socialism, *all* will take a turn in management, and will soon become accustomed to the idea of no managers at all."[98] Yenukidse was only too right in calling the Soviet Constitution "the most elastic constitution ever known to history."[99]

Third, the dynamic character of totalitarianism, its boundlessness, its millennial aspirations, its fanatic self-assertion: all these point up the tragic insecurities of "mass man," insecurities magnified, in Italy as well as Germany and Russia, by the material and ideational dislocations of the World War and its aftermath. The new secular religion would never have gained hold were it not for the fact that it satisfies a human need not met otherwise. Even if we discount propagandistic pretense, there remains the spectacle of millions chanting the militant hymns of a militant faith, people like ourselves, people to whom our liberty is a sham and to whom their serfdom is liberation. It is a spectacle made even more haunting by the display of shrewd manipulation and cold cunning on the part of the political control group. Totalitarianism once capitalized on national and individual disillusionment and frustration. Will it perish as a result of military catastrophe? Or will it merely seek a new evangelist?

And fourth, if it does not perish, its corporative features may well change the physical plant of the nation state to such an extent as to set a new international standard. Other countries may find themselves under compulsion to meet the standard. In doing so they will probably rely on their own constitutional framework as long as they can. If they are merely tugged along

[97] *Op. cit.* in note 12, p. 64.
[98] *Op. cit.* in note 5, p. 98.
[99] *International Press Correspondence*, vol. 15, no. 7, 1935, p. 221.

by competitive considerations, they are apt to lose much of their freedom of choice. The obvious alternative is to retain political option by meeting effectively those domestic conditions which cry out for redress. Undoubtedly, our country too will have to apply itself methodically to economic and social readjustment with an eye toward a durable, not a temporary, solution. Our polity cannot survive under the disastrous load of chronic unemployment and sharply marked economic inequalities. As Charles A. Beard observed the other day,[100] if we fail here, "no noble theories on the dignity of man and the perfectability of the species (however necessary to democracy) will save us from some kind of new despotism."

[100] *American Political Science Review*, vol. 33, 1939, p. 886.

PRINCIPLES OF TOTALITARIAN EDUCATION

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(Read November 17, 1939, in Symposium on the Totalitarian State)

To understand anything, Aristotle said, one must trace it from its beginning. While to follow that wise injunction, with fidelity to detail, is impossible in the limits here imposed, one must recognize that the principles of totalitarian education can be properly interpreted only in terms of their background. Grave misconceptions have arisen out of failure to do so: many appear to regard totalitarianism, whether of Bolshevist, Fascist, or Nazi hue, as a freak of social life, whereas its appearance is, in reality, but an event in the continuous stream of history, conditioned by man's past successes and failures in his quest for certainty, stability, perpetuity.

In the light of history, principles of education are mutable; depending centrally on the constitution of the society itself, the prevailing notions of man's nature and status, and the accepted standard of values, they differ as these differ, change as these change.

In the Graeco-Roman world, each city-state educated youth according to its preferred pattern, that they might be relied upon to defend its interests against those of neighboring states. Failing to discover a broader, more inclusive principle which might have served as a basis of further, more perfect integration, the states of antiquity went to their ruin, operating on this principle of divisive localism. In the wake of that disaster men turned away from transient, particularist Athens and Rome and fixed their gaze on a universal City of God. To a world divided by local loyalties, a universal loyalty was decreed, that was to erase the boundaries between Barbarian and Greek, rich and poor, bond and free. But only partial success fell to the lot of those who shaped education to further this universalism. Beneath the surface, the roots of particularism lay dormant, or but little developed, giving slight intimation of the disruptive force

they would exhibit when grown to maturity. From the thirteenth century to the sixteenth, their growth is evident in many ways, but, from a cultural point of view, most clearly in the evolution of vernacular tongues which were soon to become the vehicle of particularist education. Since the sixteenth century, particularism has gradually all but completely supplanted universalism. The sovereign national state, the modern embodiment of particularism, has become the arbiter of men's thoughts and actions, within its borders, through the operation of informal influences as well as by means of formal schooling.

Luther was at once spokesman of his day and of centuries to come, when he demanded that secular authorities take the lead in educational affairs, commanding ". . . their subjects to keep their children at school . . . ," and went on to place secular educational responsibility on the same plane with the duty of military defense.[1] By the eighteenth century the idea of education solely by the state, at the service of the state, had fully matured. La Chalotais demanded ". . . for the nation an education that will depend upon the state alone; . . . because every nation has an inalienable and imprescriptible right to instruct its members, and finally because the children of the state should be educated by members of the state."[2]

The *General Code* of Prussia (1794) provided that "All schools and universities are state institutions, charged with the instruction of youth in useful information. Such institutions may be founded only with the knowledge and consent of the state."[3] The significance of such a complete and exclusive control of education became more and more explicit in the nineteenth century, as the Prussian state grew to maturity and led the way to the formation of the Empire. The exaltation of the concept of the state reached its zenith in Hegel's dictum, "The State is God on Earth." But conditions had not yet prepared the way for general acceptance of such a perfect totalitarianism. Treitschke demurred, indeed, saying: ". . . it [the State] is not the totality of the people itself . . . the people is not altogether amalgamated with it: but the state protects and embraces the

1 Painter, F. V. N., *Luther on Education*, 269–70. St. Louis: Concordia Publishing House, 1928.

2 "Essay on National Education," *French Liberalism and Education in the Eighteenth Century*, by F. de la Fontainerie, 37. New York: McGraw-Hill, 1932.

3 Graves, F. P., *History of Education*, III, 284. New York: Macmillan, 1913.

life of the people, *regulating it externally, in all directions.*" (Italics mine.) As for regulating the mind through education, Bismarck declared: "The soul of a child is like wax. Therefore he who directs the school directs the country's future." Here, in the nineteenth century, was a complete ideological groundwork for totalitarian education which twentieth century nations, due to the concatenation of events, have variously translated into reality.

We shall go amiss if we think of totalitarianism as an absolute, that was in the beginning and must continue to be; or that its roots and potentialities exist only in German, Russian, or Italian soil. Instead, it is a step in the process of becoming, a phase in the life cycle of the social organization—here the national state. Its appearance may be more facile when the ground has been prepared through a period of accepted monarchism, more or less absolute; but it can come in the wake of regimes of self-rule, more or less liberal, more or less extensive in point of time, as ancient and modern history shows. What has been said does not mean that totalitarianism is an historical necessity and inevitable under all circumstances.

Each epoch is apt to make much of the error, incompetencies, weaknesses, of its predecessor. Christian society extolled principles antithetical to the fundamental tenets and practices of paganism. Protagonists of modern monarchism from Machiavelli to Nietzsche proclaimed the beauty, truth and power embodied in authoritarian princes of secular society, denying the validity of principles that had been held immutable. Liberals, seeking to lay the foundations of self-government, denied the fundamentals of authoritarianism, ecclesiastical or secular, and proclaimed new views of man's nature, a new constitution of society, and a goal of indefinite perfectibility for both. In like manner, totalitarianism emphasizes its negation of principles and practices of liberalism, whether found amid the ruins of liberal governments whence it sprang, or in neighboring states with which it competes for position. The two centuries just past were great with the fruits of liberalism, not only in the United States, France and Britain; Germany, Italy, and even distant Russia fell in greater or less degree under its influence, although, in the eyes of absolute monarchs, it was a monster of frightful mien. The war tested liberal principles severely; in

far-reaching circles, many of them were set aside, even where they had strongest roots. Although, at the overthrow of monarchies, many declared that authoritarianism was waning to its end, in the post-war world liberalism enjoyed a brief moment here and there, where wishful thinking had decreed that it should grow, and finally succumbed in the face of problems of reconstruction which it did not, perhaps could not, solve. Totalitarian dictatorships, each more effectively authoritarian than any government known to history, stepped in to try their hands.

Like new brooms, the totalitarians have swept clean. To make certain of loyalty to themselves, each has struck persistently at the old mind to destroy it, and has labored meticulously, employing all the technical aids produced by modern science, to construct the "new." That the "new" mind will be a synthesis of old and new, is obvious to all who have read and reflected on the records of man's past; that it will vary, depending on individual idiosyncrasies of the particular dictatorship and in consequence of differences in each national past, is equally clear. But on some points of ideology totalitarian minds will approach unanimity.

Keen students of politics, totalitarian leaders hold dear the fundamental dictum of Aristotle that educational principles must harmonize with the constitution of the state.[4] Being thoroughly authoritarian, the state actually compels agreement between the two; that is to say, the two become one.

Aristocratic or Timocratic, versus "Natural" Right.—Liberalism's "natural" right of man to participate in, and to determine, the social contractual relationship, the basic charter of free individualism, is denied by totalitarians, and is superseded by the principle of *aristocratic or timocratic* right and capacity for government which resides in the leader and a selected party of trained lieutenants. Each society gives its own meaning to the term, best. Totalitarian ideas as to what constitutes the elite ruling class do not correspond with Plato's idea of rule by the best, that is the wisest; they are closer to the Greek idea of timocracy, rule by the ambitious, strongest, most heroic. While obviously tagged in Italy and Germany by the titles of *Duce* and *Führer,* leadership is no less an accepted principle of the Communist dictatorship. "Lenin Our Leader;" "Our Leader Died

[4] *Politics,* Bk. V, 9; Bk. VIII, 1.

but His Spirit Lives;" "Let Us Follow Lenin Our Leader," these and similar mottoes appropriate to the time are ubiquitous in the Soviet Union. By the principle of anti-pluralism, as will be seen, all avenues are closed to those of divergent faith, or party, who one day might aspire to prove their own capacity for rule. Efforts to convince the masses of this aristocratic qualification of their leaders are continuous and meticulous; they range all the way from the contention of preëminent natural endowment to hints of supernatural intervention. Moreover, as kings claimed to rule, not for self interest, but at God's command, so dictators claim to enforce the people's mandate, not their own. The Communists hold that their dictatorship is the proletariat's very own. Elections are held to prove to the world that the *Führer's* voice is that of Germany. We recall Dio Cassius who says Augustus was advised to let it appear ". . . that the Senate has full authority in all matters. . . ."[5] Modern dictatorships, too, make use of democratic forms.

While it may be useful to have petty men think they have put the leaders in power, dictatorship knows, and frequently admits, the true origin and goal of its power. At such moments it denounces popular rule unequivocally. "We are not liberals. We put the interests of the Party above the interests of formal democracy." "Is it not better," say the Communists, "to have government in the hands of those who are schooled for government rather than in those of a democratic heterogeneous mob? We shall not do worse, indeed, we shall do better than 'democratic' legislatures have done on many occasions in the past. Surely government by those trained in politics, and interested in it, is superior to that of business men turned politicians because they find it a more profitable form of commerce."[6] The National Socialist leader asserts that democratic rule has only appeared during short periods of history, and these have been always periods of decadence. In small things and great, it is said, the movement stands for the principle of "absolute authority" of the leader, whose mission it is to create the Germanic state. It rejects the notion of ". . . a decision by the majority, by which the leader is degraded to the position of the executive

[5] *Dio's Roman History*, LII, 31. 1–2. Translated by H. B. Foster. 9 Vols. London: Heinemann, 1914–1927.

[6] Woody, Thomas, *New Minds: New Men?*, 468, 469. New York: Macmillan, 1932.

of the will and the opinion of the others."[7] He who would be leader bears the "highest unrestricted authority" and with it "the most serious responsibility." "He who is not able to do this or who is too great a coward to bear the consequences of his activity is unsuitable to be the leader. Only the hero is chosen for this."[8]

Whether this autocratic leadership exists to further the German *Gemeinschaft,* the class solidarity of workers, or that Italian destiny which Mussolini feels so "desperately," it needs the ablest and most loyal auxilaries today and must provide for leaders tomorrow. In the Soviet Union these are selected through careful training in youth organizations, *Octabrist, Pioneers, Komsomol,* and by a system of party schools, capped by Communist universities. These mean political education, through theoretical study and active participation, as a continuous parallel of general schooling. Similarly, *Jung-Volk,* the *Hitler-Jugend,* the *Jung-mädel* and the *Bund Deutscher Mädel* will provide the core of faithful followers and be the chief source of tested leadership. The Youth Leader is directly responsible to *Führer* Hitler. The *Landjahr* (1934) for elementary school children; the *National Politische Lehrgänge* (1935)—political short courses for youth 16 to 19 years of age; and the *Arbeitsdienst* itself (for those from 18 to 25); all go far towards impregnating youth and early years of maturity with a profound sense of loyalty to leaders. Selection and specialized training for the best was provided for particularly in January, 1937, when the *Arbeitsfront* leader, Dr. Ley, and the leader of youth, Baldur von Schirach, were authorized to create Adolph Hitler Schools of the National Socialist Party, to receive youth 12 years of age who have shown outstanding ability in the *Jung-Volk,* and to educate them through six classes.

Anti-pluralism.—Next to the principle of aristocratic or timocratic rule, is the *monistic,* or *anti-pluralistic* conception of the state. Pluralism, a doctrine defended by liberalism, negates the very character of the totalitarian state; and, carried to its logical conclusion, would reduce to impotence the state that espoused it. Fouillée touched on such a contingency, a half century ago, when he said: "The danger that, above all others,

[7] *Mein Kampf,* 478. New York: Reynal and Hitchcock, 1939.
[8] *Ibid.,* 479.

a democratic nation must avoid, is the disintegration of society into units with no immediate concern but self interest. . . ." [9]

Republican Germany's power was dissipated through the pluralistic principle which permitted rival group interests—political parties, family, church, labor organizations—free expression of their divergent views and untrammelled action. Struggle between labor and capital diminished the efficacy of her economy; freedom for all parties allowed the rise to prominence of even those, like the Communist and National Socialist, which denied democracy's basic ideals. When the latter came to power, they denied the principle that had afforded them an existence, and destroyed rival parties that might in time, with freedom, destroy them. Göring has said that "It will always remain Hitler's greatest merit that he did not bridge over the gulf between proletariat and bourgeoisie, but filled it in by hurling both Marxian and bourgeois parties into the abyss." [10] Like unto this were the first acts of dictatorship in the Soviet Union and in Italy.

Translated into educational practice, *anti-pluralism* means the rooting out of old divided loyalties and stifling their expression. In a proper sense there are no private societies, but one society. Moreover the dualism, implied or explicit, in the liberal state, between the body politic and society outside it and apart from it, no longer exists. As Mussolini has said, "Everything within the State." "Nothing outside the State. Everything for the State." There is one community; it is indivisible; to cultivate one loyalty is the preëminent function of education whether it operates through formal or informal agencies. Formerly there were numerous more or less autonomous educational agencies, each enjoying wide latitude in setting their goals. Many were closed outright; the others merged in the common national system. Dissident or non-assimilable teachers lost their jobs. *Reichsminister* Rust declared (1933): "In five months time we had to examine as to their political reliability more than 160,000 officials, of whom about 120,000 were elementary school teachers. . . ." [11] Pressures direct and indirect brought all into a common organization. German teachers

9 Fouillée, Alfred, *Education from a National Standpoint,* 4. Translation by W. J. Greenstreet. New York: Appleton, 1892.

10 Göring, H., *Germany Reborn,* 67. Berlin, 1934.

11 Quoted in *Education in Nazi Germany,* 53. London, 1938.

(of whom 97%, it was claimed, belonged to the National Socialist Teachers Union in 1937) take oath to "train the youth of Germany" according to National Socialist ideology from the lowest schools to the university.[12]

Soviet teachers, acknowledging previous "errors" and deviations, formally expressed undivided loyalty to the Party from time to time since 1925. "Henceforth we are not alone. We labor with all the workers of the world. We are with their tested leaders . . . the Communist Party . . . our party . . . In conjunction with our people, the Soviet authority, and under the guidance of the Communist Party, we are constructing a new life and a new school."[13] Doubts, divided loyalties, even contempt for what they are compelled to do—these may be admitted privately on occasion, but, publicly, pluralism of loyalties is denied.

Similarly, Fascism took energetic measures against pluralistic loyalties. "A secret political society in modern, contemporary life is a thing of nonsense, when it is not a menace."[14] As to schools, Mussolini says: "The Government demands that the school shall be inspired by the ideals of Fascism. It demands that the school shall not be, I do not say hostile, but even indifferent or skeptical towards Fascism. It demands that the whole school, in all its grades and in all its teachings, shall educate Italian youth to understand Fascism, and renew itself in Fascism, and live in the historic atmosphere created by the Fascist revolution."[15] Even a "professor of mathematics has a political office and he must be a Fascist."[16] When the first elementary teachers' examinations were given in 1923, emphasis was laid on the *risorgimento*. None were to be approved unless they had read certain books of a patriotic order. Of over seven thousand applying in Piedmont, Lombardy, and Venetia, a fifth were accepted. An effort is made to bring all elementary teachers into membership in the Fascist Association of Primary School Teachers, affiliated with the Party like other organizations of state employees.[17] Secondary and university teachers

[12] *Ibid.*

[13] Woody, *op. cit.*, 454.

[14] Mussolini, Benito, *My Autobiography*, 235. New York: Scribner's, 1928.

[15] Quoted by M. Cantarella, "The Situation of the Learned Class under the Fascist Regime," *Schoolmen's Week Proceedings* (1939), 47.

[16] *Ibid.*, 48.

[17] Schneider, H. W., and Clough, S. B., *Making Fascists*, 103. Chicago: University of Chicago Press, 1929.

have their own National Fascist School Groups, "the spiritual militia of Fascism in the field of national culture." [18] Professors who would not suffer loss of positions, must subscribe to the oath ". . . to exercise the function of teaching and to fulfill all academic duties with the purpose of forming active and valiant citizens devoted to the country and the Fascist regime." [19] Thus do totalitarian states strike at pluralist leanings at the very source.

Anti-rationalism.—Totalitarian education rests on the principle of anti-rationalism, which denies the tenet of early liberals that a rational capacity is in men, though it be but the seed of reason, as Locke said, which may lie dormant or be developed. For this it finds encouragement in certain tendencies of recent psychology which stresses the emotional, irrational side of man, and reduces learning to a mechanistic conditioning of reflexes. Psychology, properly speaking, is taboo in the Soviet Union for there is no psyche, save in mythology that has masqueraded as science; there is only the science of reflexes—*reflexology.* Men are not thinking, but believing, feeling, acting creatures who have been deluded into believing that they think. Ways of feeling, believing, acting are the result of biological and social conditioning: they are of the blood and soil. We think with our blood, says Hitler. Our blood speaks. Education must be *bodenständig,* rooted in the soil and race. *"Trau keinem Fuchs auf grüner Heid; und keinem Jud bei seinem Eid"* is a common way of saying that only Nordic feeling, speech, action, are to be trusted.

There is no universal truth; man is incapable of such objectivism as eighteenth and nineteenth century intellectuals conceived. "German Physics?" asks Professor Lenard. "I might rather have said Aryan Physics or the Physics of the Nordic species of man. . . ." There is no international science. "In reality science, like every other human product, is racial and conditioned by blood." [20] For obvious reasons, social sciences are more profoundly affected by this renunciation of objectivism. The purpose of university education ". . . is not objective science . . ." says Dr. Krieck, Rector of the University

[18] *Ibid.,* 105 *f.*

[19] Cantarella, *loc. cit.,* 48.

[20] Quoted by E. Y. Hartshorne, *The German Universities and National Socialism,* 113. London: Allen & Unwin, 1937.

of Frankfurt.[21] Political understanding and ability are necessary for the university tutor, but political soundness is placed unconditionally first.

Application of this principle is in part responsible for the decrease in enrollments. The pre-war university prepared intellectuals who were welcomed throughout the world for their knowledge and their usefulness; those of Republican Germany welcomed a great increase of students and crowded the ranks of unemployed with dissatisfied intellectuals; those of today are to train only as many as are needed in the several callings from year to year, and these must pass the tests of racial and ideological qualifications, mental ability and participation in Party work.[22] The same principle also accounts largely for dismissal of from 14 to 16 per cent of university teachers, excluding assistants, in the first three years of the Nazi regime. For, obviously, if independent reason and objectivity are impossible, if one thinks and speaks with his blood, the universities must have blood that "thinks" on the right bias.

Although the reasons assigned vary in part, untrammelled scientific objectivity has been similarly discountenanced, ridiculed, and driven from universities under Fascism and Bolshevism. Italian university teachers were subject to espionage, students being directed by the Party (1926) to watch them and to report the names of those whose deviations from dictated truth they had detected. Inside and outside the universities, the intellectuals who tried to maintain independence of thinking have been hounded. "Over 200 university professors were dismissed from their posts . . ." recently, because of Jewish ancestry, according to Professor Cantarella.[23] If in Germany the products of wayward, free intelligence have been burned, the consequences to freedom of intellectual expression are no less real or effective in Italy, where "Orders are sent out systematically and quietly to librarians to remove from the shelves all those books which the regime frowns upon; [and] militia men are sent to the publishing houses and bookshops to confiscate quietly the copies in stock." The unpublished Fascist Index lists forbidden intellectual fruit.[24]

[21] *Education in Nazi Germany,* 46.
[22] *Ibid.,* 47.
[23] *Op. cit.,* 44.
[24] *Ibid.*

In the Union of Soviets it is not the bias of blood and soil but that of class, the orthodox dogma, that stands in the way of intellectual detachment. The mentality of those of noble or priestly origin cannot be trusted. Peasant or proletarian origin; activity in youth organizations and the Party; and intensity of loyalty to the cause, combined with tested ability, are the chief tests of fitness for university and other professional posts, rather than independent objective research. Professor Pinkevitch was investigated for deviations from orthodoxy, as then conceived, was dropped from the rectorship of Second Moscow University, and assigned a position of slight importance in *Narkompros*. There is but one "true" history and science of society. Unbiased, objective history, the Bolsheviks say, is a delusion or an hyprocrisy of bourgeois society. History must enhance the perception of class lines and prepare us for the class struggle. Teachers of higher as well as of lower schools do not strive against this interdiction of independent treatment, at least not long. When the Party takes a new line, or political errors are discovered in text books, they have to be revised—and the teacher follows the "new truth." [25]

It is obvious that the effective implementation of the principle of *anti-rationalism* is accomplished in all totalitarian states by the same means: by the denial of freedom for investigation and expression on the part of teacher and learner; and through the absolute domination of minds through state-controlled cinema, radio, and press. These are supplemented powerfully by appeal to fears: fear of loss of position; loss of livelihood; loss of life itself. Periodic cleansing of the Party is a standard, tested device for producing conformity to an accepted pattern of response. Independence of mind does indeed become a myth.

Anti-rationalism, combined with *anti-pluralism,* tends to frustrate religious freedom in totalitarian states. If mind be free, man may conceive a truth and authority higher to him than the nation, higher than a class. If he does, he is moved to act in some degree conformably thereto. The divisive effects of pluralism at once appear. Hence totalitarian societies, though they cannot determine every inmost thought, prohibit its translation into actions which are competitive with state action, or in any way militate against the enhancement of national or party

[25] Woody, *op. cit.,* 271 *ff.*

prestige. Two choices are open: one's religious life is reduced to a state of reflection and contemplation; or religious ideas are assimilated in the idea of the social community which can have no other god before it. Both revealed religion and pursuit of rationalism have led many individuals to accept pacifism. In one case, destruction of human life is held a sin; in the other, it is simply a futile, irrational and unnecessary method of solving human problems, which, after the conflict, remain still to be solved by reason, if at all.

Collectivism.—The principle of *collectivism* is common to the totalitarian states, though the reasons for it, and the degree of emphasis on it in education, may differ widely. Essentially it denotes a decline and a denial of the importance of the individual person and a correspondent increase of emphasis on the importance of the social organization. As an·interpretation of man's life, it reflects the influence of a tighter economy in which individual persons have not the wide latitude they had in a world that was less closely knit. The eighteenth and nineteenth centuries laid marked emphasis on the individual; education aimed at the enhancement of his success, material and immaterial. Today, he is told, sharply in totalitarian states, more mildly in those disclaiming totalitarian tendencies, that his interests are not first but secondary, or even of a more inferior order. The idea is by no means novel; ancient Sparta sacrificed the individual to the collective weal; highly developed nationalism in some quarters, during the nineteenth century, stressed the sacrifice of the individual for national welfare. The post-war world, however, marked by the greatest feeling of insecurity on the part of states, internally and externally, has passed all previous bounds in its emphasis on the claims of the collective. The individualist is, in the popular conception at least, in totalitarian states, at best a futile and curious anachronism, at worst a menace to society.

In the Soviet Union collectivism's economic significance is most stressed. *Laissez-faire* economy, which gave the individual right of way and interfered with him little or not at all, has proved a failure. It was a satisfactory principle for the most talented; emphatically less so for the less able. Instead of prosperity of the ablest proving a benefit to the least capable, as a result of general improvement, they said, it proved their

destruction, reducing them to pauperism. Collectivism is necessary to protect the weaker from the stronger.

Lenin gave authoritative expression to this fundamental tenet that runs through all Communist literature. "We want to turn Russia, a country of poverty and lawlessness, into a rich country. This can be done only by means of collective common labor." It is at once the end and means to Communism. "Growth of cooperation is identical with the growth of socialism." By introducing social, instead of private, ownership of the means of production and exchange, by introducing well-regulated organization in the social process of production, so that the well-being and the many-sided development of all members of society may be insured, the social revolution of the proletariat will abolish the division of society into classes and thus emancipate all of oppressed humanity, and will put an end to all forms of exploitation of one part of society by another."[26]

This states the collectivist ideal. To detail its practical bearing on their education would be to tell the whole of its history, from *crèche* and *Kindergarten* to the university. *Collectivization* appeals to the eye and ear from the schoolbook, the newspaper, the cinema, the radio, the sport club, and theatre; it shouts a challenge from ubiquitous banners at the thoughtful and the heedless as they pass through city streets and dusty village lanes; but the most powerful of educative influences are part and parcel of everyday life itself, derived from cooperative activities in factory and collective farm. Most dramatic of efforts to teach collective interest and responsibility, and respect for even hard "black work," was "Saturdaying"—a practice very common in former years. On these occasions, teachers and other white collar workers gave their holiday to perform collective labor of the lowest sort. A poster widely distributed in the Union showed Lenin, the leader, out "Saturdaying" with workers in the street.

Collectivism as an educational principle affects National Socialist education profoundly, but in certain respects differently. First, there is no collective ownership and, second, the emphasis is on collectivity of the national *Gemeinschaft* (that is, community, in a more inclusive and profounder sense than we commonly use the term), rather than labor solidarity in the class struggle.

[26] Woody, *op. cit.*, 297.

As for likenesses, it is anti-individualistic, stresses the utility and dignity of common labor, and makes performance of work a passport to social approval and a guarantee of social unity and future freedom. We shall go amiss if we allow ourselves to see only the common view of those outside Germany who regret the passing of individual freedom in Germany, or in Russia. *Sturm und Drang* periods of the past have witnessed the call for sacrifice of individual freedom, the subjection of the individual, that the community might live. "We must hang together, or we shall hang separately," is not new to our ears. Germany was weak; by sacrifice of individual freedom the collective was to become strong. Realizing that, it need occasion no surprise that Nazi education stresses group comradeship and loyalty, and the idea of community good above individual gain, or *Gemeinnutz vor Eigennutz.*

Moreover, National Socialism is a testimony to the influence of the psychology of the soldiers who returned home from a struggle where they had risked everything, even life itself, and found division, fratricidal struggle, and weakness in the community for which they had fought. Some turned to Communist international ideology, with its collectivist ideal, and its promise of unity and strength; others, ultimately more of them, turned to National Socialism with its emphasis on the National *Gemeinde* which was to become strong through unity. Strong against disunity and class privileges within the state; strong against enemies without. "This then," says one of the contributors to a recent study,[27] "is the secret of our idea, and in it lies the power of National Socialism: Unity is the goal of our Leader, who wants to make the people strong, so it may become powerful again."

As to implementation of the collective *Gemeinschaft* ideal, National Socialism relies on uniform propaganda through all schools, press, radio, cinema—every carrier of the word. More reliance, however, is put upon the education of compulsory labor service and agencies of political activity for young and old. The Leader approved (1930) the principle of labor service for all citizens regardless of class, as "the great training school of National Socialism" and as a "service of honor to the nation." [28]

[27] Abel, Theodore, *Why Hitler came to Power,* 138. New York: Prentice-Hall, 1938.

[28] *German Youth in a Changing World,* 41. Berlin, 1936.

These were written into the Labor Service Law, put in force in 1935.

We ought to observe particularly, finally, that *collectivism,* proceeding from divergent basic assumptions, has different consequences for national minorities. In the Soviet Union, where the basis of *collectivism* is social ownership and class solidarity rather than national or racial *Gemeinschaft,* a great deal of emphasis has been placed on cultural freedom for national minorities and with highly significant results. Where the blood tie and the idea of cultural superiority of the master *Volk* is taken as the basis for collectivism, minor nationalities must be driven out, extirpated, or live under restrictions which reduce them to the status of pariahs.

Activism.—A close correlate of the principle of *anti-rationalism* is *activism,* a principle which affects both method and content of education. *Activism* rests essentially on a monistic view of man's nature which negates that ancient dualism, mind and body. Such monistic tendency is by no means a creation of totalitarian society; its scientific basis is found in much modern psychology, which stresses the oneness of mind and body, or the non-existence of mind as previously conceived. This monistic interpretation is serviceable to the attainment of totalitarian ends. It is reflected in the constant emphasis on the wholeness and unity of man's life, as opposed to an earlier emphasis on the cultivation of intellect, as of superior importance, coupled with neglect of the physical and even contempt for it.

National Socialists hold that pre-war Germany more completely than any other nation embodied an undue emphasis on education as an intellectual activity, whose end was knowledge rather than character and capacity for action. If the English identified life with sport, the Germans identified it with knowledge. The Germans must change their ways, they must decrease the emphasis on knowledge and increase that on body and character, so as to form an harmonious whole, capable of serving the nation. Knowledge is not to be neglected; no German needs to be reminded of that, they say. Honor, loyalty, willingness to bear responsibility, self-sacrifice, courage, determination, self-confidence, modesty, obedience and a knowledge of everything that concerns his profession—these are to be

attained by everyone, but before all these, he must have a body "slim and strong, as swift as a greyhound, as tough as leather, and as hard as Krupp steel," as Hitler said.[29] In conformity with this ideal, formal schooling takes a less exalted place than formerly. The school must realize that, even with fundamental alterations, it can ". . . make but a small contribution to education as a whole." Physical sports, labor service, and military service, the wide range of activities of Youth organizations, are the most important agencies, whose education through *activism* is to supplant the intellectual "weakling" of the past.

The emphasis on the cult of physical perfection, having as its end the service of the national will, is universal in the totalitarian states, though each may will different ends. The *Balilla* and *Avanguardia* provide an *activist* training, primarily physical, for Italian children and youth, as do the German and Russian Youth organizations, which culminates in the capstone of party membership and military training.

It should be observed that *activism* as a principle of education has two divergent interpretations, one that is free, the other controlled. Education through free activity, free growth, made a strong appeal to liberals who have held that the principle connotes activity in the most complete and all-sided sense; freedom actively to determine goals and to act for their realization. The *activism* of totalitarian education, while indebted to liberal forerunners, deviates from their views and permits and encourages only controlled activity, limited to the attainment of goals that have been pre-determined. This shift from free activism to controlled, limited activism may be seen clearly in Russia. Certain liberal reformers before the Revolution held to the Rousseau-Pestalozzian-Tolstoyan idea of free *activism;* but those who lived to see the Revolution and subsequent totalitarian rule, for example, Shatsky, forsook the broader for the narrower activism, seeing that pedagogical principles must conform to the political, and accepted the pre-established ends which the Party decreed. Similarly, the *activism* of experimental schools under the Republic and of the early *Arbeitsschule* reform movement, has been brought under control through the emergence of a regnant nationalism that determines all ends.

[29] Graefe, Theodore and Gerhard: *German Education Today,* 10. Berlin: Terremare Office, 1937.

Men appear to have grown weary with facing problems, weary of the responsibility for resolving them, which liberalism once laid upon them, and doubtful of their own capacity to solve them. Under these circumstances, when problematic situations increase in number and weighty importance, when instability and uncertainty abound, then timid, fearful man cries for a king, a cunning man, an able man, to take responsibility and solve his problems for him, and bends his energies to that one's will.

In the foregoing, *aristocracy,* or *timocracy; anti-pluralism; anti-rationalism; collectivism,* and *activism* have been shown to be common principles of totalitarian education. Different factors may account for their acceptance in the several states, and many divergences in practical applications appear; but these common principles constitute the basic educational framework of societies that look for stability and perpetuity on the basis of strict regimentation of the individual's capacities, rather than through their free development; they rest on a conviction, well or ill-founded as may be, that the experiment of liberalism, whether in its economic, political, or educational consequences, failed to show that men, if they had freedom, would use it for the common good. How completely outmoded are the conceptions of education outside totalitarian boundaries may be gathered from Alfred Baeumler, who explains that totalitarian educators think "Copernically" while all others "are still reckoning in terms of the Ptolemaic system." They cannot ". . . possibly feel these formulas of an outworn world of thought as anything but a dance of mimes around the cenotaph of that concept of man which was produced by the enlightenment." [30]

[30] "Race: A Basic Concept in Education." *World Education,* Nov. 1939, Vol. IV, 506.

THE TOTALITARIAN PHILOSOPHY OF WAR

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CHARACTERISTIC of our time is the progressive disintegration of language as an instrument of universally accepted rational concepts. The same words cover different and sometimes opposite meanings, and much confusion is due to the indiscriminate and ambiguous use of words. One of the words which has lately changed its meaning is "war." Until very recently war was regarded, even by Clausewitz or Bismarck, as a strictly circumscribed and exceptional state of affairs. War was an instrument of politics, to be used only as the ultima ratio, as a case in extremis. Politics was the art of avoiding war which was considered an anormality. The effort of statesmen concentrated upon maintaining the normal life, and the occurrence of war was frequently regarded as a proof of faulty statesmanship, as a bankruptcy of policy. Even Mussolini ended the article which he contributed under the title *Audacity* to the first issue of his Il Popolo d'Italia on November 15, 1914 with the words: "This cry is a word which I would have never used in normal times . . . : a frightening and fascinating word: war!"[1] In the last years, however, Fascism has proclaimed war the normal state of life, not an aberration, caused by an intellectual or moral insufficiency, but the culmination in which the vital and ethical energies of man reveal themselves at their best. Politics now becomes a preparation for war, receives its direction and meaning from this extreme "Ernstfall." War ceases to be anything strictly circumscribed and limited; the border lines between war and peace grow more and more fluid, everything becomes part of warfare, actual or potential, and everything may therefore be called peace. Where the whole way of life

[1] Benito Mussolini, *Scritti e Discorsi. Edizione Definitiva* (Milan, Ulrico Hoepli), vol. I, p. 10.

is dominated by the norm of war, the words war and peace themselves lose their meaning.

A similar society is known to us from the descriptions of Spartan life in antiquity. This Peloponnesian state, with its economic foundations in the serf labor of the helots and in the exploitation of conquests, was nothing but immense barracks where the spirit of the army shaped and permeated all expressions of civic and individual activity. In contrast to the Athenian democracy, no interest in arts or letters was evinced, no political life with party tensions and dissensions was allowed, the luxuries of civilized life were despised. Physical education predominated, the pedagogic effort was entirely concentrated upon building up bodies and characters hard as steel; even the teaching of ethics as far as any existed was entirely subordinated to the goal of rearing a warrior race.[2] Disobedience of even the most arbitrary commands was regarded as the greatest offense. Trade, commerce, and the arts of peace were haughtily scorned, justice and moderation derided as sentimentalism. From the cradle to the grave, men and women were regarded solely as means for strengthening the military machine.

This first example of the totalitarian state disappeared under the influence of stoic humanism, of Christianity, and later of that rational ethicism which Grotius developed from Stoic and Christian sources. Only the Prussia of Frederick William I revived consciously the Spartan ideal as a model for the state and a conduct of life for the citizen. "Concern for arts or for science appeared to him in no way better than one of the seven deadly sins. Nach seinem Willen sollte alle Welt nur eine Sache im Kopfe haben, die Männer das Kriegswesen, die Weiber den Haushalt."[3] As is well known Frederick II stated that under Frederick I Berlin had been the Athens of the north—an opinion greatly overstating the case—and that under Frederick William I it became Sparta—an opinion this time much nearer the fact. And Johann Wilhelm Gleim started his "Preussische Kriegs-

[2] Spartan children were instructed in stealing, robbing and deceiving. They were severely punished when caught, not because they were stealing, but because they were stealing badly. In the drabness of their life "the only relief found by the Spartans was spying on each other" (Leo Strauss in *Social Research*, November 1939, p. 517).

[3] Carl Biedermann, *Deutschland im achtzehnten Jahrhundert*, vol. II, Part I (Leipzig, J. J. Weber, 1858), p. 167 f. Frederick William I would have abolished the Prussian Academy founded by his father if the Academy had not proposed to help in training surgeons for the army by the study of anatomy.

lieder in den Feldzügen 1756 und 1757 von einem Grenadier" with the exclamation: "Berlin sei Sparta!"

But although Prussia foreshadowed the totalitarian philosophy of war, its application was limited by the acceptance of the moral standards of Christianity and of common western civilization. Bismarck's statesmanship employed war as a means for definite ends. Cynically and brutally he was ready to use this means, but it was never allowed to become dominant; it was always limited to its subservient function, and at a given moment stability and security were preferred to risk and expansion. The totalitarian philosophy of war derides stability and security with its accompanying preference for a quiet and comfortable life. War becomes the highest and normative state of life; what is called peace is only a pause between the "real" manifestations of life, preparing for them, subservient to them.

This totalitarian philosophy of war grew up under the influence of two intellectual movements, the roots of which we can trace back to the second half of the nineteenth century.[4] One of them is the belief that the substance of life and history is struggle and conflict. The application of Darwinism to the social sciences made war and strife appear as the normal manifestations of all nature of which man was only part. Soon the accepted moral values were revealed as ill-suited to this new conception. The fundamental basis of Chancellor Hitler's *Mein Kampf* is an interpretation of man according to which he is purely a natural being, biologically determined, and inescapably subject to the "iron logic of nature" which he has to obey as animals do, if he wishes to preserve or increase his strength and to be true to his "nature." In such a world strength and success alone count: the "idle dreams" of universal truth and justice disappear in the dust heap of bookishness before the triumphant march of full-blooded man. In the face of this nihilism, this twilight of all moral values, Life is exalted as the only and inexorable arbiter of all action and conduct. "World history is the world tribunal: it has always justified the stronger, the fuller, the more self-assured life, has given it the right to existence, whether or not it was regarded as good. It has always sacrificed truth and justice to might and vitality,

[4] I tried to trace the genesis of totalitarian "Weltanschauung" in my two recent books, *Force or Reason* (Harvard University Press, 1938) and especially *Revolutions and Dictatorships* (Harvard University Press, 1939).

is abolished, and property becomes a fidei-commissum. Ernst Jünger created in his *Der Arbeiter. Herrschaft und Gestalt* the apotheosis of a thoroughly mechanized and militarized worker, a modern machine-man, a member in a group closely knit together in hierarchical order and pervaded by one spirit, united for higher efficiency and external action.[8] In a similar way Mussolini stressed the unity of soldiers and workers, he called his Popolo d'Italia the "newspaper of combattants and producers." His cooperative state is the expression of an "Italian socialism," based upon presuppositions hardly different from those of "German socialism."

This worker and soldier is being trained in the totalitarian states to accept hardness of life and risk as the normal life, tragic heroism as human destiny, and to reject contemptuously any desire for a sheltered and a comfortable life, for the amenities of the ridiculed bourgeois or western civilization. In intentionally terse and striking sentences Mussolini has tried to formulate this new way of life: "Tutta la nazione deve essere militarizzata . . . Nella vita la felicità non esiste . . . Io considero la Nazione Italiana in stato permanente di guerra . . . Vivere per me è la lotta, il rischio, la tenacia . . . Il Fascista disdegna la vita comoda . . . Il credo del Fascismo è l'eroismo."[9] Whereas it is characteristic that Lenin and Stalin had no need to boast of their humble origin, and certainly never emphasized that they had been common soldiers in the war or had wished to do their duty and regarded their soldierly duty and experiences as the zenith of their lives, Hitler and Mussolini both stress their humble origins and above all their experiences as common soldiers in the World War.[10] Asked by Emil Ludwig,

[8] Ernst Jünger, *Der Arbeiter* (Hamburg, Hanseatische Verlagsanstalt, 1932). In a former book *Das abenteuerliche Herz* (Berlin, Frundsberg Verlag, 1929) Jünger had expressed the underlying nihilism of the fascist attitude, its despair of any values in life, its glorification of vitality in itself: "To what purpose one exists, that may never be learned; all so-called goals can only be pretexts of destiny. But that one exists, that is the essential. . . . For that reason this time demands one virtue above all others: that of resolution. It is essential, to find will and faith, quite apart from and irrespective of any contents of this will and faith. . . . All the fight today about flags and symbols, laws and dogmas, order and systems, is humbug. Your very horror of these quarrels reveals that you are not in need of answers, but of sharp questions, not of flags but of combats, not of order but of rebellion, not of systems but of men."

[9] *Op. cit.*, vol. I, p. 283; vol. II, p. 230; vol. V, p. 238 f.; vol. VIII, p. 69; vol. IX, p. 43. These are only a very few examples taken at random.

[10] On Hitler see *Revolutions and Dictatorships*, p. 181 and 343. From Mussolini: "I am proud to be a son of laborers. I am proud to have worked with my own

of what Mussolini is proudest in his career, the head of the Italian government answered without hesitation, of having been a good soldier. The answer was probably sincere; it would have sounded improbable in the mouth of Bismarck or Crispi, of Lloyd George or Clemenceau, of Bethmann-Hollweg or Neville Chamberlain.

In this new philosophy of life strategic considerations of soldiery take precedence over economic well-being. Walther Rathenau, the famous German industrialist and statesman, had pronounced economics to determine our fate. "Wirtschaft ist Schicksal." In this attitude the two great currents of the later nineteenth century, liberal capitalism and socialism, had agreed. Now they are confronted with the proclamation of the precedence of politics over economics. "Politik ist Schicksal," affirms Carl Schmitt. Politics is life and life is politics, as Oswald Spengler says; but with the discarding of economic man, the meaning of political man has been changed as fundamentally. Now arises a concept of politics which receives its meaning, not from what has been considered the normal life of society, but from the border-line case. The normal does not try any more to dominate and limit the anormal; it is the anormal, the exceptional, the ultima ratio, which determines and directs the normal. A German political scientist has best expressed this new attitude. "One can say that here, as elsewhere, precisely the exceptional case has a particularly decisive meaning and reveals the heart of the matter. . . . It is from this most extreme possibility that the life of men gains its specific political tension."[11] In his theory about the origins and legitimacy of right, a theory which he calls decisionism, Carl Schmitt lets right be determined by the legislator who has the power to realize and enforce the decision. Ideal justice or positive law are discarded as norms of law-making. Starting from the extraordinary situation, the "Staatsnotstand" ("Not kennt kein Gebot"), where the necessities of existence seem to demand the disregard of abstract justice or of the existing positive law, Schmitt applies this "anormal" case to the "normal" course of existence.

hands." And again: "Considero il momento più bello della mia vita quello in cui fui lacerato dalle ferite." And again: "I am and I remain on the ramparts (sulla breccia); I am bound, not to my caprice, but to my soldier's post." *Op. cit.*, III, p. 49; IV, p. 18, p. 248.

[11] Carl Schmitt, *Der Begriff des Politischen* (Hamburg, Hanseatische Verlagsanstalt, 1933), p. 18.

"Right" is thus always dependent upon the concrete situation and has its source in the decision with which the supreme power-authority meets the situation. "Jegliches Recht ist Situationsrecht." As each situation is unique and concrete, there cannot be any general and abstract norm. Each decision is valid only for its own situation. "Justice" becomes the function of the power which makes the essentially political decision; political and judicial functions are no longer separated, although political decisions continue to be made to appear as judicial ones. But in practice, and frequently in theory, the judicial function is subordinated to the political. In his address to the Deutsche Juristentag in 1936 Rudolf Hess repeated Treitschke's words: "Alle Rechtspflege ist eine politische Tätigkeit."

This exaltation of life over law—in Spengler's terminology of Dasein over Wachsein—produces a dangerous existence on the rim of an abyss. Carl Schmitt bases his concept of politics on the inescapable antagonism between friend and enemy, an antagonism as fundamental as that between good and bad, or between the beautiful and the ugly. Political conflicts are therefore for Schmitt not rationally or ethically determined or solvable; they are "existential" conflicts,[12] in which existence itself is at stake. For this political theory war is the culmination, the zenith of political life and that means of life in general: the inescapable friend-enemy relation dominates all life. This political philosophy corresponds to the supposed primitive combative instinct of man, who tends to regard anyone who stands in the way of the realization of his desires as a foe who has to be done away with. This concept clearly marks all the policies, internal and foreign, of Chancellor Hitler's government. Civilized statesmanship, on the other hand, consists in finding the ways and means to overcome the primitive instincts by compromise, by patient negotiations, by an effort at reciprocity, and above all by the acknowledgment of universally binding law.[13]

[12] Existential is one of the new expressions corresponding to attitudes in Germany produced by the post-war nihilism. The "existential" political theory of Schmitt corresponds to the existential philosophy of Heidegger. In his *Der Begriff des Politischen,* p. 8, Carl Schmitt says: "Der Feind ist in einem besonders intensiven Sinne existenziell ein Anderer und Fremder, mit dem im extremen Fall existenzielle Konflikte möglich sind. Derartige Konflikte können weder durch eine im voraus getroffene generelle Normierung, noch durch den Spruch eines 'unbeteiligten' und deshalb 'unparteiischen' Dritten entschieden werden." And on p. 15: "Der Krieg folgt aus der Feindschaft, denn diese ist seinsmässige Negierung eines anderen Seins."

[13] See my *Force or Reason,* p. 19 f. Schmitt says *op. cit.,* p. 48: "Die Höhepunkte der grossen Politik sind zugleich die Augenblicke, in denen der Feind in konkreter Deutlichkeit als Feind erblickt wird."

Thus from all sides war is acclaimed as the supreme moment of life. Whereas in the western nations after the World War, the war itself was recognized as a great calamity and tragedy, a very large part of the German people did not regard the war as a tragedy or calamity, but the defeat. They blamed all the ills, which the western nations blamed on the war, on the peace treaties. Whereas the western nations blamed themselves for having got into the war and having made the peace, the large majority of the Germans never blamed themselves, but blamed only the enemy for having devised the peace. National Socialist propaganda increased this growing estrangement from the west by glorifying the war and the German army, and by strengthening the already too strong German tendencies of seeing the source of all their maladjustments, not in their own faults or shortcomings, but in the machinations of their "enemies." "We National Socialists know that the Great War from 1914 to 1918 will once live in the memory of later generations as a mythical great deed without equal." [14] Hitler's racial theory had the effect of destroying the remaining sense of reciprocity and responsibility in the German people and of convincing them that on account of their superior qualities they are always right and that the heroic warrior ideal which they have cultivated justified their world domination. "The struggle for a German rebirth is a struggle for the assertion of the German hero ideal against the democratic shop-keeper ideal," says Rosenberg, and Hitler praises Germany because she was "a most magnificent example of a nation, created on the foundations of pure power-politics. Prussia, the germ cell of the Reich, arose through radiant heroism ('durch strahlendes Heldentum') and not through financial manœuvres or commercial transactions; and the Reich itself was only the most glorious reward for power-political leadership and warrior's courage in face of death." [15]

Thus the pacifism which became predominant in the western democracies after the World War was stamped out by the new philosophy in Fascist countries. "War is to the man what maternity is to the woman. I do not believe in perpetual peace; not only do I not believe in it, but I find it depressing and a

[14] Alfred Rosenberg, *Das Wesengefüge des National Sozialismus*, 4th ed. (Munich, Eher, 1933), p. 9.

[15] Alfred Rosenberg, *Der Mythus des 20. Jahrhunderts*, 37th ed. (Munich, Hoheneichen Verlag, 1934), p. 639. Adolf Hitler, *Mein Kampf* (Munich, Eher, 1933), Vol. I, p. 169.

negation of all the fundamental virtues of man."[16] The preamble to the statute of the Italian Fascist party of December 20, 1929, prides itself that "from its beginnings until now, the Party has always thought of itself as in a state of war. Fascism is above all a faith under the impulse of which the Italians work as soldiers, pledged to achieve victory in the struggle between the nation and its enemies." This attitude had already been foreseen by Mussolini during the years of the World War, and by Oswald Spengler in his *Decline of the Occident* where he characterized the new time: "Life is harsh. It leaves only one choice, that between victory and defeat, not between war and peace. . . . Pacifism implies the personal renunciation of war on the part of the great majority, but with that also the unadmitted readiness to become the prey of others who make no such renunciation. It begins with the desire of a general reconciliation, and it ends with no one stirring a hand as long as the misfortune befalls only the neighbor."[17]

For outside consumption and for purposes of diplomatic war the spokesmen and advocates of the new philosophy of totalitarian war may speak sometimes of their desire for peace and may indulge themselves in some kind of pacifist propaganda, strictly forbidden under heaviest penalties to their own subjects. But even a cursory perusal of the fundamental sources of the new philosophy, be it the writings and speeches of Mussolini or Hitler, be it the vast literary output of their enthusiastic followers, reveals the unbridgeable gulf between their own profession of faith and their pragmatic toying with the pleasantness of peace. On August 25, 1934, Mussolini declared: "We are

[16] Mussolini, *op. cit.*, vol. IX, p. 98. Well known are the anti-pacifist passages in the often translated article by Mussolini on the doctrine of Fascism in the *Enciclopedia Italiana.* Translations can be found in Alfred Zimmern, *Modern Political Doctrines* (London, Oxford University Press, 1939), p. 31 ff. and in Michael Oakeshott, *The Social and Political Doctrines of Contemporary Europe* (Cambridge University Press, 1939), p. 164 ff. See there too on p. 180 f. the two Fascist Decalogues. The most characteristic passages are: "Above all, Fascism believes neither in the possibility nor in the utility of perpetual peace. . . . War alone brings up to their highest tension all human energies and puts the stamp of nobility upon the peoples who have the courage to meet it. . . . Fascism carries over this anti-pacifist spirit even into the lives of individuals. It is education for combat. . . . The Fascist looks on life as duty, ascent, conquest."

[17] *Op. cit.*, p. 538, 545 f. See also in his *Politische Schriften,* p. 55: "War is always the higher form of human existence, and states exist for the sake of war. . . . Even were a weary and lifeless humanity desirous of renouncing wars, it would become instead of the subject of war, the object for whom and with whom others would wage wars."

becoming and we shall become ever more, because we will it, a military nation. Because we are not afraid of words, I shall add: a militarist one. To say it fully: a warrior nation (guerriera), which will be endowed to an ever higher degree with the virtue of obedience, of sacrifice, of dedication to the fatherland. That means that the whole life of the nation, the political as well as the economic and spiritual life, must be directed towards our military necessities. . . . I recall to you that the military forces represent the essential element of the hierarchy among the nations. Nothing has yet been found which could substitute that which is the clearest, most tangible and most determining expression of the complex force of a whole people: that is the size, the prestige, the power of its arms on land, on sea and in the aïr." [18]

In the totalitarian régimes society is entirely subordinated to the state and even destroyed by it. There is no individual or social sphere outside the state. But even the state is not a true state; it is not more than the apparatus of one party which entirely identifies itself with the state and society, absorbing all their functions. The great difference even between Bismarcko-Wilhelminian Germany and the state created by National Socialism was clearly indicated by the declarations of the respective leaders at the outbreak of the great wars. In 1914 Emperor William II declared that he no longer recognized any parties among the Germans and that he stretched out his hands to all his internal opponents for cooperation and internal peace. At the outbreak of the war in 1939 Chancellor Hitler did not invite the cooperation of any internal opponents, but on the contrary, threatened them in terms unusually violent, even for him. The famous protestant theologian, Karl Barth, rightly pointed out that the National Socialist state consists in the disintegration of the just or right state, that it is a state only, in so far as it is not yet National Socialist in certain remnants carried over from the old state, but that otherwise it is an anarchy tempered by tyranny, or a tyranny tempered by anarchy.[19] The disappearing social order is replaced by an extension and

[18] *Op. cit.*, vol. IX, p. 113–115. See also p. 197 f.; vol. III, p. 60; vol. IV, p. 294; vol. V, p. 29, 118, 181; and passim.

[19] *Die Kirche und die politische Frage von Heute* (Zollikon, Evangelische Buchhandlung, 1939), p. 35. This remarkable book had been published in an English translation, *The Church and the Political Question of Our Day* (New York, Scribner, 1939).

imitation of the order of the army. The terminology of war, warriors and struggle, is applied to every phase of life, even the most civilian. All differences between the military and other walks of life are gradually abolished until the totality of life is subordinated to the set of values of the army, and farmers and teachers, industrialists and scholars are turned into soldiers of the régime.[20] As in an army, discipline and hierarchy, appeal to comradeship, and readiness to sacrifice are stressed. The personality of the leader gets full scope and is elevated by the amorphism of the masses beneath it. The fate of the individual in a nation which has become an army has been masterly formulated in Mussolini's famous words: "In the Fascist state the individual is not suppressed, but rather multiplied, just as in a regiment a soldier is not weakened but multiplied by the number of his comrades." The National Socialist youth exalted as its educational and social ideal the Männerbund, a soldiery order after the model of the Teutonic knights or the Prussian officer's corps. "The principle," wrote Hitler "which in its time made the Prussian army the most wonderful instrument of the German people, must in the future become the principle of the structure of our whole conception of the state: authority of every leader downwards and responsibility upwards."[21] The totalitarian philosophy of war has been aptly summed up by Carl Schmitt: "War is the essence of everything. The nature of the total war determines the nature and form of the totalitarian state."[22]

[20] The editor of the *Historische Zeitschrift,* Professor Karl Alexander von Müller, ends an editorial postscript about the war in the issue published September 15, 1939 (160, 3, p. 680): "It is in this battle of souls that we find the section of the trenches which is also entrusted to the German science of history. It will mount on guard. The watch word has been given by Hegel: The spirit of the universe gave the command to advance; such command will find itself blindly obeyed."

At the same time the lack of that chivalry which is so characteristic of non-totalitarian or real armies is astonishing in the totalitarian régimes. This lack expresses itself in the ostentatious feeling of superiority against weaker armies, in the undignified persecution of and brutality against defenceless groups, in the scorn heaped upon defeated foes, in the complete absence of any sense or feeling of reciprocity.

[21] *Mein Kampf,* p. 501. See also p. 734.

[22] "Totaler Feind, Totaler Krieg, Totaler Staat" in *Völkerbund und Volkerrecht,* Jg. 4, p. 139 ff. (June 1937). "Im Kriege steckt der Kern der Dinge. Von der Art des totalen Krieges her, bestimmen sich Art und Gestalt der Totalität des Staates." On the efforts of National Socialist science to arrive at a theory of international law, see the excellent book by Eduard Bristler, *Die Völkerrechtslehre des Nationalsozialismus* (Zürich, Europa Verlag, 1938), and more generally Edmond Vermeil, *Doctrinaires de la Révolution Allemande* (Paris, Fernand Sorlot, 1938).

This philosophy of war gains even greater importance by the fact that in a war, in which a totalitarian nation is involved, we do not find one nation fighting against another as equal partners within a common humanity. The totalitarian nation fights inspired by its consciousness of a unique mission, the Sendungsbewusstsein, which is fulfilled in the war and invests its fight and victory with an almost sacral character. The racial theory, as evolved by the National Socialists, amounts to a new naturalistic religion for which the German people are the corpus mysticum and the army the priesthood. The new faith of biological determinism, fundamentally opposed to all transcendent and to all humanist religion, bestows upon the people an immense strength in their permanent total war against every other conception of man, be it Christian or rational. The people now represent the Reich, the realm of salvation; the enemy represents the Gegenreich; it becomes as much of a mystical and mythical fiction as the Reich itself; only that the one is invested with all imaginable virtues, and the other with all imaginable, and sometimes even unimaginable, vices. One of the weaknesses of this position consists in the fact that, whereas the Reich is a constant factor, the Gegenreich is a variable factor, according to circumstances, the political exigencies of one moment putting up another adversary than those of another moment. Here Chancellor Hitler made a master-move by pointing out the Jews as the Gegenreich,[23] and by identifying all his enemies with Judaism. Thus he could ''unmask'' the accidental enemy of the hour, Russia and Communism, Great Britain and Democracy, France and the United States, President Roosevelt and capitalism, in short, whoever seemed to stand in a concrete situation in the way of the fulfillment of Germany's wishes, as an instrument of the devil, opposing the march towards salvation of the Reich. This attitude gives to the totalitarian politics at the same time an immense flexibility and, to its own followers, the appearance of a great persistency. Spengler had foreseen this

[23] *Mein Kampf,* p. 355. ''Among our people the personification of the devil, as the symbol of everything evil, takes on the actual appearance and figure of the Jew.'' On the mystical character of the race see Alfred Rosenberg, *Der Mythus des 20. Jahrhunderts,* p. 114: ''Heute erwacht aber ein neuer Glaube: der Mythus des Blutes, der Glaube, mit dem Blute auch das göttliche Wesen des Menschen überhaupt zu verteidigen. Der mit hellstem Wissen verkörperte Glaube, dass das nordische Blut jenes Mysterium darstellt, welches die alten Sakramente ersetzt und überwunden hat.'' See also p. 119 and 529.

attitude when he defined the new imperialist Cæsarism which he saw coming as "that type of government which, in spite of all constitutional and philosophical formulation is by its inherent nature lacking utterly in defined form." [24] This flexibility allows the substitution of one enemy for the other most abruptly and enables the leader to direct the almost mystical totalitarian hatred of his followers against the most diverse objects. That explains the startling change in the attitude of the leader of the anti-commintern pact towards communism and the Soviet Union after August 1939. Only a very short while ago the "destruction of Bolshevism was regarded as a fundamental right of the law of the nations and to this extent an elementary duty." It was proclaimed that "the Soviet Union must be expelled from the juridical community of nations" and that the League of Nations was "no community based on law any more, because it had recognized the total enemy of right and law as de jure equal." [25]

The totalitarian army gains its strength not only from the concentration of the whole national and all individual life upon war. It draws its main inspiration from the totalitarian vision according to which each individual war is nothing but a step towards imposing the new way of life upon the whole of mankind. The totalitarian army knows itself as the instrument of

[24] *Der Untergang des Abendlandes,* vol. II, p. 541. In reality the Gegenreich for National Socialism and Fascism is everything universal, everything that believes in the oneness of mankind, in common human aspirations, in a final harmony: Christianity in all its forms, liberalism, humanism, rationalism, capitalism, communism, freemasonry, the ideas of 1789, democracy, etc., even down to the Rotarian International or any of the most innocuous forms of human cooperation or civilized intercourse.

[25] E. H. Bockhoff, *Völker-Recht gegen Bolschewismus* (Berlin, Institut zur wissenschaftlichen Erforschung der Soviet Union, 1937), p. 238, 228, 99. This book had the honor of an introduction by the Reichsminister of Justice, Dr. Frank, who welcomed it as a contribution to the "struggle for the immortality and strength of the idea of right generally." In an article "Das Lebensrecht des deutschen Volkes" (*Deutsche Juristenzeitung,* 1936, p. 342) Karl Lohmann said: "As France allowed herself to conclude a pact of guarantee even with the devil himself (the author meant the Franco-Soviet pact), she created such a situation of menace that the measure of injustice necessarily came to overflow."

Mussolini also changed his attitude, this time with regard to Prussian militarism. On April 8, 1918, he said: "No man of good will, even not the last befuddled brain, could any longer believe that it is not Germany which did wish the war, and that it is not Germany which wishes to continue the war in order to reduce the whole world into a horrible Prussian barracks." *Op. cit.,* vol. I, p. 306. Six years later he proclaimed as the aim of Fascism "non la caserma Prussiana, ma la nostra caserma." *Op. cit.,* vol. IV, p. 321. Fourteen years later he introduced the Prussian goose-step into the Italian army and entirely Prussianized Italian life.

a national will, aspiring to the highest goal, to make the nation not only a powerful nation in the Bismarckian sense, but a world nation for which its world-day has arrived with the adoption of the new philosophy which is destined to become the new faith of mankind. Spengler defines as the duty of the German youth "to work out a new mode of political will and action from the newly formed conditions of the twentieth century, to bring to light new forms, methods and ideas, which like the ideas of the French Revolution and the customs of the English House of Commons will spread as models from one land to the other, until the history of the coming time progresses in forms whose beginnings will in the future be found in Germany."[26] The German master-race feels the mission of bringing the new world order. The same vision enlivens the grandiose picture which Mussolini unfolds before the eyes of Italian youth. In 1932 he proclaimed proudly: ". . . L'appello alle forze giovani risuona dovunque: la Nazione che ha precorso i tempi, anticipando di un decennio l'azione degli altri Paesi, è l'Italia," and two years later, reviewing the astonishing developments of the preceding luster, he boasted: "From 1929 until today Fascism has become out of an Italian phenomenon a world phenomenon." The essence of Fascism he defines in the same way as Spengler and the National Socialists do: as an absolute revolution against western civilization, against Anglo-Saxon liberalism and against the achievements and consequences of the French Revolution. "We represent a new principle in the world, we represent the clear-cut, categorical, definitive antithesis of the whole democratic world . . . , of the whole world, to say it in one word, of the immortal principles of 1789." From this starting point, he assured the people of Milan in a speech on October 25, 1932, of the coming world leadership of Fascist Italy. "Today, with

[26] *Politische Schriften*, p. 146. Spengler foresaw two revolutions, the revolution of the lower classes and the revolution of the colored races, class war and racial war. He regarded Prussian Germany as the savior of the "white" world against these two revolutions. But he did not foresee that National Socialist Germany which vulgarized and materialized his teaching, as they did with that of his master Nietzsche, would become itself the centre of race war and would ally herself against western civilization at one time with Japan, at another time with Soviet Russia, the representative of class war. Spengler regarded it as the hour of greatest danger for western civilization should race war and class war combine. "This possibility lies in the nature of things, and neither of the two revolutions will scorn the help of the other only because it is contemptuous of the other's bearer. Common hatred extinguishes mutual contempt." Ibidem, p. 164.

a fully tranquil conscience I say to you, immense multitude, that the twentieth century will be the century of Fascism, the century of Italian power, the century during which Italy will become for the third time the leader of mankind (la direttrice della civiltà humana), because outside of our principles there is no salvation, neither for the individuals, nor even less for the peoples.''[27]

The totalitarian philosophy of war makes wars at present fought by the totalitarian states fundamentally different from the wars of the nineteenth century and even from the first World War. It is for this reason that all analogies drawn from the first World War remain on the surface and do not touch the real problems involved in the war which started in 1939. As the Fascist imperialism of the thirties of our century is fundamentally different in its methods and aims from the liberal imperialism at the turn of the century, in spite of certain similarities and in spite of the confusing use of the same word for both—so the word war has acquired an entirely different meaning in the totalitarian states. For the liberal state war is a hateful necessity at some given moment, something anormal and even monstrous. Recognizing the interdependence and common interests of all men and the equality of all peoples, liberal statesmen strive for the creation of an international order which would eliminate war altogether. Wars exist, for the liberal conception, only as a result of the shortcomings of the political and social order which in a not too distant future may be overcome by the rational efforts of man. In the totalitarian philosophy war is the normal and welcome concomitant of all life, the supreme manifestation of vitality and virtue, an unalterable and dominating part of the whole system. Ultimately these two different and even opposite concepts of war rest upon two different concepts of the nature and destiny of man.

[27] *Op. cit.*, vol. VIII, p. 232; vol. IX, p. 32; vol. V, p. 311; vol. VIII, p. 131. The world leadership of Fascist Rome resounds in many other messages of Mussolini. ''In questo mondo oscuro, tormentato e già vacillante, la salvezza non puo venire che della verità di Roma e da Roma verrà''; or ''La Rivoluzione fascista non è soltanto il privilegio e lo sforzo dell'Italia, ma la parola d'ordine e la speranza del mondo,'' vol. VIII, p. 140, and 254. On National Socialism in this respect see: *Revolutions and Dictatorships*, pp. 352 ff., 370 f.

THE RELATION OF TOTALITARIANISM TO INTERNATIONAL TRADE AND FINANCE

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Probably the nearest parallel to what we think of as totalitarianism in the sphere of international economic relations is to be found in the policies customarily employed by belligerent nations. This amounts to saying that all belligerent countries act more or less as totalitarian states act in both peace and war. The absolute prohibition of certain imports, the forcing of means of payments that may be required to insure importation of goods regarded by the state as essential, the channelizing of international transactions by authoritarian bureaus, the control of shipping; all these are the very typification of totalitarian economic policies. They are no less the policies characteristically followed by a nation at war. There are significant differences between the two situations, perhaps the most important of which is that we ordinarily regard a state of war as only temporary, while we are not sure that totalitarianism as a form of government is temporary. Nevertheless, the best guide we yet have as to the nature and significance of totalitarian policies in international economic relations is the pattern of a modern state at war.

It is important to recognize, however, that there are widely varying degrees of administrative control over international economic affairs. That is to say, the totality of totalitarianism is far from uniform in different countries. The most extreme example of totalitarianism is probably Russia. If the present system of control in Germany were suddenly swept away, individualistic organization would be ready and able to carry on. That is not true in the case of Russia. Italy may represent a still less extreme form of totalitarianism than Germany. Even before the outbreak of war, England had moved much farther in the direction of totalitarian forms of organization than is gen-

erally recognized in this country. Under the pressure of war, both England and France have become virtually totalitarian in their international economic policies.

Whether the subject is viewed over a period of centuries or over the all too short period from the end of one war to the end of the next, the key to the study of the history of international commercial policies lies in the changing conception of the desirable balance of economic activity within the nations concerned, and the most significant feature of this is the varying importance accorded to economic and political considerations. To the mercantilists the most important objective of international commercial policy was the strengthening of the political power of the state. To the classical economists, on the other hand, the chief concern was economic strength. It is true that many of the classical economists recognized the importance of the political factor yet, despite Adam Smith's concession that "defense is of much more importance than opulence," the primary consideration in their minds was economic. And so was it in the minds of the later advocates of protection. The conflict between the protectionist and the free trader was not over ends; the issue turned on whether the economics of the one or of the other was wisely conceived to the same end.

With the development of totalitarian policies in the last few years, countries have again turned to political considerations as the basis of international commercial policy. This is frankly recognized in the remark that "cannon are more important than butter." If one chooses to interpret it so, there is little fundamental difference between this statement and Smith's that "defense is of more importance than opulence." A strange pairing this, Adam Smith and Hermann Goering! They cannot, of course, be realistically put in the same bracket, but the primary contrast between the two points of view lies in a difference of emphasis. This difference is sufficient to spread a vast gulf between the policies endorsed by Adam Smith and those of the Third Reich.

The essence of totalitarian methods in foreign trade is the substitution of authority for free individual choice in the determination of what is traded. Free choice is presumably based on price, authority on other considerations. Tariffs did not eliminate price as the force directing economic activity, though

they did restrict and alter its operation. Authoritarian methods, such as quotas and absolute restrictions, have the effect of greatly reducing the importance of price as a regulator of international trade, even though considerations of cost still impose certain ultimate limitations as to the goods that may be exchanged.

The chief importance of price lies in the fact that it is the basis of economic calculations, a means of measuring relative benefits and sacrifices. When the operation of price is circumscribed, some other basis of calculating gain and loss becomes essential. Such a basis is very hard to discover. How, for example, can one evaluate the political or other non-economic elements that enter into the determination of the policies of a totalitarian state? But unless we are able to arrive at some means of evaluating such intangibles as these, how are we to decide, for example, that measures to meet the competition of totalitarian countries are generally advantageous to the United States?

The most significant feature of the economic policies of totalitarian states is, then, their disregard of cost as the term is customarily understood. It is this fact almost solely that gives rise to the problems that confront countries competing with totalitarian states. It also points a moral as to the wisdom of attempting to meet the competition of totalitarian countries in foreign markets. The fundamental economic argument for trade was that it allowed specialization along lines of comparative advantage. With price more or less eliminated as the guide to production, economic activity no longer follows lines of comparative advantage and, therefore, the presumption in favor of trade no longer necessarily holds.

Two alternative courses for coping with the situation presented by the existence of totalitarian practices suggest themselves. The one is for the United States to meet the competition of totalitarian countries, whatever the cost. Righteous indignation, as well as national pride, suggests the adoption of such a course. But this amounts to saying that if others disregard economic considerations we should do the same—in other words, fight totalitarianism by becoming, in some measure, totalitarian ourselves. Another possibility is to let these countries go their way, hoping that the burden of selling with little regard to cost

will become so great that they will voluntarily abandon it in the course of time. In short, it is to be questioned, first, whether the old lines of trade are worth preserving by the means that would be required and, secondly, whether we may not too readily overlook the fact that there are limits to the lengths to which totalitarian states can go. Moreover, while the competition of totalitarian countries may affect to some extent the pattern of our foreign trade it is by no means certain that it will ever appreciably alter the total volume of it. That is, the loss of trade in certain foreign areas that might result from the competition of totalitarian countries may be offset by an expansion of our trade in other foreign areas.

The foregoing remarks apply more or less generally to all phases of international economic activity. It may be appropriate to refer more specifically to their bearing with respect to international finance. It is often said that international trade is the handmaiden of international finance. This signifies that the movement of goods makes possible financial transfers between countries. The reverse may also be true: international finance may be employed as a means of fostering the movement of goods from one country to another. Indeed, current proposals for trying to preserve our markets in Latin America largely turn on the extension of foreign credits to these countries. The idea seems to be that if we can have our markets in no other way we should buy them with foreign loans. Whether or not this is a good way to save our markets it would appear to be an almost certain way to lose the investments.

We must, by all means, keep our heads in meeting the problem before us. The imagined loss from any development as new and strange as this can easily be conjured into absurd proportions. There is little doubt that, at the present time, we are more likely to overestimate than to underestimate the seriousness of the threat of totalitarianism in international economic affairs.

THE ECONOMICS OF THE TOTALITARIAN STATES

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I

Totalitarian States resemble one another in the technique of their governments and in the fundamental conception that the will of these governments is not limited by any protective guards of the individual; there is no sphere of individual freedom which the government is not allowed to invade. There is not much difference between the governmental practices of Hitler, Mussolini or Stalin; they are in each case irresponsible and arbitrary. But there are no common principles underlying their attitude to economics.

They differ fundamentally from one another as to the place of economics in the affairs of the world. To Mussolini and Hitler economics are not ends in themselves, they are mere means which society needs for the pursuit of its main purpose "power," in order to dominate other societies and to grow at their expense in accordance with the law of nature which makes weaker and less numerous societies the prey of their stronger rivals. Preservation as well as growth can be safeguarded only by superior military strength and superior military valor, which is identical with superior value as such, but which can only be proven on the battlefield. War—not peace—is the true aim of nations, the natural state in which they can deploy their essential manly qualities. In these circumstances economics count only indirectly; neither the pursuit of wealth nor of welfare by individuals matters; their contentment and the satisfaction of their wants are important only insofar as they increase the efficiency for war of the national unit to which they belong. Individuals are the cells of the social body; their wealth and their strength affect it, but they have not and cannot have an independent individual existence. The pursuit of life, liberty and happiness for individuals as objectives of policy is senseless;

they can only be reached as by-products of national welfare. This view of society is highly collectivist,—in some ways more collectivist than that of the communists, for these people see society as a physical unit, an organic body made by nature not by man. Its coherence is not due to a social contract or to scientific discoveries, thanks to which a society hitherto split by class-warfare can be unified through the destruction of classes. Society and nation are really identical; a society which is not purely national is in a bad way, and a super-national society is unthinkable. The members of a healthy society are not held together by economic ties under a system of division of labor and free contracts, but by consanguity, by a force which is outside their will. In such a society into which members are born there is really no such a thing as self-determination, all is predetermined and preordained.

The collectivism of Nazism, and to a lesser degree of Facism, is however not egalitarian. Society is not composed of cells of equal value, it is highly stratified and hierarchical. Nazism objects no doubt to inequality determined by the accidents of plutocratic possession; wealth is degraded from an end to a means; it cannot be a directing or regulating instrument. Worth, not wealth, matters. Society is organized according to worth,—the true value being embodied in the members of the party who form the elite,—and the regular supply of this elite is to be guaranteed by proper breeding. Society is really a kind of pedigreed herd, where prize bulls are carefully selected, in order to improve the breed. In this respect the biological dreams of a perfect society conceived in the brain of a childless and (anthropologically) mongrel and artistic fanatic like Hitler differ fundamentally from the would-be scientific concepts of Russian communists. The levelling tendencies inherent in both doctrines are based on opposed principles. Nazism is engaged in breaking down a highly stratified pre-Nazi Society, such as existed in Germany, and to replace it by a new hierarchical order in which the party dominates; Communism has destroyed a rather primitive society and means to keep it level, but it needs a functionary class which finally may form a nucleus for permanent differentiation.

There are, however, two fundamental differences between Communism and its illegitimate offspring, Fascism and Nazism.

Communism not only accepted but stressed the rationalist doctrines of liberalism that growth and welfare of societies are regulated by immutable economic laws, the discovery of which enables man to develop a more or less perfect human society. It differs from its predecessors as to the proper nature of these laws, it agrees with them about their immanent character; they cannot be flouted, and if they are ignored, they will finally assert themselves against all blind efforts. But they can be used for speeding up development considerably once they have been properly recognized. On the assumption that Communists are in full possession of these laws their dictatorial acts are not arbitrary; they merely impose the laws of the universe on those who are too dense to discern them. The application of force implies no coercion of nature, it merely represents a short-cut, the cleaning up of the debris of past mistakes.

Fascism and Nazism laugh at such unbreakable universal laws. Their own desires, dreams and aspirations as formulated in the minds of their leaders, are the true laws of the universe. Hitler approaches world problems with the supreme contempt for facts characterizing the artistic temperament which looks upon all things as mere material for self-expression, —in this respect he may be a true artist, though his actual performance in the field of art does not reveal genius. He hates and despises communism as far as he is able to understand its abstract meaning, because it is based on reason and deduction and represents an impersonal scientific approach to objective truth by way of the intelligence and not through emotions; it would deprive a leader of men of the opportunity for glorious personal decision and purely arbitrary subjective action.

The same fundamental divergence separates Bolshevists and Nazists in other theoretical aspects, though these distinctions are frequently lost in their policies. Lenin's conception of the State was that of a Manchester Liberal. He saw in it a power of evil, the embodiment of wickedness, which must be completely destroyed as soon as a period of violent transition is over; for only when the State has completely withered, when there are no more professional soldiers and career men, liberty,—genuine liberty—is assured. Bolsheviks must use the State for the purposes of coercion during a period of transition; it always remains to them a means, and not a very clean one either; it never

becomes an end. But to the Nazis, even more so than to the Fascists, the State is the embodiment of the Nation and of every value in human affairs. Leviathan is of Divine origin, the noblest of all animals on land and sea. That it has to be fed by the destruction of millions of small fish, merely demonstrates its noble superiority.

The Communists are in some ways the heirs of liberal hedonism and eudaimonism. They know that individuals form the ultimate basis of society, that its growth and its decay depend on their physiological functions; that all organic conceptions of society which go beyond the assertion that individuals do not live isolated lives and are dependent on other individuals, are abstract, mystical conceptions, which fail to explain the relations of man to man and to the external world. The motivations and the actions of human beings embedded in masses of other human beings keep the world going and the satisfaction of their wants in the most ample ways is the supreme test of successful policies. Their happiness as conceived by eudaimonist philosophies is the ultimate end and this can only be reached by ever increasing control over the external factors of life and especially of goods. Communists are convinced that liberals and their methods are quite incapable of accomplishing these ends in which they themselves believe as fervently as ever did any Benthamites. Occasionally they had to preach the Gospel of Ascetism when political, and especially military, necessities prevented them from providing the masses with the goods they desired—but this was a mere temporary setback deliberately planned in order to attain finally the objects of hedonism. In this respect too, Communist policy occasionally runs parallel to that of the Nazis or the Fascists; when preparing for war they had to adopt the methods of war economy. But Bolshevists do not believe in war as an ultimate aim and destiny of mankind. They are by no means pacifists either in theory or in practice; but war to them is merely a means for speeding up social development and in no way a unique and most desirable means. In their analyses of society the Bolshevists are realists, they see in the "organic conception of the State" only a trick by which the wishes, hopes and ambitions of accidental leaders are substituted for the wants and the desires of the masses. They know that all aims are conceived in individual brains and all actions

are released by individual volition,—however strongly key individuals may reflect mass impulses. But their ultimate view of society is messianic. They foresee an inchoate, hazy millennium in which governments no longer function and where neither professional soldiers nor career civil servants interfere with the pursuit of human happiness. Once mankind has reached the starry milky way, order and regularity are maintained by the self-discipline of every driver; not even a traffic policeman will be needed.

Their heaven is universal, open to all, no group will be excluded from sharing it by place, space or race. It represents a curious combination of seemingly exploded religious dreams with 18th century prescriptions for happiness.

Here too, the antagonism to Nazism is evident. For Nazism does not believe in a universal heaven. Walhall, where the Germans are going to meet the Gods, is a closed preserve, the entrance to which is closely watched by pedigree sniffing sentries, who jealously guard it against an influx of non-German souls and non-Ayran spirits. They do not object to other heavens being constructed for the wants of minor nations, in fact, they want them to build their own spiritual ghettoes in the next world as a complement to what Germans are providing for them in this world, as the best method for preventing clandestine immigration. Nazism most forcefully denies the Universalism of Communism, the faith that national diversities count for little and that the same social system will finally be adopted by all races, irrespective of color, size, situation or history. Their final picture of the world is not one of universal peace in which the lion lies down with the lamb, but a permanent struggle of all against all,—not of the men within a nation for there must be no struggle within, but a struggle of nation against nation. Leviathan must learn the ways of the shark and, engaged in constant fights against other sharks, rule the seas.

II

Nazism as well as Fascism was Socialist; the latter because its most prominent member was steeped in socialist lore, the former because socialism as an emotional appeal was a valuable asset for propaganda purposes. Both were violently anticommunist; the Fascists because Communist excesses in Italy

had given them their innings and had enabled them to justify their brutal methods as self-defense; the Nazis on account of the pacifist and international character of Communist doctrine in Germany. Both movements claimed the support of the capitalist class, whilst their followers at the same time frequently showed great sympathy with Communist methods if not with Communist aims. Lenin's doctrine of the need of a ruling group of professional and professing revolutionaries was eagerly lapped up by both.

Nazism had a program with many socialist features,—though it smacked rather of petty bourgeois socialism. Fascism dropped its platform when Mussolini got control of it, for he prided himself on his activist opportunism, feeling equal to all situations and recognizing the dangers involved in a fixed program, which might hamper one after one had acquired power. Hitler, on the other hand, was greatly attracted by the catch in a program, which would appeal strongly to the German's love of system; he was not worried by fear of having to carry it out. Economics did not interest him very much, but it was excellent bait for the shoal of small fish he wanted to haul.

Both German and Italian Totalitarians reached power by the active help of influential business men who looked upon them as their political body-guard. These groups did not mind a socialist program provided it did not give power to trade unionists. They were not much worried by distant promises of nationalization, but objected to strikes and collective bargaining. In Germany they had made sure of their position by securing all important ministerial departments for their appointees; they were quite prepared to leave political and cultural jobs to their allies. This confidence seemed justified. None of the radical platform planks, like trust busting, elimination of department stores or the splitting up of big estates were carried out in Germany,—in Italy, some close settlement of lands in conjunction with the need for employing would-be emigrants were started. In neither country was property as such expropriated, though of course the property of political enemies or of Jews was confiscated by a series of legal subterfuges. The great estates which were to be divided amongst freeholders able to prove Ayran descent were left alone, partly because lands with which the owners could be compensated were not available be-

fore the conquest of Poland, but mainly because they produced the grain surplus needed for the army and the industrial population. In Italy, too, the system of private property, on which capitalism and capitalist societies was founded, was not abolished. For quite a long time Mussolini sung the praises of free initiative, he declaimed violently against nationalization and the clumsiness of state enterprise. Even after the adoption of the corporate state the structure of property was not touched. Its power-functions and its political weight were to go to the State—to which power belongs. But the corporate state was really a myth, for the state was in no way willing to hand over political functions to semi-independent self-governing corporations composed of employers and employees; it devised these bodies as state departments through which the government could control prices and wages. It did not delegate power to them; it wielded it through them. At the same time it was willing to let property owners exercise the purely business functions of the trades in which they were engaged.

Neither in Germany nor in Italy was this attitude maintained. In order to give work to the unemployed the German government became the main contractor of public works, whilst the Italian government embarked on the conquest of Abyssinia. In both cases this meant a huge rise in Government expenditure which had to be defrayed by loans, even though part of it was returned later on to the government by a great increase in tax revenue. An ever growing part of the national income was directed in both countries to such non-productive investments; road building, conquering of colonies and armaments do not differ much from one another when viewed as public works. The spending of this money gave work to the unemployed and the spending of their wages started a secondary demand for goods. But the capital goods created by this expenditure did not yield an addition to the national income. The process could be kept going as long as sufficient national income could be gathered through taxes and loans to expand the production of unproductive plants. Once a point had been reached where additional resources were no longer available, the government would have to print money, however cleverly they might camouflage it and once more go in for inflation. Totalitarian governments fight shy of this danger and they have discovered a way for postpon-

ing it. Inflation implies the haphazard indiscriminate expropriation of owners of fixed interest-bearing securities in favor of the Government and of other debtors; it equally despoils the beneficiaries of fixed salaries. It can be avoided by an all-round expropriation of income carried through a general reduction of the standard of living.

This can be done by effective price control. Nearly all prices of essential commodities are fixed by the central authority; the sellers can neither exploit the needs of consumers due to physical scarcity nor hand over goods to them in response to offers of more cash, resulting from money and credit expansion. The execution of old and the making of new agreements at stable prices can be enforced and the gap in control which might arise by producers withholding goods is efficiently bridged by the imposition of compulsory quota deliveries. In fact a considerable part of the national output is requisitioned at fixed prices, whilst another part is bought outright for government consumption at government rates. Producers cannot hold back goods and elude maximum prices. On the other hand, consumers cannot make a successful attempt at inflating prices by competitive offers of more cash. They are rationed and rationing means registration. They do not pay for rationed goods with money so much as with money-coupons; they occasionally may over-pay a money-coupon when taking it over from other coupon holders, which merely increases the money supply of the coupon sellers; it does not change the price of rationed goods expressed in money. The spread of rationing narrows the field of free commodities and the play of competitive money factors steadily becomes less sensitive. The supply of money may increase when the government gives additional orders, but the recipient must plough back most of it into their works after they have returned to the government an ever-increasing proportion of their income in the form of taxes. What they can spend on free goods is strictly limited by the quantity and the quality of such goods. They may use a surplus for buying non-rationed goods, and the prices of these goods may be inflated, but if important they will be controlled and rationed. As a flight of capital is next to impossible under a system of exchange control, the only thing surplus money can do is to flow into securities, and here again the market is closed and prices are fixed.

The more the government spends on contracts, the greater is the amount of spare money finally available. The more rationing is extended to new commodities and the more they are reduced in quantity or quality, the greater is the amount returnable. It finally finds its way to the government in the form of loans, for as new capital issues are regulated and as the purchase of real estate depends on permits, the only possible evasion is hoarding which is not safe, as it can be easily investigated when large income tax payers are concerned. Thus an ever increasing part of everybody's income is swallowed up by the government in taxes and loans, whilst the extension of the hours of labor frequently increases efforts. In other words, a growing percentage of private income is taken over by the state and used for non-productive purposes. The state provides the cash with which most of the nation's output and services are bought, after having collected it by taxes and loans from its members. It has not deprived them of their property in the physical or legal sense of the word, but it determines the use to which they can put it. It gives orders directly or indirectly to the entrepreneur, prescribing the nature of the product, the price at which it must be sold, the customers to whom it must go. It allocates labor and raw material to them on equally regulated terms. The farmer cannot grow what he likes, pay the wages he fancies and use his own judgment in fertilizing his farm. He is directly, or indirectly, compelled to grow what the government wants, to furnish to it a definite part of his crop at fixed prices, and he must not retain for his own use more than a specified portion. Labor can no longer bargain collectively and insist on change in labor conditions by means of a strike. The workers are functionaries of the government, a class of minor civil servants. They can no longer decide where they want to work and at what they want to try their hands, but they have to take the posts the government assigns to them regardless of residence and family ties. They, like farmers and entrepreneurs, are members of a huge government army in which they hold different rank but where there is no longer any freedom of choice. They are allowed to earn wages and to make profits, limited of course by price control, but they are not free to spend their earnings according to their whims. The capitalist is more and more subject to government direction. Exchange control has stopped most of the

gaps through which capital trickled abroad. It provides a government with an additional regulator for price control. All foreign exchange is accumulating in the hands of the government which allot it to the various groups who have to make payments abroad. It fixes a price at which it is willing to release exchange and has thus been able to reduce the burden of foreign debts quite arbitrarily by compelling foreign creditors to accept a great reduction of their claims when converting them into their national currencies. It can shift purchases from one class of goods to another and from one country to the other. It can even discriminate between individual firms, allocating exchange to one and withholding it from its rivals. As all foreign exchange must be handed over to it, it gets complete knowledge of all transactions abroad, of markets as well as of customers. Whenever it needs additional exchange, it can force exports by making producers sell abroad at a loss, and by compensating them from profits made on domestic markets. Thus a completely government-run system of economy has grown up in which private institutions can only work by permission, though sometimes some of them can exercise pressure on the government. A hierarchical collectivist society has been established which can be best described as 'barrack-room socialism'; it has transferred the principles of army collectivism to the civil life of the entire nation. It is not a form of state capitalism,—for the state has not taken over the capital; it leaves it in the hands of its owners, whilst it is reducing and destroying its value and it may finally wipe it out in part or totally by a levy on property. This system was not deliberately planned. The men who created it believed in the old capitalist regime which they hoped to save from socialization by putting the Nazis into power. But they could not determine the ultimate aims of policy; the Nazis did that. They made the capitalists furnish the means for accomplishing their ends; they were far too ignorant to have done it without their help. Their experts had to devise a series of emergency measures, each of which was a step nearer the collectivist system they abhorred and they had to recognize that they could not run a free economy within a coercive political system, the controllers of which flatly deny the importance of the economic outlook and degraded economics to the role of mere hand-maiden to their wider policies. Capitalism has not been

destroyed by confiscation, it is being hollowed out by requisition and regimentation. It is becoming a mere shell which may finally crack.

Even now it is not so very different from the system actually in force in Russia, not of course from Communist philosophy, for Russia too is drifting towards a hierarchical inegalitarian system of collectivism run by a bureaucracy. Were these bureaucratic posts to become permanent, as is almost inevitable when they are held by a closely knit party group, it would soon resemble the collectivist hierarchy developing in Germany in which the management of property as well as of persons is becoming a mere function carried out by orders from a centralized government. When this process has been completed, the economics of all totalitarian States can be described in a few words.

DISCUSSION OF TOTALITARIANISM

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A DEFINITION of totalitarianism would have been a useful hitching post for this discussion. Unfortunately that invaluable reference work, the *Encyclopaedia of the Social Sciences,* does not help us, for it jumps from Torts over Tory Party to Totemism; it does not even give as cross reference "For Totalitarianism, see Torts and Totemism." The four papers we have heard did not give us a uniform definition; but they did with unqualified unanimity give us a uniformly bad opinion. From every angle they condemned the culprit; not a word could be said in his favor; he is, as someone once said of Bimetallism, a worse cure for a bad disease. If totalitarianism were local or promised to be ephemeral, we might dismiss and condemn it as a temporary aberration of harassed minds. But its widespread existence and its apparent tenacity compel us to take a different viewpoint, and since the four previous speakers have all thrown bricks at totalitarianism I am obliged, although I agree with most that has been said, to play the part of devil's advocate, and see what can be said in extenuation or defence.

The first obvious thing to be said is "Why should we be surprised at the advance of totalitarianism?" There is nothing final in economic or political institutions, and no one can assume that the capitalistic democratic state is the last word, a final perfection which will endure for ever. If it cannot "deliver the goods," it may cease to be capitalistic—as we understand that term—and stop being democratic. There is nothing sacred about our ideas of freedom of enterprise and thought, or of areas fenced off from state intervention. They are ideas which could become popular under certain favorable circumstances, but in the long span of human history they have been abnormal rather than normal. They are especially the product of that strange and even unique period in history, the Hundred Years'

Peace which stretched from 1815 to 1914. During that century the world in general was demobilized, and there was no Armageddon. During the same period there was a great expansion of settlement, migration, trade, and production of wealth, and under such comfortable circumstances the doctrines of laissez-faire, tolerance, and individual freedom could breathe. Yet even in that century both the political ideal of the do-little state and the political technique of democratic methods were challenged.

That brings me to my second comment. We think of totalitarianism in terms of its concept of an all-controlling state and of its rough-shod methods of asserting control. But what is the alternative? May I plead for some fractional labels, such as "Halfitarianism," "Three-fourthsitarianism," and so on, and consider how such labels describe states which have not been in the pillory this afternoon? The Hobbes view of the Leviathan state, with no limits to its authority, is not incompatible in whole or in part with democracy; and even the Locke view of the limited state does not prevent democracy from advancing the line to which the state can move. One highly democratic country (Australia) took over "a new province of law and order" when it began the state regulation of wages; another democratic country attacked one of man's oldest habits when it imposed prohibition; and during the depression of the early nineteen-thirties democracies sent governments into power with overwhelming majorities, almost blotted out opposition parties, and then called for far-reaching action from their rulers. Democratic dictatorships and democratic totalitarianism have not been quite unknown during the last decade. There may have been little of the roughshod technique, though there has been some; and that technique as practiced in Germany, Russia, and Italy, is perhaps not so much totalitarian as German, Russian, and Italian. If the western nations, with their greater resources and deep-rooted individualist traditions, were forced to move far toward totalitarianism, we must at least try to understand why other countries went all the way.

THE NOVELTY OF TOTALITARIANISM IN THE HISTORY OF WESTERN CIVILIZATION

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If totalitarianism were synonymous with dictatorship, the subject of the following remarks would be pointless. For dictatorship is no novelty of the present age. Twenty-five centuries ago it was exemplified in the Greek matrix of our distinctively Western civilization by tyrants like Periander of Corinth and Peisistratus of Athens. Two thousand years ago, when the seat of that civilization had moved from Greece to Rome, dictatorship in the tradition of Alexander the Great was revitalized by Julius Caesar; and thenceforth it was the ideal and usually the practice of long lines of Caesars—Constantine, Justinian, and all the emperors of Byzantium down to 1453; Charlemagne, Otto, and all the Holy Roman Emperors down to 1806; Ivan, Peter, and all the autocratic Russian Tsars down to 1917. The brilliant age of Humanism, five hundred years ago, was an age of Italian despots. The pregnant age of the Enlightenment, two hundred years ago, was an age of enlightened despots—Frederick of Prussia, Charles of Spain, Joseph of Austria. Only one hundred and twenty-five years separate us from the great dictator Napoleon Bonaparte; only seventy, from the little dictator Louis Napoleon. While on the frontiers of Western civilization—in Latin America and the Balkans—the common garden variety of dictator has continued to flourish like any hardy perennial.

Dictatorship or despotism or tyranny, call it as one may, is, then, a constant, or at least a recurrent, *motif* in the whole history of Western civilization. It is as much a characteristic of bright as of dark ages. It is equally apparent in formative Greek times, in fully developed Christian eras, in epochs of rationalism or reaction. In fact, it is as constant or recurrent as that other and contradictory *motif* in the history of our civilization, the *motif* of individual liberty and representative

government. For this, too, let me remind you, is no novelty. It has had repeated and manifold expression in Athenian democracy and Roman republic, in feudal contracts, in medieval communes, guilds, and estates, in Italian and German city republics, in Swiss cantons, in English and American and French revolutions, in the numberless written constitutions and elected parliaments of the last two hundred years. Liberty and tyranny, democracy and dictatorship, such contradictions and antitheses occur and recur throughout the whole history of the Occident as if to prove to every generation the truth of Aristotle's ancient dictum that an excess of the one automatically produces the other.

But though dictatorship of itself is no novelty, a new genus of dictator has arisen in our generation. Previously the successful despot came of good family and usually possessed excellent education and military fame. Such was the case alike with the Greek tyrants of antiquity and with the Spanish-American dictators of the last century. Alexander the Great had been tutored by no less a scholar and scientist than Aristotle, and it would be hard to find any petty Latin-American dictator who had not commanded at least a regiment and fortified himself with the positivist philosophy of Auguste Comte. Certainly the Italian despots of early modern times were men of culture as well as prowess, and the royal despots of the eighteenth century were products of the most selective eugenics and the most meticulous education. Even Napoleon Bonaparte was a scion of the Corsican aristocracy and a graduate of the aristocratic French military school of Brienne; he was the leading general before becoming the outstanding dictator of his age.

In contrast with the quality and training of earlier dictators, note who and what the present ones are. There is Benito Mussolini, whose father was a blacksmith, whose education was obtained in a minor normal school, whose career was that of an unsuccessful elementary-school teacher and a second- or third-rate journalist, and whose military service was brief and inconspicuous. There is Joseph Stalin, generated by a peasant shoemaker, dismissed from a seminary and further schooling at the age of seventeen for irregularity in conduct and discipline, self-trained in the strong-arm arts of highway robbery and factory disorder, and as a convicted criminal relieved of any military

service in the World War. There is Adolf Hitler, son of a minor Austrian customs-official, with a minimum of formal schooling and a maximum of frustrated efforts at hack work in painting and drawing, and with a curious record of having fought for four years in the German army without rising above the rank of corporal. In fine, not one of these world-shaking despots of our day comes of what we would deem distinguished antecedents; not one of them has been well educated or gained military repute.

How has it happened that such persons could become dictators? It is, I submit, precisely because they have come from the masses rather than from the classes and could therefore more readily get the ear of the masses. For the masses count nowadays as never before, and this is the first and most fundamental of the novelties which distinguish contemporary dictatorship and the totalitarianism accompanying it.

Though European civilization has been inhospitable to any rigid caste system, such as that in India, it has traditionally harbored classes and class distinctions and been respectful of them. Even Christianity, which so long and so effectively molded our civilization, was not conducive to the wiping out of classes or to the abating of honors paid them. The equality which it preached was an equality of souls before God, an equality in dignity of human personality. It was not an equality of minds or of purses, of talents or of occupations. And the precept of *noblesse oblige* was an essentially Christian consecration of the classes to service of the masses, with implicit reminder to the masses that they in turn should respect the classes. How universal and persistent has been such a conception of the relationship between classes and masses is evidenced by the fact that in every European parliament which functioned at any time between the Middle Ages and the middle of the last century persons who could vote belonged to special classes and were presumed to represent the inarticulate and non-voting masses. Right up to the present day, moreover, the surviving congresses and parliaments, chosen though they are by universal suffrage, are replete not with peasants, mechanics, stevedores, and other members of the ordinary populace but rather with landlords, bankers, lawyers, and similar persons of substance and education from among the restricted upper and middle classes. Until

recently, at any rate, cats could look at kings but they did not aspire to be kings.

Now a radical change is occurring. Cats want to be kings and, through sheer press of numbers, some of them actually become kings. It is an aspect of the new and widespread "revolt of the masses," which the Spanish philosopher, Ortega y Gasset, described a few years ago with prophetic insight. Perhaps "revolt of the masses" is too journalistic and strong a term. "Self-conscious emergence of the masses" would probably be more accurate. At any rate, a significant transformation has come over the masses in recent times. It represents a response in part to such external stimuli as popular compulsory schooling and popular "yellow" journalism, and in part to such stimuli within the masses as trade-unionism, the coöperative movement, and the propaganda of Marxian socialism. But underlying all these stimuli and providing a favorable field for them is the intensive industrialization of our day, with its attendant dislocation of the masses. Always before, the masses have been relatively settled in body and mind. Now they are migratory, nomadic, almost Gypsy-like. They move from country to country, from country to city, from city to city, from one tenement to another. This means that a vast and growing number of them are what the French call *déracinés,* uprooted from ancestral soil, from established habit and tradition, from personal responsibility. They experience, furthermore, an extraordinary economic insecurity, which is apt to produce an extraordinary psychological maladjustment. And huddled and agglomerated as they are in overgrown cities, they are peculiarly susceptible to demagogic purveying of easy panaceas, especially by those who assure them that they shall no longer be mere minor cogs in machine-industry but major wheels in affairs of state, full-fledged comrades suitably shirted.

All this appeals with full force to the youth among the masses. Youth is notoriously more adventurous than age, more idealistic and less stable, more open to suggestion and less hesitant about acting upon it; and the present generation of youth, while provided with unusual opportunities for schooling and recreation, for sport and physical development, finds its opportunities for remunerative employment sharply curtailed or indefinitely postponed. No wonder that contemporary dictator-

ship is a mass-movement, and especially a youth-movement. Hitler, Stalin, and Mussolini were all under forty-five years of age when they became dictators, and of their several movements, youth has been the vanguard and spearhead.

There is another novel trend of our age which affects the masses and forwards dictatorship—and totalitarianism. It is a decline of traditional religion and an obscuring of religious values. In the past every civilization has been builded upon a particular religious profession and upon the popular *mores* emanating from it; and to this generalization our occidental civilization is no exception. The background of all that is distinctively Western is Graeco-Roman paganism, and central to the spacious foreground is Judaeo-Roman Christianity. To countless generations of our ancestors, regardless of nationality or class, the Christian faith was the impelling ideology and the Christian ethic the standard of conduct. We must admit that this holds true no longer, though I wonder if we fully appreciate how very recently the change has occurred. For two centuries some of the classes, especially intellectuals, have been repudiating our common religious heritage, but indifference or hostility of the masses towards it, of the rural as well as the urban masses, is a strictly contemporary phenomenon. How this has come about, I shall not here attempt to suggest. I merely remark the fact, which seems to me self-evident, and pass on to an important consequence. No man, whether he be Western or Eastern, lives by bread alone. Everybody must have faith, a faith in some mysterious power outside of one's self, a faith attested by feelings of reverence and expressed in external acts and ceremonials. When a man loses faith in one religion, he naturally attaches himself consciously or unconsciously to another object of worship. It may be worship of Christ; it may be worship of totem or fetish; it may likewise be worship of science or humanity—provided these concepts are written in his mind with capital letters.

In the present crisis, when the historic Christian faith of the Western masses grows cold, a kind of religious void is created for them. But inasmuch as any such void is unnatural and eventually unendurable, the masses promptly seek to fill it with some new faith. This they hardly find in "humanity" or "science," which are too abstract and intellectual and nowadays

a bit stale. They find it rather in materialistic communism or in nationalistic deification of blood and soil. Indeed, we miss a main point about the nature of contemporary communism and nationalism and their relationship to contemporary dictatorship if we overlook the essentially religious element in them and the essentially religious appeal they make. It is as high priests of novel and fervent religions that the dictators of today appeal to the masses. It is as recent converts that the masses respond enthusiastically and fanatically to the appeals of dictators.

So far I have dwelt upon certain features of dictatorship which are peculiar to our age—its origin in the masses rather than the classes, its close association with youth, and its personification of popular religious change. Let me now speak of totalitarianism, which is something more than dictatorship and which, unlike dictatorship, is a brand-new event in the history of Western civilization.

From time immemorial Western civilization has developed under what may be termed a pluralism of sovereignties. Whatever might be the theory at any given time, the practice for centuries has been to distribute state-powers between a central government and a congeries of local governments. To the latter usually belonged the care of roads, the administration of common justice, the raising and support of militia, the dispensing of poor relief. The central government had enough to do to maintain a court and an army. For centuries, moreover, state-powers, whether exercised locally or centrally, were limited by the powers accorded by custom or charter to semi-public, semi-independent corporations like church, nobility, guild, or university. Most educational and charitable work was the concern of church, not of state. Industry and commerce were long regulated less by state than by craft- or merchant-guild. Even so-called absolutist states respected privileges of nobility and of universities and recognized the rights of parents and a large degree of family autonomy.

Several developments in modern times have contributed to the breakdown of that pluralism: the gradual secularizing of religion and the universities; the rise of liberalism with its atomizing of society and its warfare on privileged corporations; the French Revolution; and, most recent and fateful of all, the Industrial Revolution, which has created unprecedented eco-

nomic and social problems and at the same time furnished the state with unprecedented opportunities and incentives for expanding its activities. The outcome is today manifest: a centralizing in the state of all sorts of functions—military, judicial, administrative—commercial, industrial, agricultural, labor—educational, charitable, health, even family; and an exercising of this totality of functions less and less locally and more and more by the central national government. It is what the French call *étatisme,* and some measure of it is evident today in all industrialized countries, whether republican or monarchical, democratic or dictatorial.

It is conceivable that a democracy, already far advanced in *étatisme,* might become outright totalitarian; it would merely utilize an elected majority instead of a dictator to gather all power in its hands and crush minority dissent. Actually, however, this has not yet happened, nor is it likely to happen. Democracy may break down, as it did in Italy and Germany, but it is an ensuing dictatorship which completes the transition to totalitarianism. Naturally so, because democratic government is apt to be cumbersome and sluggish; and the greater and more numerous are the functions which it is called upon to perform, the more cumbersome and sluggish it becomes; with the result that persons who are eager for quick, drastic action and who demand efficiency at any cost lose patience with democracy and yearn for a strong man—a dictator. Responsibility which should be theirs close at hand they nonchalantly shove off on a fabulous figure afar. With rapture they listen to the ambitious demagogue and welcome the advent of dictatorship, and it is their expectation of prompt and manifold miracles to be wrought by it which renders dictatorship the natural vehicle for totalitarianism.

I am not contending that the recent trends which I have mentioned—the increase of *étatisme,* the loosening hold of traditional religion, the self-conscious emergence of the masses—render totalitarianism inevitable. I am neither of Calvinist nor of Marxian proclivities, and I am sceptical of any historic determinism. Totalitarianism is not universal, and I doubt if it ever will be. Not all the emerging masses in every country are in revolt. Traditional religion may be in decline, but surely it has not lost all influence everywhere. In many countries, too,

there are still enough limitations and divisions of the functions and powers of government—enough pluralism—to check indefinitely the drift toward totalitarian dictatorship. Besides, wherever this kind of dictatorship has triumphed, it has been the achievement not of a deliberative majority but of an active minority, a result of pressure politics; and in each instance it has followed something like a national cataclysm: war and despair in Russia, defeat and humiliation in Germany, disorder and disillusionment in Italy. However, a believer in democracy should not be unduly optimistic. The new sort of totalitarian dictatorship is an extraordinarily substantial affair, and, once it is established, it is not likely to be ended except through another and very terrible cataclysm. On the other hand, a democracy may presumably be carried into totalitarian dictatorship through cataclysmic misfortune exploited by a demagogue, a militant minority, and pressure politics. Which only heightens the danger of a great international war as the biggest possible gamble on cataclysms.

I have now reached the point where, with your continuing indulgence, I may summarize what I believe to be the specific novelties of dictatorial totalitarianism.

First, it is really totalitarian. It monopolizes all powers and directs all activities of individuals and groups. It subordinates to itself all economic, religious, and educational institutions and policies. It levels classes and restricts or suppresses the liberty of family and person. It leaves no room for the free play of individual wills and recognizes no utility in free inquiry. On the contrary, it has a passion for making everyone conform to the will and thought of the governing party and dictator. Alike to Communists and to Fascists, the state is omniscient and infallible as well as omnipotent. In all these respects the Russia of Stalin is totalitarian as the Russia of the Tsars never was, the Germany of Hitler as no divine-right monarchy of Hohenzollerns or Habsburgs ever was.

Second, it commands and rests upon mass-support. It is no affair of an aristocratic class or a military caste. It is frankly for and by the half-educated and half-propertied lower middle class and upper proletariat. It springs from and returns to the great median of what are truly the masses, and everyone above or below who is not sycophantic is suspect and liable to liquida-

tion. Previous régimes may have been aristocratic or plutocratic, but at least they had a tolerance for the masses which these, under the new dictatorship, seldom evince for the classes. And despite past apologies for Fascism in Italy and National Socialism in Germany on the ground that they were last-ditch defenses against the anti-capitalist Communism of Russia, time has already shown that their real defense has been piece-meal capitulation to the same socializing of goods and the same levelling of persons, which latter is a levelling down and not a levelling up.

Third, totalitarian dictatorship is maintained, and its overthrow rendered unusually difficult, by novel and marvellously effective agencies of popular education and propaganda. The radio and amplifier only date from after the World War; the cinema, from just before that war. The production of wood-pulp paper and large-scale cheap journalism arose in the 1890's, and compulsory school attendance began only in the 1880's and then only in a very few countries. Now all these agencies are perfected, and ready to be seized and used by a dictator. Once in power, the dictator immediately decrees a monopoly and extension of state schools. Private and religious schools are suppressed, and in the public schools, which have been greatly extended not only in Hitlerian Germany but more sensationally in Russia under Stalin and in Italy under Mussolini, every child is forcibly fed with mental pabulum nicely calculated to make him an unquestioning devotee of a given brand of totalitarian despotism. Everyone learns to read, of course; that is a favorite boast of contemporary dictators. It does flatter the masses, and it probably renders them more gullible than they would be if they could not read. For what they later read—and hear—is as closely prescribed or censored as the content of their elementary schooling. The whole publishing business is "coördinated"—this is the technical term—with the state, and there is nothing to read of which the state disapproves. Cinema and radio are outright state monopolies; the one flicks at the popular eye, the other dins into the popular ear, only what the régime adjudges advantageous to itself; and Hitler with an amplifier is more frightful—and far more effective—than was Napoleon without one.

Again, totalitarianism has an almost irresistible allure—it

moves people—by reason of the emotional and essentially religious spirit which its leading apostles have infused into it. Communist Russia and Nazi Germany are churches as well as states, and they are churches not old and conventional but pristine and full of missionary zeal. Their mythologies (that is, their ideologies) are new; their banners and rituals and slogans are new. No wonder that the number of their first-generation converts is incomparable. Masses of people who have lost contact with old gods want new, and now they get them—a Dionysus-like tribal god of blood and soil, a Lucretian god of fatalism and materialism. It has been fondly imagined that these gods and their respective worshippers would be quite irreconcilable, and undoubtedly the one has gained adherents by fulminations against the other: racialists against communists, and communists against racialists. One can still perceive jealousies between them, and some continuing differences, for example, about the correct manner of saluting one's fellow believers, about whether you style them "Proletariat" or "Volk," and about what saints are most potent, whether Marx and Lenin or Wagner and Nietzsche. Recent events have disclosed, nevertheless, that peculiarities of mythology are not necessarily divisive when it comes to arranging for a partition of foreign-mission fields. Perhaps, after all, these peculiarities are more fanciful than real. Perhaps, having performed the high service of enlisting the masses in the worship of totalitarian dictatorship, they may coalesce and syncretize into a neo-paganism analogous to that of primitive times.

Furthermore, totalitarian dictatorship has evolved a new pattern of methods and techniques. Behind a mask of plebiscites, popular elections, and occasional assemblings of a so-called parliament—which listens and applauds but doesn't really parley—the government actually functions through and with a single political party which comprises a minority, usually a small minority, of the nation, but which is more or less hand-picked, severely disciplined, and equipped with a monopoly of the means of influencing public opinion and enforcing the will of the dictator. The party permeates and ultimately dominates the army, the courts, the ubiquitous secret police, the schools and universities, the newspapers, radios, and cinemas, the telegraphs and telephones, the pulpits and rostrums, the youth organiza-

tions, indeed all organizations, whether economic, social, or cultural. And a Russian Tsar, a Louis XIV or an Alexander the Great might well envy the speed and effectiveness with which dissenters are liquidated, doubters purged, and suspects gathered into concentration camps.

Still another novelty of totalitarianism is its exalting of might and force, not only as means to an end—there is precedent aplenty in Western history for that—but as an end in itself. In earlier and less totalitarian days, a despot who got rid of a foe or appropriated some neighbor's land went to considerable trouble, as a rule, to justify his action on conventional moral grounds. Now the totalitarian despot is hardly expected to offer any explanation at all, and when he does, it bears no trace of the Decalogue or the Sermon on the Mount. It suffices to echo the more up-to-date Nietzschean and pseudo-Darwinian principles that patience is a vice and that progress depends on a struggle for existence and *Lebensraum* for the fittest. Or it may suffice to recite the Marxian creed that proletarians have to fight for what they get and they are always right. The fact remains that brute force is boastfully invoked alike in the internal affairs and foreign policies of the totalitarian state. Force against Jews and Christians! Force against domestic critics! Force against Czechs and Albanians, Poles and Finns! The exalting of force and terrorism does not signify merely the immoral doctrine that the end justifies the means. It signifies an utter denial of any moral law superior to the might of dictators.

In sum, the dictatorial totalitarianism of today is a reaction—nay more, a revolt—against the whole historic civilization of the West. It is a revolt against the moderation and proportion of classical Greece, against the order and legality of ancient Rome, against the righteousness and justice of the Jewish prophets, against the charity and mercy and peace of Christ, against the whole vast cultural heritage of the Christian Church in middle ages and modern times, against the enlightenment, the reason, and the humanitarianism of the eighteenth century, against the liberal democracy of the nineteenth. It repudiates all these major constituents of our historic civilization and wars to the death on any group that retains affectionate memory of them. In Russia Christians and in Germany Jews are first

gotten rid of. Presently it will be the turn of Christians in Germany and Jews in Russia, for if you are to erase the most constant memory of the Western mind you have to destroy Judaeo-Christianity both in its roots and in its flowers. Meanwhile the attack is pressed on other fronts—against champions of representative government and personal liberty, against counsellors of moderation, reason, or peace, against advocates of ordinary decency. For with Western civilization in any of its moral or cultural features, the proclaimed objectives of a Stalin or a Hitler are in flat contradiction. Stalin would build on its ruins an utterly new civilization, godless, materialistic, communistic. Hitler would resuscitate from under the ruins that tribal civilization of virility and valor which is imputed to pre-Christian aboriginal Teutons. Backward to primitive barbarism or onward to conscienceless utopia, that seems to be the choice of goals of the great contemporary revolt.

In the present crisis one may easily be tempted to pessimism. The factors which now make for totalitarianism, for its embodiment in dictatorship, and for its warfare against historic civilization seem so obvious and so overwhelming that the whole resulting process assumes in one's mind the nature of a cosmic drama—remorseless and inevitable. Yet in my philosophy at any rate, there is nothing absolutely inevitable. And in conclusion I would suggest two further antidotes for undue pessimism. First is the reflection that totalitarian dictatorship is a novelty of the last two decades only, a mere moment in the twenty-five centuries of Western civilization, and what has long endured is likely to outlast any untested novelty. Second is the recognition of the resourcefulness as well as of the inertia of the strange creature we call man. His inertia and submission have repeatedly brought him some sort of dictatorship and slavery. But his resourcefulness and rebelliousness have as often put him in the way of liberty, equality, fraternity. So the tide of human affairs ebbs and flows, for man belongs no less with the angels than with the beasts.

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BLAISE DE VIGENÈRE AND THE "CHIFFRE CARRÉ"

CHARLES J. MENDELSOHN, Ph.D.*

(Communicated by Waldo G. Leland)

ABSTRACT

The aim of this paper is to trace the development of cipher-writing from the very simple devices of the Ancients to the so-called Chiffre Indéchiffrable, generally called the Vigenère Cipher. The elementary substitution of one letter of the alphabet for another, used by Cæsar and Augustus, was succeeded by the use of arbitrary characters for the letters. Since there is no limit to the possible number of arbitrary characters, while the number of letters in the alphabet is fixed, the practice later arose of having variants for frequently occurring letters. This practice is known as early as 1401. Leone Battista Alberti (+ 1472) devised a system of multiple alphabets—several equivalents for each letter employed in a fixed order—and at the same time found a way of discarding the cumbersome arbitrary characters and of returning to the use of ordinary letters of the alphabet. The Abbot Trithemius (+ 1508) made a series of tables of alphabets to be used in a fixed order. Giovanni Battista Belaso or Bellaso (otherwise unknown) about 1550 added the principle of the *key*, thus rendering possible the use of the alphabets in an arbitrary order. Giovanni Battista Porta in 1563 mixed the order of letters in the alphabet. Blaise de Vigenère, Bourbonnois, was a man of varied attainments. In 1586 he combined the table of Trithemius, the key of Belaso and the mixture of letters of Porta into what is generally called the Vigenère Cipher or Chiffre Indéchiffrable. In all the literature of cipher I know of only one writer who has correctly described Vigenère's cipher, and that was well over one hundred years ago. Vigenère does not claim to have invented the cipher, nor does he consider it the inexpugnable cipher par excellence. He gives, with one exception, a substantially correct account of its history, although he overflows with rancor against those who preceded him in the field. He does add a new method for keying the alphabets. This—his only original contribution to the cipher—has never been credited to him, but arises again in the nineteenth century as an invention of that day. The cipher enjoys an exaggerated reputation for safety, but still lives—in fact, of all ciphers except the very simplest, it is certainly the most widely known.

BLAISE DE VIGENÈRE, Bourbonnois (1523–1596), enjoyed a wide fame in his day as a scholar and prolific writer.[1] Reared

* Dr. Mendelsohn died on September 27, 1939. He was born in Wilmington, N. C., in 1880, graduated A.B. (1900), Ph.D. (1904), from the University of Pennsylvania, was tutor (1905) and instructor (1907) in Greek in the College of the City of New York and a member of the faculty in the history department since 1920. During the war he was engaged in foreign language work in postal and newspaper censorship and was made Captain in the Military Intelligence Division of the General Staff of the Army in 1918 in charge of decipherment of German codes. After the war he continued research in codes and prepared studies in diplomatic code for the U. S. Government.

His associate and friend, Lieut. Col. William F. Friedman, Sig. Res., Principal Cryptanalyst in the Office of the Chief Signal Officer of the U. S. Army, has graciously corrected the proof of this article.

1 Niceron, *Memoires* (Paris, 1731), Vol. XVI, p. 26, ff.; Vol. XX, p. 94, f. Niceron's chief source of information is Du Verdier, *Prosopographia*, Vol. III, p. 2570.

in luxury as a nobleman's son, he was taken to Paris at the age of twelve, and after four or five years of study there went to court and was employed under Bayard, First Secretary of State under Francis I. In 1545 he attended the Diet of Worms with the French envoy De Grignan, and after some time spent in travel—to Rome among other places—became secretary to the Duc de Nevers. Upon the death of the Duke and his heir Vigenère devoted himself to the study of Greek and Hebrew. In 1566 he became secretary to the Embassy at Rome, and the three years spent there were most important for the cryptographic studies which had been stimulated by his first visit. Later he served as Secrétaire de Chambre under Henry III. His death of cancer in 1596, after a long siege of suffering, is recorded in the *Journal de Règne de Henri IV*.

Italy was the seat of the great development in cryptography which the Renaissance produced. In Rome Vigenère associated in both state and church circles with men versed in the subject, and, even though we cannot suppose that they taught him all their secrets, he acquired a wealth of knowledge in the field. One episode of this period is worth repeating here, both for its own sake and for the light that it sheds upon Vigenère's environment in Rome.

Messere Paulo Pancatuccio of Volterra, Vigenère informs us,[2] had been appointed by the Pope to employ his skill in reading captured documents written in cipher, an occupation "in which, as a matter of fact, he was reasonably well versed, and in which he performed some lesser miracles of a minor kind." Certain "bons compagnons" of the party of Francis, however, who wished to humble his pride a bit, prepared a letter in cipher, addressed it to "Monsieur le Baron de Grissemenisse, Grand Superintendant, etc.," endorsed it "most important," contrived to have it come into the hands of the unfortunate Pancatuccio, and waited for nature to take its course.

Pancatuccio readily deciphered the opening words, which were intentionally written in a very simple cipher. They called attention once more to the extreme importance and to the secrecy of certain matters that were in the hands of "Monsieur l'Illustrissime Dinero, qui commande a tout comme vous sçavez." His curiosity now thoroughly aroused, Pancatuccio deciphered

[2] *Traicté des Chiffres* (Paris, 1586), p. 197.

further and read, as he was intended to: "O poor wretched slave that you are to your decipherments, on which you waste all your oil and your pains, what does it profit you to eat out your heart in the quest of these vain curiosities, presuming by your laborious researches to be able to attain to the discovery of the secrets of others, which are reserved to God alone? Come, use your leisure and your work in the future for things more fruitful, and stop uselessly frittering away your time, one lone minute of which cannot be bought back by all the treasures of this world. Put matters to the test now, and see if you can get at the meaning of one little letter of what follows here."

What followed was written in an extremely complicated cipher which, without giving the message it contained, Vigenère thoroughly describes. His inability to read it casts no reflections upon the skill of poor Pancatuccio.

Niceron lists no less than twenty-two printed works of Vigenère, and mentions several others concerning which he is unable to say whether they were published or not. Rarely has opinion concerning the merit of an author so greatly changed as in the case of Vigenère. Thevet and Du Verdier were extravagant in their praise.[3] The former says: "Of Sieur de Vigenaire [sic!] I shall not say much, since his works suffice to show the excellence of his art, his ease in handling the French language, and the serious studies which he faithfully pursued, so that he is esteemed by all who are not jealous of the honor due to men of intellect." And Du Verdier goes much further: "Of all the nurslings of the muses to whom France has given birth, Vigenère has spoken so well that he may be considered to have closed the door, both in excellence of language and in learning, upon all who may come hereafter." From such verdicts it is a far cry indeed to the biographer in the Nouvelle Biographie Générale, who tells us that Vigenère's works have fallen into a "just oblivion." Justly or unjustly, the works of Vigenère, with one exception, are rarely spoken of today. How many know even the names of his "Entrée d'Henri III à Mantoue," of his "Traité des Comètes, avec les causes et les effets"; of his "Discours sur l'Histoire de Charles VII"; or of his translations of De Fulstein's "Chroniques et Annales de

[3] A. Thevet, *Vies des Hommes Illustres* (Paris, 1584), p. 619 b; Du Verdier, *Bibliothèque Française*.

Pologne,'' of Cæsar's Commentaries, of Plato, Cicero and Lucian on Friendship?

There is, however, one work of Vigenère which is constantly mentioned by writers in its field—the ''Traicté des Chiffres ou Secrètes Manières d'Escrire,'' published at Paris in 1586 and again (unchanged) in 1587. Almost no work on cryptography of the present day fails to mention this compendium of the subject. It is no exaggeration to refer to it, not, to be sure, as the Cryptographer's Bible, but certainly as one of the books that comprise that canon.

Unfortunately, however, Vigenère's treatise will bear comparison with the Bible in another respect. It will be brought out in the present study that, while often spoken of, it is generally left unread. As a result the author has widely been credited with something which he neither produced nor claims to have produced, while on the other hand he has failed to come down to posterity as the author of what he actually did bring forth.

The Traicté des Chiffres, as has been stated, is a compendium of the cryptographic knowledge of Vigenère's day, or at least of so much of it as was allowed to get out from behind locked doors. It is something besides that too, for its author was permeated with gnostic philosophy, and takes many opportunities to digress from the main theme of his work into side issues. The Traicté covers nearly seven hundred pages, but almost one hundred must be read before the author seriously gets under way with his subject, and even when he does another philosophical digression soon follows. ''All the things in the world,'' he tells us,[4] ''constitute a cipher . . . all nature is merely a cipher and a secret writing. The great name and essence of God and his wonders, the very deeds, projects, words actions and demeanor of mankind—what are they for the most part but a cipher? For beneath a pretended and hypocritical appearance of zeal, piety, devotion, charity, kindness, good nature, probity and other upright, holy and praiseworthy purposes which may be compared to the characters of a double cipher . . . men conceal for themselves a secret meaning of their malicious courage, their hatreds, rancor, treason, unfairness, avarice, ambition, bloodthirstiness and vengeance; and the alphabet in which these things are written is known only to Him who penetrates all

[4] Vigenère, *op. cit.*, p. 53 b; p. 54 a, b.

disguises." This philosophy, at one point at least, betrays the author into a blunder, as we shall see later.

In the world of ciphers Vigenère's name is connected almost exclusively with one cipher of the endless number described in his treatise—the so-called "Vigenère cipher," "Chiffre carré," or "Chiffre indéchiffrable." It is usually described in a manner closely corresponding to the following account which I have taken, with one insignificant change, from A. de Grandpré:[5]

The French diplomat Blaise de Vigenère devised a table which . . . is a rearrangement of another table designed by the Italian physicist, Porta.

	A	B	C	D	E	F	G	H	I	J	K	L	M	N	O	P	Q	R	S	T	U	V	W	X	Y	Z
A	a	b	c	d	e	f	g	h	i	j	k	l	m	n	o	p	q	r	s	t	u	v	w	x	y	z
B	b	c	d	e	f	g	h	i	j	k	l	m	n	o	p	q	r	s	t	u	v	w	x	y	z	a
C	c	d	e	f	g	h	i	j	k	l	m	n	o	p	q	r	s	t	u	v	w	x	y	z	a	b
D	d	e	f	g	h	i	j	k	l	m	n	o	p	q	r	s	t	u	v	w	x	y	z	a	b	c
E	e	f	g	h	i	j	k	l	m	n	o	p	q	r	s	t	u	v	w	x	y	z	a	b	c	d
F	f	g	h	i	j	k	l	m	n	o	p	q	r	s	t	u	v	w	x	y	z	a	b	c	d	e
G	g	h	i	j	k	l	m	n	o	p	q	r	s	t	u	v	w	x	y	z	a	b	c	d	e	f
H	h	i	j	k	l	m	n	o	p	q	r	s	t	u	v	w	x	y	z	a	b	c	d	e	f	g
I	i	j	k	l	m	n	o	p	q	r	s	t	u	v	w	x	y	z	a	b	c	d	e	f	g	h
J	j	k	l	m	n	o	p	q	r	s	t	u	v	w	x	y	z	a	b	c	d	e	f	g	h	i
K	k	l	m	n	o	p	q	r	s	t	u	v	w	x	y	z	a	b	c	d	e	f	g	h	i	j
L	l	m	n	o	p	q	r	s	t	u	v	w	x	y	z	a	b	c	d	e	f	g	h	i	j	k
M	m	n	o	p	q	r	s	t	u	v	w	x	y	z	a	b	c	d	e	f	g	h	i	j	k	l
N	n	o	p	q	r	s	t	u	v	w	x	y	z	a	b	c	d	e	f	g	h	i	j	k	l	m
O	o	p	q	r	s	t	u	v	w	x	y	z	a	b	c	d	e	f	g	h	i	j	k	l	m	n
P	p	q	r	s	t	u	v	w	x	y	z	a	b	c	d	e	f	g	h	i	j	k	l	m	n	o
Q	q	r	s	t	u	v	w	x	y	z	a	b	c	d	e	f	g	h	i	j	k	l	m	n	o	p
R	r	s	t	u	v	w	x	y	z	a	b	c	d	e	f	g	h	i	j	k	l	m	n	o	p	q
S	s	t	u	v	w	x	y	z	a	b	c	d	e	f	g	h	i	j	k	l	m	n	o	p	q	r
T	t	u	v	w	x	y	z	a	b	c	d	e	f	g	h	i	j	k	l	m	n	o	p	q	r	s
U	u	v	w	x	y	z	a	b	c	d	e	f	g	h	i	j	k	l	m	n	o	p	q	r	s	t
V	v	w	x	y	z	a	b	c	d	e	f	g	h	i	j	k	l	m	n	o	p	q	r	s	t	u
W	w	x	y	z	a	b	c	d	e	f	g	h	i	j	k	l	m	n	o	p	q	r	s	t	u	v
X	x	y	z	a	b	c	d	e	f	g	h	i	j	k	l	m	n	o	p	q	r	s	t	u	v	w
Y	y	z	a	b	c	d	e	f	g	h	i	j	k	l	m	n	o	p	q	r	s	t	u	v	w	x
Z	z	a	b	c	d	e	f	g	h	i	j	k	l	m	n	o	p	q	r	s	t	u	v	w	x	y

To use the table, a letter or word is taken as a key.
If I wish to encipher

"JE PARS CE SOIR POUR NICE" with the key "ain," this will be my arrangement:

JEP ARS CES OIR POU RNI CE
ain ain ain ain ain ain ai

I take row J and column A in the table, and at their intersection I find j; row E and column I give me m where they cross. I continue in this way with each letter to the end of my despatch, so that my cryptogram will be

j m c a z f e m f o q e p w h r v v c m.

[5] *Cryptographie Pratique* (Paris, 1905), p. 12, ff.

The use of this cipher has been so widespread that no writer on cryptography can ignore it. A few examples of its use in modern times may be mentioned. It formed one of the official methods of communication of the Confederate States of America;[6] it was used by the Duc d'Orléans and figured in court in France in 1899;[7] and to my own knowledge it was for a while employed by one of the minor Bureaus of the United States Government during the World War. For a long time it was regarded as extremely safe and the term "chiffre indéchiffrable" was applied to it, and even credited to Vigenère himself.[8]

A few lines on this point may be quoted from Dlandol's "Le Contr'espion ou les Clefs de toutes les Correspondances secrettes," published at Paris in 1794—two hundred years after Vigenère's death. In chapter VI, which is headed "Le Chiffre par excellence," Dlandol says: "This cipher has been so named because it combines the greatest number of advantages to be desired in a secret correspondence. It would combine all these qualities without exception if it were not that it is a trifle slow of execution; but it atones for this inconvenience by its incredible safety. This safety is such that the whole world would recognize it if the word agreed upon as key by the correspondents is kept secret; a letter might be shown to the whole world, and not a soul would be able to read it."

As given by Vigenère himself[9] the cipher differs in important respects from the method just described.[10] The table as

[6] W. R. Plum, *The Military Telegraph During the Civil War in the United States* (Chicago, 1882), Vol. II, p. 45.

[7] Bazeries, *Les Chiffres Secrets Dévoilés* (Paris, 1901), p. 51.

[8] Lange et Soudart, *op. cit.*, p. 29; A. Figl, *Systeme des Chiffrierens* (Graz, 1926), p. 81.

[9] *Op. cit.*, p. 50 b.

[10] I. B. Lindenfels, *Den Hemmelige Skrivekonst* (Copenhagen, 1819), reproduces the table correctly. See his text and especially the note on page 171. Selenus (*i.e.* Augustus, Duke of Brunswick-Lueneburg), *Cryptomenytices et Cryptographiae Libri IX* (Lueneburg, 1624), gives the table correctly (p. 262) with credit to Vigenère, whom he uses freely and admiringly throughout his work. He uses the table, however, for a cipher somewhat different from that which we are discussing.—Lindenfels, so far as I can ascertain, is the only writer who accurately presents both the table and the directions of Vigenère. In the form given here from De Grandpré the table may be found in almost any general treatise on cryptography. The following may be noted: Dlandol, *op. cit.*, opposite p. 96, has a table with an extremely slight and inconsequential variation.—J. L. Klueber, *Kryptographik* (Tuebingen, 1809), says, p. 147, that "each horizontal row must differ in the order of its letters from all the other rows. The manner in which they differ is arbitrary." This description is not according to Vigenère, and despite its variation from that commonly given Klueber gives the table in the usual form.—P. Lacroix, *La Cryptographie* (published anony-

		O	P	Q	R	S	T	V	X	A	B	C	D	E	F	G	H	I	L	M	N
		E	*F*	*G*	*H*	*I*	*L*	*M*	*N*	*O*	*P*	*Q*	*R*	*S*	*T*	*V*	*X*	*A*	*B*	*C*	*D*
O	*E*	a	b	c	d	e	f	g	h	i	l	m	n	o	p	q	r	ſ	t	v	x
P	*F*	b	c	d	e	f	g	h	i	l	m	n	o	p	q	r	ſ	t	v	x	a
Q	*G*	c	d	e	f	g	h	i	l	m	n	o	p	q	r	ſ	t	v	x	a	b
R	*H*	d	e	f	g	h	i	l	m	n	o	p	q	r	ſ	t	v	x	a	b	c
S	*I*	c	f	g	h	i	l	m	n	o	p	q	r	ſ	t	v	x	a	b	c	d
T	*L*	f	g	h	i	l	m	n	o	p	q	r	ſ	t	v	x	a	b	c	d	e
V	*M*	g	h	i	l	m	n	o	p	q	r	ſ	t	v	x	a	b	c	d	e	f
X	*N*	h	i	l	m	n	o	p	q	r	ſ	t	v	x	a	b	c	d	e	f	g
A	*O*	i	l	m	n	o	p	q	r	ſ	t	v	x	a	b	c	d	e	f	g	h
B	*P*	l	m	n	o	p	q	r	ſ	t	v	x	a	b	c	d	e	f	g	h	i
C	*Q*	m	n	o	p	q	r	ſ	t	v	x	a	b	c	d	e	f	g	h	i	l
D	*R*	n	o	p	q	r	ſ	t	v	x	a	b	c	d	e	f	g	h	i	l	m
E	*S*	o	p	q	r	ſ	t	v	x	a	b	c	d	c	f	g	h	i	l	m	n
F	*T*	p	q	r	ſ	t	v	x	a	b	c	d	e	f	g	h	i	l	m	n	o
G	*V*	q	r	ſ	t	v	x	a	b	c	d	e	f	g	h	i	l	m	n	o	p
H	*X*	r	ſ	t	v	x	a	b	c	d	e	f	g	h	i	l	m	n	o	p	q
I	*A*	ſ	t	v	x	a	b	c	d	e	f	g	h	i	l	m	n	o	p	q	r
L	*B*	t	v	x	a	b	c	d	e	f	g	h	i	l	m	n	o	p	q	r	ſ
M	*C*	v	x	a	b	c	d	e	f	g	h	i	l	m	n	o	p	q	r	ſ	t
N	*D*	x	a	b	c	d	e	f	g	h	i	l	m	n	o	p	q	r	ſ	t	v

The Chiffre Carré, as given by Vigenère, *Traicté des Chiffres*, Paris, 1586, p. 50 b.

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mously, Paris, 1858), gives no table but his description follows Dlandol (pp. 110, ff.).—F. W. Kasiski, *Die Geheimschriften und die Dechiffrierkunst* (Berlin, 1863), at the end of the book.—Fleissner von Wostrowitz, *Handbuch der Kryptographik* (Vienna, 1881), p. 8.—A. Kerckhoffs, *La Cryptographie Militaire* (Paris, 1883), p. 25, f.—H. Josse, *La Cryptographie et ses Applications à l'Art Militaire* (Paris, 1885), p. 47.—Marquis de Viaris, *L'art de Chiffrer et Déchiffrer les Dépêches Secrètes* (Paris, 1893), p. 33.—Valerio, *De la Cryptographie* (Paris, 1893), p. 7.—Carmona, *Tratado de Criptografia* (Madrid, 1894), p. 57.—A. Collon, ''Étude sur la Cryptographie,'' in *Revue de l'Armée Belge*, Sep.-Oct. 1900, p. 89.—F. Delastelle, *Traité Élémentaire de Cryptographie* (Paris, 1902), p. 34.—A. Langie, *De la Cryptographie* (Paris, 1918), p. 245.—E. Droescher, *Die Methoden der Geheimschriften* (Leipzig, 1921: Heft 3 of the ''Frankfurter Forschungen, Neue Folge''), says that the order of the letters *in the squares* may be chosen at will, but once chosen must be adhered to throughout. The letters in the alphabets at the top and at the side are to be in normal order. His table is in the common form.—M. Zanotti, *Manuale de Crittografia* (Milan, 1928), pp. 41–43.—Général M. Givierge, *Cours de Cryptographie* (2nd Ed., Paris, 1932), p. 52.—Lange et Soudart, *Traité de Cryptographie* (Nouvelle Ed., Paris, 1935), p. 29.—Generale L. Sacco, *Manuale de Crittografia* (2nd Ed., Rome, 1936), pp. 25–26.

given by him and here reproduced has two lines of capital letters at the top and two columns at the side, the second line at the top and the second column at the side being printed in red ink. The first line and the first column read

O P Q R S T V X A B C D E F G H I L M N

while the second line and the second column read

E F G H I L M N O P Q R S T V X A B C D.

These differences from the usual table are explained and greatly accentuated by Vigenère's directions:[11] We are to use the table "taking the capitals which run across the top for the message to be conveyed and those that run perpendicularly downward at the left for the keys. I have put two rows of capitals here, one black, the other red, *to show that the alphabets of the text as well as of the keys may be transposed and changed at will* (italics mine) to keep knowledge of them from all except one's correspondents."

Let us analyze the differences between the common form of the cipher and the method actually given by Vigenère. If we denote our clear text by capitals and our cipher text by small letters, then

(1) In the cipher as usually described, if $C = f$ we know that $A = d, B = e, L = o,$ etc. In other words, if in any one alphabet we know the cipher equivalent of one letter, we know the whole of that cipher alphabet.

(2) If we mix the order of the letters in the key (side) alphabet, that will change the *order* of the cipher alphabets but not the alphabets themselves.

(3) If we change the order of the clear text (top) letters, our cipher alphabets are changed: if now, as before, we have $C = f$ we are still ignorant of all the other equivalents in that cipher alphabet.

In the usual description of the cipher, Vigenère is given credit for what we have here noted under (1), while in point of fact he describes what we have given under (3), and even goes a step beyond that since he *mixes the order* of the letters both at the top and at the side. Any cryptographer will admit the decided difference in difficulty of solution of these two types of

[11] *Op. cit.*, p. 49 b.

cipher. The former is no longer regarded as in any degree difficult; the latter is by no means easy.

I can find no passage in Vigenère's treatise to justify the statement that he considered this method the inexpugnable cipher par excellence. In discussing the cipher [12] he does, it is true, use strong language: "It is impossible . . . that any guess, no matter how subtle, or any patient and unceasing labor, or any trick or effort at decipherment, could ever manage to solve this." These words are emphatic enough to have given rise to the term chiffre indéchiffrable, and it may be due to them. The fact is, however, that Vigenère is very free with his claims of strength for many different ciphers, as a few examples will show. In describing a cipher which designates the letters by indicating the place where the letter in question occurs in a book agreed upon as the key, he says: "This cipher is truly impregnable unless the key is furnished; for how would it be possible to make any conjectures about it?" [13] Of a cipher that employs only four letters of the alphabet he says: "Truly any one not in the secret would be hard put to it to find out or understand anything about it; and he would stand as little chance—less, indeed—of succeeding in deciphering it." [14] Again he tells us of another type of cipher: "So that I do not see how any intellect, be it ever so subtle, could manage to solve it unless the key is furnished"; [15] and a cipher of still different type he calls "the cipher superlative, above all others, since it is beyond all doubt or suspicion of having anything concealed under that which meets the eye." [16] Other examples of similar claims might be offered, but those given will suffice to show that Vigenère never considered the chiffre carré as pre-eminently the "chiffre indéchiffrable."

We come now to the question of authorship. The chiffre carré has been claimed for Johannes Tritheim as well as for Vigenère,[17] and in Germany it is more generally known as

[12] *Op. cit.*, p. 48. Vigenère is referring to that form of the cipher usually ascribed to G. B. Porta, which, however, he identifies with the chiffre carré.

[13] *Op. cit.*, p. 209.

[14] *Op. cit.*, p. 248 b.

[15] *Op. cit.*, p. 276.

[16] *Op. cit.*, p. 279.

[17] *Cf.* Klueber, l. c., who says the method was first published by Tritheim and later by Vigenère. Fleissner von Wostrowitz, l. c., does not mention Vigenère. Carmona, l. c., decides in favor of Tritheim, as does Collon, *op. cit.*, pp. 78–83. Droescher, l. c., says the method, *i.e.* the common procedure, is named after Tritheim, but is certainly older. To these may be added W. Suess, "Ueber Antike Geheimschriften-

Tritheim's cipher. General M. Givierge[18] speaks of the table (in its usual form as given above) as "that which is called in France the 'square table of Vigenère,' after the French cryptographer of the 16th (misprinted 17th) century whom certain authors regard as the inventor of a system long considered very difficult to decipher, although others assign its authorship to Tritheim." General Givierge adds: "We shall not stress these questions of authorship in cryptography; the secrecy that ordinarily surrounds the practical manifestations of this science is eminently favorable for the competition of authors."

In this connection we shall have occasion to consider the part played by Tritheim (1462–1516) as well as the contribution of another outstanding scholar of the Renaissance—G. B. Porta (1535–1615).

Johannes Tritheim, whose short life was filled with labor, was one of the very great scholars of his day.[19] The two monasteries—Sponheim and Wuerzburg—where, successively, he pursued his studies, were visited by a constant throng of learned men who came from all quarters in search of his counsel. His historical writings and the fact that, great scholar as he was, he has been accused of falsifying history, do not here concern us, and it is sufficient to mention his two works on cryptography —the "Steganographia," about which a storm of controversy has raged, and which caused its author to be charged with necromancy, and the "Polygraphia," first printed in 1517, which is directly connected with our subject.

The contributions to science of Giovanni Battista Porta (or

methoden und ihr Nachleben" (in *Philologus*, 78, 1922, pp. 142–175), who says that in Germany the system is called after Tritheim, but does not accept him as the inventor. He leaves the question of authorship unanswered. The other writers mentioned in note 10 all assign the cipher to Vigenère except Givierge who refuses to decide between Tritheim and Vigenère, and Kasiski, who names no author.

[18] *Op. cit.*, p. 52.

[19] A good short sketch of Tritheim or Trithemius will be found in Henry Morley's Life of Cornelius Agrippa von Nettesheim (London, 1856), Vol. I, pp. 213, ff. A very detailed account appears in Heidel, *Johannis Trithemii . . . Steganographia* (Mainz, 1676), pp. 1, ff. For other accounts see M. Ziegelbauer, *Historia Reo Literariae Ordinis S. Benedicti,* revised by O. Legipontius, Augsburg and Wuerzburg, 1754, Vol. III, pp. 217, ff.; I. Silbernagl, *Johannes Trithemius,* Landshut, 1868, revised edition, Regensburg, 1885; W. Schneegans, *Johannes Trithemius und Kloster Sponheim*, Kreuznach, 1882; M. Marcuse, *Ueber den Abt Johannes Trithemius,* Halle, 1874. None of these, with the exception of Heidel, goes deeply into Tritheim's cryptography, and Heidel is concerned mainly with the "Steganographia." The best commentary on the "Polygraphia" is still Tritheim's own explanation of the various books, and the notes of Glauburg in the Cologne edition of 1571.

della Porta) require no comment here. It is well to call to mind, however, that because of his "Magia Naturalis sive de Miraculis Rerum Libri IV" (1558), and—more pertinent to our subject—his contributions to cryptography, he was, like Tritheim, accused of witchcraft. He was, in my opinion, the outstanding cryptographer of the Renaissance. Some unknown who worked in a hidden room behind closed doors may possibly have surpassed him in general grasp of the subject, but among those whose work can be studied he towers like a giant. In the case of the cipher we are discussing, Porta's contributions are limited though significant; in the general field of cryptography, however, there is nothing before the nineteenth century to compare to the "De Furtivis Literarum Notis" (1563).

To arrive at an answer to the question of authorship we shall follow the devices that combine to make up the "Vigenère cipher" through their historical development. It will be well, however, first to see what Vigenère himself says on this subject, and what claims, if any, he makes for himself. He says:[20]

> First I shall present the cipher which I for my part credit to a certain Belasio of the suit of Cardinal Capri, since he was the first, so far as I know, who used it, and gave it out before the year 1549[21] when I was first at Rome. For Baptista Porta's book, already mentioned, in which he inserted this cipher without mentioning whence he got it, was not published until 1563;[22] and at that time he must have predated it by four or five years because it was not on sale until 1568. The Grand Vicar of St. Peter mentioned above, who enriched the cipher with many devices, went back none the less to its very beginning and assigned its invention to Belasio so far as the keys are concerned. The shufflings of the letters pertain to no particular time, as I have said; and there are tables in the Polygraphia of Tritheim, although they are not put to good use. If the keys consist of several words at once, as is the practice among those who employ whole verses of Virgil and other poets, they are a bit troublesome and likely to become confused so as to overwhelm one's correspondent when he attempts to read them. I have accordingly—and this I claim as my own invention—introduced the device of making all the letters depend upon one another, as if they formed a chain or were

[20] *Op. cit.*, p. 36.—*Ibid.*, p. 186, he again says that "Neither Tritheim nor Belasio is the original author of this device."

[21] Belaso's booklet is dated 1553. In his dedicatory preface he says the system had been printed the preceding year, "on a single page, without the directions for use," but that the supply had been exhausted. Any instructions that Belaso gave concerning his cipher prior to 1552 must have been either oral or in manuscript.

[22] *I.e.* Porta's *De Furtivis Literarum Notis.* The book bears the imprint "Neapoli MDLXIII," and I can find no intimation, outside of this remark of Vigenère's, that it did not actually appear in that year.

connected like masonry—a result obtained by the way in which they are placed and in which they follow one another, as will be shown later. This table, besides, whether one credits it to Belasio or to Baptista Porta, belongs, as a matter of fact, neither to the one nor the other, but goes back to the Ziruph of the Iezirah which is composed of 22 letters. Twenty, however, or even less are sufficient for a chiffre carré, so that the rest may be saved to be used as nulls or for a secret meaning of their own. This has hitherto been noticed by no one so far as it is possible to learn.

Certainly this passage is very far indeed from setting up a claim to the invention of the chiffre carré or of the use of a key in connection with its use: credit is given to cryptography in general, to Tritheim, Belasio and Porta, and to the Hebrew philosophical treatise, Sepher Yezirah, of unknown authorship. Only the automatic system of keying is claimed by Vigenère as his own. We shall now endeavor to see how accurate Vigenère's account is.

The cipher consists essentially of two parts—the alphabets and the device of the key. We shall follow their history separately.

The representation of one letter of the alphabet by another goes back to the well known devices of Julius Cæsar and Augustus. Cæsar, we are told, replaced each letter by one three letters removed from it in the alphabet, while Augustus—less cryptographically minded—merely substituted for each letter that one which followed it alphabetically.[23] The system we are discussing differs from the methods of Cæsar and Augustus in that it uses now one and now another of several alphabets.[24]

While it is difficult, especially because of the secrecy that has necessarily surrounded cryptography, to ascertain with certainty the origin of any given system, we can at least follow out available records as far as they will take us.

Who was the first to represent a letter of clear text by more than a single cipher equivalent in the same message, when he did it, or where, we do not know. The first instance of the prac-

[23] Suetonius, *Cæsar*, LVI; *Augustus*, LXXXVIII. *Dio Cassius*, XL, 9. Of the cipher of Cæsar mentioned by Aulus Gellius, XVII, 9, and said to have received treatment by Probus, nothing is known. If Aulus Gellius is right, the cipher must have been something more than the simple one mentioned by Suetonius and Dio Cassius.

[24] *Polygraphiae Libri VI*, Oppenheim, 1518, folio o, i. Since this edition, which, incidentally, is the earliest printed book devoted to cryptography, has no page numbers, and references to it are accordingly inconvenient, I am adding in each case a reference to the corresponding passage of the edition published at Cologne in 1571. In the present case the page of the 1571 edition is 551, f.

tice of which we have any knowledge occurs in a cipher of Mantua of the year 1401 recorded by Aloys Meister,[25] and contains four separate arbitrary characters to represent a, with

Mantua.

2.

Cum Simeone de Crema. Zifra ultima 1401.

a	b	c	d	e	f	g	h	i	k	l	m	n	o	p	q	r	s	t	u	x	y	z
ç	y	x	v	t	s	r	q	p	o	n	l	m	k	j	h	g	f	e	d	c	b	a
2				4									8						ff			
3				ς									9						Ј			
+				4									Ŀ						~			

Alphabet with variants for vowels, in use at Mantua in 1401, from A. Meister, *Anfaenge der Modernen Geheimschrift*, Paderborn, 1902, p. 41.

the same number for e, o, and u. This alphabet is reproduced here. The practice is frequent in Italian cryptography of the 15th century and is extended to consonants as well as vowels, so that in a partially reconstructed Florentine cipher of 1414 [26] we find two or more equivalents for all the vowels and for many of the consonants. Examples of a similar practice can be cited from Siena in 1433 [27] and from Milan in 1448.[28]

In these ciphers the variants are used in no fixed order: the effort is merely to confuse the would-be decipherer by lowering the number of repetitions of the individual characters employed. The first use of a cipher in which multiple equivalents for the characters are used systematically is the disc cipher described in the treatise on cryptography written by the famous architect, poet and scientist, Leone Battista Alberti (1404–1472). This treatise exists in several Latin Manuscripts. An Italian translation was published by Cosimo Bartoli in 1568, and the Latin text by Aloys Meister in 1906. An English translation of Alberti's description of his cipher, by the present writer, has also appeared.[29] Since Alberti died in 1472, the cipher necessarily antedates that year.

25 *Anfaenge der Modernen Geheimschrift* (Paderborn, 1902), p. 40.

26 *Ibid.*, p. 49.

27 *Ibid.*, p. 51.

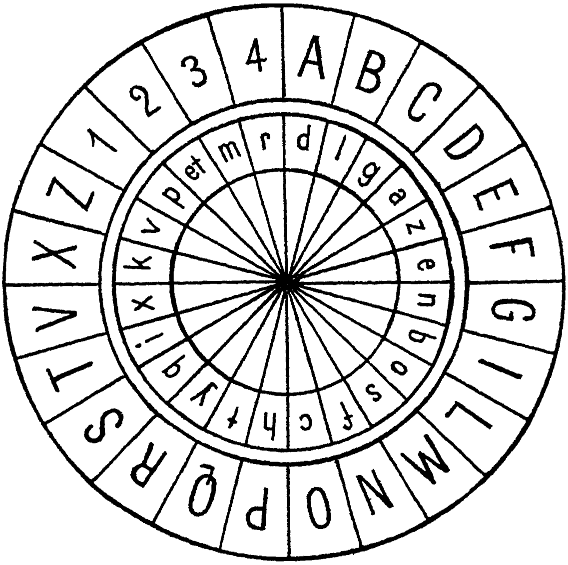
28 *Ibid.*, p. 30.

29 *Cf.* L. B. Alberti, *Opuscoli Morali*, Tradotti e Parte Corretti da M. Cosimo Bartoli, Venice, 1568, pp. 200, ff.; Aloys Meister, *Die Geheimschrift im Dienste der Paepstlichen Kurie*, Paderborn, 1906, pp. 25, ff., and 125, ff.; *U. S. Government Signal Corps Bulletin*, Washington, Oct.–Dec. 1937, p. 62. A note on the bibliography of the treatise, by the present writer, is to be published in *Isis*.

Alberti is very proud of his achievement—as pleased with his effort in a new field as a child with a new toy. We can hear him chuckling as he writes: "No cipher that one could use is quicker, none will be written with greater ease, none more secret can be devised if you do not know the keys arranged between me and my correspondent. I make the assertion that this method will nullify all the clever and cunning tricks of all the men in the world, all the perseverance of the most careful, and all their painstaking investigation, their skill and their effort. Industry will not enable any one except the initiated to learn anything that we write in this cipher. Moreover any scribe that you might call upon will be able to write the cipher at your dictation in ordinary well-known characters, but will know nothing of what he has written, and another will just as readily read the characters of any message sent to you from some station abroad. You will understand everything thoroughly, while he whom you get to read the message aloud to you will not comprehend one syllable. I am justified in calling this cipher worthy of kings, since they can make use of it with very little work and without the help of a decipherer initiated into its mysteries. I hope this little treatise will be safeguarded by my friends and not be allowed to get among the common herd of the unskilled and be profaned. The cipher is worthy of a prince, and should be dedicated to the execution of affairs of the greatest moment. . . . It is my wish that this cipher, most convenient and beautiful as it is, and marvelously capable of contributing to the safety of the state and carrying on affairs of the greatest moment, be dedicated as a consecrated offering to posterity. By using the tables as prescribed, people who are under siege or widely separated will be able to tell one another what is needed without sending written messages, but by an arrangement or movement [metuve of Meister's text is meaningless; I read motuve and have translated accordingly] of lights or smoke. If you will understand and consider how valuable this device is, you will congratulate me. What, I pray you, is more worthy of admiration than to have a method by which, even against the will of the enemy, and from the greatest distance, you may send word and report on the condition of affairs, saying what you wish done and what the prospects are?"

Alberti describes a device of two concentric discs, one moving

against the other, and each containing an alphabet, one normal, the other with the letters in mixed order. The device is here reproduced. The clear text letters are taken from one disc, the



Leone Battista Alberti's Disc Cipher, as reconstructed by A. Meister, *Die Geheimschrift im Dienste der Paepstlichen Kurie,* Paderborn, 1906, p. 28.

cipher equivalents from the other, and the setting of the discs determines the alphabet to be employed.[30] The essential advances of this cipher upon the simple schemes attributed to Cæsar and Augustus are three in number: (1) The cipher text appears in a mixed alphabet so that the identification of one letter does not identify the rest; (2) The setting of the discs may be changed at will, bringing a new cipher alphabet into use; (3) The cipher is written in ordinary letters of the alphabet and not in arbitrary characters such as squares or crosses, and is suitable for dictation or sending by fire signals, etc.[31]

Vigenère knew of Alberti's work, but he had no admiration for its author. In speaking of a form of cipher with which we are not here concerned, he says: [32] "It is sufficient to have mentioned

[30] Alberti ingeniously associates with this device a rudimentary code system of which he was very proud, and which was certainly in advance of the cryptography of his day. This feature of his system, however, does not come within the scope of the present paper.

[31] *Cf.* the reprint in Meister, *Geheimschrift im Dienste,* etc., p. 138.

[32] *Op. cit.,* p. 209.

it in passing, because I have seen some who prize it very highly, just as a certain Leon Alberti of Florence prizes his own cipher, 'worthy,' he says, 'of an emperor or of a king.' " It should be noted that in the passage cited above Alberti actually calls his cipher "worthy of kings" and "worthy of a prince," so that Vigenère is quoting accurately.

From Alberti to Tritheim's alphabets is only a step. Instead of the disc devices we find in the "Polygraphia" a square like that of Vigenère, but without the mixed and variable alphabets at the top and at the side. In addition, Tritheim gives what he calls the "expansion" of the table: afraid, perhaps, that the table will not present the matter clearly enough, he shows in full the results produced by using each alphabet of the table in turn.

At first glance, the Tritheim alphabets appear to mark a step backwards, for instead of the disc and the mixed alphabets of Alberti we find a set of normal alphabets, the first beginning with A, B, C, etc., the second with B, C, D, etc., and so forth. This collection of alphabets, however, which he calls "Tabula recta" is followed by another set with the letters in inverted order, and called "Tabula aversa."[33] Tritheim then goes a step further, giving samples of other collections of alphabets the arrangement of which he describes as follows: "After the development of the Tables come certain 'orchemata' (*i.e.* 'jumpings' or 'skippings') in which there is not only a shifting of all the letters but also an orderly jumping of one letter over another." These alphabets run, *e.g.*, A, C, E, etc., or A, D, G, etc., and may be almost infinitely varied. Let us listen for a moment to Tritheim's own words:[34] "A wonderful leaping takes place in the working of the orchema. It contains twenty-four alphabets. In the first no letter is skipped, in the second, one, in the third, two . . . and so on to the twenty-fourth alphabet in which twenty-three letters are skipped. . . . This writing by means of 'orchemata'—these jumpings and leapings—is the safest method of all, because it affords a greatly varied shifting of letters instead of a uniform change."

The reference to "twenty-four alphabets" is not accurate—what is meant is not twenty-four alphabets but twenty-four orchemata, each containing twenty-four alphabets. That this

[33] Tritheim, *Polygraphia*, 1518, folio o, iij, ff.; 1571, following p. 554.
[34] *Op. cit.*, 1518, folio B, iiij, verso; 1571, p. 71.

is the meaning is definitely proved by the short sample message which Tritheim gives in both cipher and clear:[35]

Cipher: p r d h i f y q y n b d y e n z h k t s x s o l i n q
Clear: HUNC CAVETO VIRUM QUIA MALUS EST

Twenty-four alphabets are used in this message. Tritheim does not give them, but I have recovered them from the message itself. All are based on the same "orchema," *i.e.* the interval between two consecutive letters is always the same. The first alphabet is a, c, e, i, l, n, p, r, t, x, z, b, d, f, h, k, m, o, q, u, s, y, w; the second, b, d, f, etc.; and similarly for the rest.

Although Vigenère constantly quotes Tritheim, he is none too gentle in dealing with him. In fact, he occasionally arouses the suspicion of jealousy, as when, for example, he says of a certain kind of cipher[36] that "the aforementioned Tritheim gives no sign of ever having had even the smell of it." So too he says of the alphabets we are discussing that they are not put to good use,[37] and that the orchemata especially are confusing and inconvenient as against Vigenère's own method in which one single table does the whole trick.[38] All the more striking then is the acknowledgment of Tritheim as the immediate source of Vigenère's tables.

Iacopo Silvestri, whose printed work, *Opus Novum,* followed Tritheim's Polygraphia by six years, has a two-disc device, which, however, he uses for a single-alphabet cipher. As he himself says, "this is similar to the cipher of Julius Cæsar, who, according to Tranquillus (Suetonius) substituted one letter for another." Giovanni Battista Argenti claimed in 1581 to have solved an alphabet cipher in which a key word had been employed,[39] but there is no reason to believe that Vigenère knew of this feat even if—and I have grave doubts on this point—it was actually performed.

The method of Giovan Battista Belaso or Bellaso (he himself writes the name in both ways) has been described by Meister[40] from the edition of 1564. Vigenère had learned the cipher in Rome in 1549 before it was in print. The edition of 1564—

[35] *Op. cit.*, 1518, folio C, i; 1571, p. 72.
[36] Vigenère, *op. cit.*, p. 195.
[37] Vigenère, *op. cit.*, p. 36 b.
[38] Vigenère, *op. cit.*, p. 50; *cf.* also *ibid.*, p. 96.
[39] Meister, *Geheimschrift im Dienste,* etc., p. 59; pp. 294, f.
[40] *Ibid.*, p. 36.

which Vigenère gives no intimation of ever having seen—gives the author's cipher in a form decidedly different from that of the 1553, the first printed, edition. In no work on cryptography outside of Vigenère have I seen Belaso's cipher of 1553 even mentioned. Almost, but not quite, the same cipher is given by Porta, and all writers on the subject except Vigenère, so far as I have been able to see, assign the cipher to the great physicist; in fact, it is almost universally known among cryptographers as the Alphabets of Porta.

The treatise of Belaso, written in Italian, is its author's sole claim to fame. He sets no small store by it, as may be seen from the following translation of the title-page: "The cipher of Sig. Giovan Battista Belaso, Gentleman of Brescia, lately brought by him to the greatest brevity and perfection; which cipher, even though printed, is of such marvelous excellence that all the world may use it, and notwithstanding no one will be able to understand what the other writes, save only those who possess a very brief key, as is taught in this booklet, together with his explanation and method of use."

In the edition of 1553 Belaso makes provision for a key as in the chiffre carré—his words on this point will be quoted later—and then gives his series of alphabets in the following eleven tables:

A	B	a	b	c	d	e	f	g	h	i	l	m
		n	o	p	q	r	s	t	u	x	y	z
C	D	a	b	c	d	e	f	g	h	i	l	m
		t	u	x	y	z	n	o	p	q	r	s
E	F	a	b	c	d	e	f	g	h	i	l	m
		z	n	o	p	q	r	s	t	u	x	y
G	H	a	b	c	d	e	f	g	h	i	l	m
		s	t	u	x	y	z	n	o	p	q	r
I	L	a	b	c	d	e	f	g	h	i	l	m
		y	z	n	o	p	q	r	s	t	u	x
M	N	a	b	c	d	e	f	g	h	i	l	m
		r	s	t	u	x	y	z	n	o	p	q
O	P	a	b	c	d	e	f	g	h	i	l	m
		x	y	z	n	o	p	q	r	s	t	u
Q	R	a	b	c	d	e	f	g	h	i	l	m
		q	r	s	t	u	x	y	z	n	o	p

S T	a b c d e f g h i l m p q r s t u x y z n o
V X	a b c d e f g h i l m u x y z n o p q r s t
Y Z	a b c d e f g h i l m o p q r s t u x y z n

The first letter of the key is found among the capital letters at the side, and the first letter of the message among the small letters of the same table. The letter of the message is then enciphered by the letter in the table standing above or below the message letter. Thus, if the first word of the message is larmata and the key begins with uirtuti, we write

Key	u i r t u t i
Message	l a r m a t a

and the cipher text will be....s y b o u e y.

Porta, as already stated, uses this cipher almost without change.[41] The tables as he gives them are more regularly arranged than Belaso's, and this regular arrangement is followed by Vigenère and, so far as I have been able to see, by all other writers on the subject without exception. It should be noted, however, that such a description of what is, in effect, a simpler form of the chiffre carré, overlooks one important point: just as the literature of cryptography in general ignores the fact that Vigenère provides for mixed as well as for normal alphabets, so Vigenère himself and the literature throughout ignore the same fact in the case of Porta. Porta's language is perfectly clear on this point:[42] the order of the letters "may be arbitrarily arranged, provided no letter be skipped." It may be added, too, that Porta at another point[43] gives no actual tableau carré, which, like Tritheim's "tabula recta," uses the alphabets in regular order, but denotes the letters by arbitrary cipher characters placed in an extra column at the left.

This table appears also in Diego Fernandez' "Historia del Peru," which was published at Seville in 1571.[44] Any one who will compare the ten pages of this book that deal with cryptog-

[41] Porta, *De Furtivis Literarum Notis*, p. 101.
[42] *Op. cit.*, p. 100.
[43] *Op. cit.*, p. 85.
[44] P. 108. See also Carmona, *op. cit.*, p. 170, ff., who has, however, failed to note that Fernandez' source is Porta.

raphy with corresponding passages of Porta will have no doubts as to the source of Fernandez' material, though Porta is nowhere mentioned. At times the correspondence is practically word for word. Fernandez does take one step which he proudly mentions as his own: in addition to the columns of arbitrary characters at the left of the table (as in Porta), he has an alphabet with the letters in mixed order at the right, and tells us that either column may be used for the cipher, or the two may be employed alternately.

There is a passage in chapter XII of Hieronymus Cardan's *De Rerum Varietate,* published at Basel in 1557, which, I think, shows clearly that the great mathematician knew of a cipher system similar to Belaso's. Unfortunately the passage of Cardan is somewhat obscure, and is, I think, torn from its context, so that it would lead too far afield to discuss it here. In any case, following Belaso, as it does, by several years, the passage does not bear on the actual invention of the cipher system in question.

The table of F. F. Piccol'huomini (or Piccolhomini) reproduced by Meister [45] may or may not have antedated Vigenère. Its exact date is uncertain.[46] In any case, however, it was not published but was kept in the papal archives, and it is scarcely possible that Vigenère knew of its existence.

Vigenère does make an attempt, however, as we have seen, to trace his alphabets further back than Tritheim, and to assign them to the Hebrew work, Sepher Yezirah. Later on he gives us a table of biliteral combinations of Hebrew letters, and says,[47] "The following table of 'Ziruphs' (The word means 'combinations': the Ziruphs are various combinations of letters and the permutations that can be made of one letter with others) is taken from the Yezirah. We have inserted it here to show, first of all, the great antiquity of ciphers, and beyond that to make more and more clear what we have already stated—that all cipher devices came originally from the Hebrews; and to show that the first and the second table on pages 46 and 50,[48] no matter who their authors may be, as well as the table of double ciphers and chiffres carrés which will be treated at their proper place, were borrowed from these tables of the Ziruphs."

[45] *Geheimschrift im Dienste,* etc., p. 142.
[46] *Ibid.,* p. 45.
[47] Vigenère, *op. cit.,* p. 95; p. 93 b; *cf.* also p. 184.
[48] *I.e.,* the table with the so-called "alphabets of Porta," and the tableau carré.

The *Sepher Iezirah* or *Sepher Yezirah* (*i.e.* "The Book of Creation") is a treatise on Gnosticism—that branch of philosophy of which Vigenère is so extremely fond that he devotes endless pages of the Traicté des Chiffres to its discussion.[49] Its authorship and date are alike uncertain.[50] It has been assigned to various periods varying from the second to the tenth century of the Christian era. A Latin translation by Postellus was published in Paris in 1552, and Vigenère may have known the work in this form as well as in the original Hebrew. An edition with notes and an English translation by the Rev. Isidor Kalisch appeared in New York in 1877. A careful inspection of the latter edition establishes chapter II as the only possible basis for Vigenère's statement. Kalisch's translation of this portion is as follows:

> The twenty-two letters which form the stamina (foundation) after having been appointed and established by God, He combined, weighed and changed them, and formed by them all beings which are in existence, and all those which will be formed in time to come. . . .
>
> He fixed the twenty-two letters, stamina (foundation) upon a sphere like a wall with two hundred and thirty-one gates, and turned the spheres forward and backward. For an illustration may serve the three letters, ayyin, nun, gimmel. There is nothing better than joy, and nothing worse than sorrow or plague. [The three letters given when read from right to left spell a Hebrew word signifying "joy," and when read in reverse direction make a work meaning "sorrow" or "plague."]
>
> But how was it done? He combined, weighed and changed the aleph with all the other letters in succession, and all the others again with the aleph; beth with all, and all again with beth; and so the whole series of letters. Hence it follows that there are two hundred and thirty-one formations, and that every creature and every word emanated from one name.
>
> He created a reality out of nothing, called the nonentity into existence and hewed, as it were, colossal pillars from intangible air. This has been shown by the example of combining the letter aleph with all the other letters, and all the other letters with aleph. He predetermined, and by speaking created, every creature and every word by one name. For an illustration may serve the twenty-two elementary substances by the primitive substance of aleph.

It is hard to see how this passage can be made to refer to ciphers of any kind. Vigenère has allowed his enthusiasm to betray him. He has even been guilty of an error in computa-

49 Vigenère, *op. cit.*, passim.

50 *Cf.* Jewish Encyclopedia (New York, 1906), Vol. XII, s. v. "Yezirah," and the references given in Kalisch's edition.

tion: his table represents each of the twenty-two Hebrew letters for cipher purposes by eleven other letters successively, giving $22 \times 11 = 242$ equivalents, and then, by reversing the values, he produces 242 more—a total of $22 \times 22 = 484$. The passage of the Sepher Yezirah, however, clearly refers to the combinations —not permutations—of the twenty-two letters two at a time excluding doublets—a total of 231, as is twice expressly mentioned in the text.[51]

We proceed to the question of the key—the method of indicating a change in the alphabet to be employed in the cipher message, and the frequency with which the change is to be made.

Alberti, with his discs, indicates the alphabet to be used by the setting of the discs; the correspondent writes a single letter of the outer circle (provided with capitals) and this is to signify that the discs are set with that letter opposite a permanently agreed upon letter of the inner disc (provided with small letters). The clear text is then found on one disc and the letter opposite on the other disc is taken as the cipher equivalent. This continues until another capital in the cipher text indicates a new setting. Alberti tells us that the key should be changed "after writing three or four words." In other words, his cipher messages would consist of a series of three- or four-word passages, with the alphabet changing at the end of each of them. If Alberti had used a key to change the alphabet *after each letter* he would have had the equivalent of the chiffre carré with mixed alphabets. As it is, he has the alphabets but not the letter-to-letter change.

Tritheim has several methods of using his collections of alphabets:[52] (1) We may employ them individually and have a different alphabet for each correspondent. This, of course, is not a multiple alphabet cipher at all. (2) We may use the twenty-four alphabets of the "tabula recta" in order, changing from alphabet to alphabet with each letter in the text. (3) We may make a forty-eight alphabet cipher by using the twenty-four alphabets of the "tabula recta" for the first twenty-four letters and the twenty-four alphabets of the "tabula aversa" for the next twenty-four letters. (4) We may use the alphabets of any of the various "orchemata" in any of the manners just de-

[51] *Cf.* the note (no. 21) in Kalisch's edition, p. 50. Kalisch gives no intimation whatever of anything connected with ciphers.

[52] *Op. cit.*, 1518, folio B, iiij, ff.; folio o, i, ff.; 1571, pp. 69, ff., 551, ff.

scribed. In none of these cases is a key employed, and a careful study of the "Polygraphia" and of the "Steganographia" with their commentators [53] has led me to the conclusion that Tritheim was ignorant of this device. One of his disciples, however, has tried to show that Tritheim actually knew the key maneuver and intended to explain it in that part of the "Steganographia" which was never completed.

Among the many boastful assertions which Tritheim makes in various places is one [54] which reads: "There are methods of writing with complete secrecy and without arousing any suspicion, in which a word will not represent a letter but each word will stand for another entire word that is part of the secret." Tritheim gives us as an example the secret message, "Conspiraverunt in necum tuam Melancius, Tyberius, Ioannes et Petrus famuli comitus de Asoto, et quarta die post Laurentii in nocte circumvallabunt domum tuam, provide quid agas." This, he says, may be expressed in cipher thus: "Oro te amice charissime, ut mutuo mihi transmittas decem florenos cum latore praesentium, quia sunt mihi valde necessarii pro constructione cuiusdam aedificii, eos tibi fideliter restituam." Or, he says, the same secret message may be expressed in another way, thus: "Domine Iesu Christe fili Dei vivi, qui homo fieri, pro nobis pati et mori voluisti, miserere mei peccatoris indigni, nunc et in terribili hora mortis meae."

Tritheim nowhere explained this method—at least not in any of his surviving writings—nor has any one since his day succeeded in offering a convincing explanation. Vigenère regards Tritheim's boast as impossible of fulfillment.[55] Father Athanasius Kircher [56] attempts to show that Tritheim was intentionally obscure in making this promise, and that what he really had in mind was a multiple alphabet cipher for which an open message (such as either of these given here) was to be used

[53] The various controversies concerning the "Steganographia" do not enter into the present discussion. The *Clavis Steganographiae Ioannis Trithemii,* first published in the Frankfurt edition of 1608, contains several disc ciphers, which, however, are merely single alphabet ciphers similar to the disc cipher of Silvestri. Meister's statement (*Geheimschrift im Dienste,* etc., p. 40) that Tritheim's methods are more complicated than Alberti's is apparently due to a failure to note that the disc ciphers in Collange's French edition of *Tritheim* (Paris, 1561; mentioned by Meister, *op. cit.,* p. 42) are added by Collange and are not in Tritheim's work.

[54] *Op. cit.,* 1518, folio B, ij, verso; 1571, p. 64.

[55] *Polygraphia Nova,* Rome, 1603, pp. 139, ff.

[56] Vigenère, *op. cit.,* pp. 182 b, f.

as a key, the resulting cipher text to be sent to the correspondent. It is no exaggeration to call this explanation nonsense. Kircher has failed to note that his plan disregards Tritheim's two main points—that the cipher text is to represent the clear text *word by word* and not letter by letter, and that the cipher text is to be of such a nature that it will not arouse suspicion. Whatever Tritheim may have meant, he certainly did not mean the procedure suggested by Kircher; and there is no reason whatever to believe that he alludes here or anywhere else to the device of changing his alphabets by means of a key.

The first cryptographer actually to publish a method of using a key with a multiple alphabet cipher—and we have no knowledge of any use of the method before him—was Bellaso, whose work has been described above. Because of its importance as the first publication of the key method, his brief description may here be given in full: "A key (contrasegno) must be agreed upon by the correspondents. This key may consist of some words in Italian or Latin or any other language, and the words may be few or many as desired. Then we take the words we wish to write, and put them on paper, writing them not too close together. Then over each of the letters we place a letter of our key in this form: Suppose, for example, our key is the little *versetto* virtuti omnia parent. And suppose we wish to write these words: Larmata Turchesca partira a cinque di Luglio. We shall put them on paper in this manner:

v i r t u t i o m n i a p a r e n t v i r t u t i o m n i a p a r e n t v i
l a r m a t a t u r c h e s c a p a r t i r a a c i n q u e d i l u g l i o"

A bit further on he points out that in order to avoid errors, it will be a good plan to have a number of mottos on hand to be used as keys, and begin each new line with a new key.

Porta, a few years later, employs the key system in the same manner as Vigenère.[57] He gives us also a disc cipher with a mixed alphabet like that of Alberti's described above, except that Porta does not use the code feature of Alberti's device, and that he does use a word or phrase as a key to indicate a change of alphabet with each letter of text. If Alberti had made use of the key in this manner, he would have had the equivalent of the chiffre carré. Porta's sample key incidentally is a long one —Castum foderat Lucretia pectus Algazel.

[57] Porta, *op. cit.*, p. 102.

The treatise of F. F. Piccol'huomini referred to above as containing a tableau carré speaks of a method of using the table with a key to govern a change of alphabets with each letter of text. The author gives a sample message in both cipher and clear text, but refuses to explain the method of using the key. Instead he defies discovery of his method even with the material given. My study of his example makes me strongly suspect that his description does not stick strictly to the truth. In any case, as already mentioned, there is no reason to believe that Vigenère knew of the existence of this treatise.

By the time of Vigenère key devices for multiple alphabets were fairly floating around in the air. To quote him:[58] "As for the afore-mentioned keys, it is neither possible nor necessary to set any rules or bounds for them. Every one may do in this respect as he pleases, and make endless devices as his fancy dictates. Some correspondents provide each other with a certain number of words which they agree upon. The first of these words is used for the first dispatch, the second for the second, and so forth. It is quite true that the longer the key is, the more difficult it is to solve the cipher, but the difficulty and confusion in both the enciphering and the reading of the message are similarly increased. Hence, some prefer several words at once, or even whole verses of some poet, while others are content with the date of the month and the day of the dispatch, as for example, 'October the fifteenth' or similar phrases. Others use the word that immediately precedes the cipher text, or one of the letters prefixed to it as nulls [*i.e.,* extra letters of no significance], this letter then serving at the same time as a key: the whole of the first word is enciphered with this letter as key, and the first word then serves as the key for the second, the second for the third, and so forth with the rest. If some clear text intervenes, the method is continued when the cipher starts again, or the process may be begun afresh, or some other course agreed upon by the correspondents may be followed. Still others go through the entire alphabet letter by letter, commencing wherever they wish, provided they use the whole alphabet; and then they begin again, going around in a circle. This greatly increases the labor involved, so that I should rather confine myself to a single letter, from which all the rest follow in order, as if

[58] *Op. cit.,* pp. 48 b, f.

born of one another. This may be done in two ways, as an example will show. Each letter may be enciphered by the preceding letter, thus: with the . . . text. . . . Au nom de l'eternel and the key D, we say, a [the first letter of the text] from D [the key letter] gives x; u [the second letter of the text] from A [the first letter of the text, which now becomes the key] gives i; n from U, a; o from N, h . . . etc. The other method, which is more secret,[59] is to encipher each letter of the clear text not by the letter [of that text] which precedes it, but by the letter by which the preceding letter is enciphered. Thus, a from D, which is the key, gives x; u from X [the first letter of the cipher text] h; n from H, e; o from E, e. . . ."[60]

Strangely enough, while the chiffre carré in general has been called the "Vigenère Cipher," credit for the self-keying device, which Vigenère expressly claims, has been assigned elsewhere.

Lange et Soudart,[61] under the heading "Systems with indefinite key," say: "The name 'systems with indefinite key' is applied to those systems in which the key selected contains an unlimited number of letters. The said key consists of one or more pages chosen from a book agreed upon [This does not concern us] *or of the text that is to be put into cipher* (italics mine)." The italicized words are the essence of Vigenère's proposal. The account of Lange et Soudart, however, continues: "The invention of systems with an indefinite key is apparently to be attributed to Hermann; at least we have not found the description in any other author before him." Hermann's booklet, "Le Livre des Clefs," with a separately printed "Introduction," was published at Paris in 1892. The key system described[62] is the first of the two just quoted—the use of the successive letters of a passage of some book.[63] The other method—the one italicized above—is not given by Hermann, but is further described by Lange et Soudart as follows: "In the latter case the system is called autoclave; the beginning is enciphered with a short key and the encipherment is continued by

59 In this statement Vigenère is mistaken: the first of the two methods is safer.

60 Vigenère is here enciphering not from his tableau carré but from his own arrangement of Belaso's alphabets. He uses an alphabet of twenty letters, while Belaso employs twenty-two. Vigenère has slightly shifted the order of the letters.

61 *Op. cit.*, p. 182.

62 *Op. cit.*, p. 49.

63 In his discussion of long keys—which he considers disadvantageous—Vigenère (*op. cit.*, p. 49) comes close to describing this kind of key also.

the use of the text itself as the key." A comparison of this description with Vigenère's words shows that he anticipated the invention of the autoclave.

The above account of the chiffre carré justifies, I think, the following conclusions:

1. Vigenère's description of the cipher differs decidedly from the form usually ascribed to him, and presents an essentially more difficult problem to the would-be decipherer.
2. Even so, Vigenère nowhere speaks of the cipher as the chiffre indéchiffrable par excellence.
3. Except for the self-keying process, Vigenère lays no claim to having originated the cipher.
4. Instead he gives an essentially accurate account of its history.
5. This account errs in assigning the "Sepher Yezirah" as the ultimate source of the chiffre carré; that book is in no way concerned with ciphers.
6. Vigenère's contribution of the self-keying device or autoclave is generally ignored in the literature on the subject.

THE PALÆOLITHIC BEGINNINGS OF RELIGION—AN INTERPRETATION

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ABSTRACT

The burials and the art of the Aurignacian period show that men then worshipped a mother goddess, and this worship can be traced back to Mousterian times, when Neanderthal man flourished. The same art shows that women reverenced the erect phallus. These are the only objects that they seem to have considered divine. There is reason to believe that the part of a father in procreation was not yet known. The worship was not a fertility-cult in the later sense. No privacy existed; men and women knew the details of each others' physical forms. Men saw women miraculously produce children. Like the male animals, they had from instinct coitus with her. Orgasm give them the divinest thrills they knew. It was to them like the later bacchic ecstasy of intoxication. Women became their goddesses. Probably they did not generalize more than the dog, but each was devoted to his mistress. Women obtained a similar mystic ecstasy from the experience. She did not deify man, but the erect phallus. The heart of religion is a mystic thrill, uplift or satisfaction. Creeds, rituals, and conduct are all subordinate to this. Palæolithic religion was, then, sex-mysticism. The psychologic unity of the race made it universal as its survivals in the historic period prove. This is the real origin of religion. It was not begotten by fear (Lucretius), nor by animism (Tylor), nor by ancestor worship (Herbert Spencer), nor by the *mysterium tremendum* (Otto), but by the *mysterium feminium* —a *mysterium tremendum* indeed, but scarcely that which Otto contemplated. In adult life we forget the umbilical cord and the nursing; similarly religion has now almost everywhere left far behind its biological beginnings.

WITHIN the last thirty years researches in the field of prehistoric archæology have brought to light facts which throw the problem of the origin of religion into a new perspective and enable one to attempt its solution on the basis of the interpretation of objective data. Hitherto theories as to the origin of religion have been projected by philosophers, anthropologists, or theologians, who, when guided by facts, have been familiar only with the conceptions which underlie historic religions or the beliefs and customs of contemporary savages. Thus it has been supposed that religion began with the worship of ancestors,[1] or that it had its origin in the worship of the friendly spirits by which man was surrounded, *i.e.*, in animism,[2] or that its beginnings were motivated by fear.[3] It has remained for a contem-

1 Herbert Spencer, *Principles of Sociology,* Pt. VI, London, 1885.

2 E. B. Tylor, *Primitive Culture,* London, 1891.

3 Lucretius, *De Rerum Natura,* Book V, 1161–1240.

porary philosopher, to find the origin of religion in the *mysterium tremendum*.[4] These theories of the root from which religion sprang have been projected by men whose minds have been "sicklied o'er by the pale cast of thought." They have earnestly tried to base their theories on facts, but the earliest objective manifestations of the religious spirit were not known to them. To solve the mystery of religion's beginnings, one needs the most primitive facts as a starting point, and then the ability to look at the world through the mind of the earliest religious men. To such a man, the *mysterium tremendum* certainly appeared quite differently than to a modern professor or theologian.

For a quarter of a century data from palæolithic pre-history have been available for a new approach to the subject, but they have been scattered here and there in anthropological journals, and students of religion have been unconscious of their significance. The present writer, though actively engaged for fifty years in researches in this field,[5] and although he had been vaguely aware of these discoveries, came but recently to realize their significance. The beginning of this realization he owes to an unpublished manuscript entitled "The Great Mother" written by the Reverend John Lewis, Ph.D., Pastor of the Calvary Presbyterian Church of Milwaukee, Wisconsin. In this work Dr. Lewis has collected from the widely scattered publications of explorers, the facts concerning the prehistoric mother goddesses and has related these discoveries to the many fertility cults in all parts of the world. Such cults are or have been practically universal. Dr. Lewis has, with great insight, interpreted in a broad way the facts which he has so laboriously collected. If, however, we carry the psychological interpretation of the phenomena a little further than he has done and look for the *Sitz im Leben* into which they fit, we gain at last a valid explanation of the origin of religion.

To appreciate the facts one should first glance at the background of the old stone age, the cultural history of which is traceable in the artifacts, implements, skeletons, drawings, and sculptures left by the men of that period. It is estimated that it covered a time extending from about 100,000 B.C. down to

[4] Rudolf Otto, *Das Heilige*, Stuttgart, 1923; *The Idea of the Holy*, London, 1925.

[5] See his "Semitic Ishtar Cult" in *Hebraica*, Vols. IX and X, 1893, *Semitic Origins*, 1902, and *Semitic and Hamitic Origins*, 1934.

10,000 or perhaps 7,000 B.C. It is divided by archæologists into three periods: Lower Palæolithic, 100,000–40,000 B.C.; Middle Palæolithic, 40,000–25,000 B.C.; and Upper Palæolithic, 25,000–10,000 B.C., or a little later.[6] Three periods can be discerned in the Lower Palæothic culture: the Pre-Chellean, Chellean, and the Acheulean. The Middle Palæolithic culture is represented by the Mousterian period. The relative chronology of these cultures is established by the stratification of gravels containing artifacts of these periods at Chelles-sur-Marne in northern France.[7] The Upper Palæolithic consists of four periods: the Aurignacian, Solturean, Magdalenian, and Azilian-Tardenoisan cultures. The Aurignacian and Solturean cultures lasted from about 25,000 to 16,000 B.C.; the Magdalenian, from about 16,000–12,000 B.C.; the Azilion-Tardenoisan, from about 12,000 B.C. till the coming of the Neolithic culture.

During the Pre-Chellean and Chellean periods tropical weather prevailed in Europe, mastodons and other tropical animals roamed the land, man dwelt in the open, and nature did not compel him to engage in a hard struggle for existence.[8] It was during this period that the so-called Heidelberg and Piltdown men flourished. The only remains we have from these periods consist of artifacts found in the gravels of river-terraces, and of occasional skeletons found accidentally in gravels. With the Acheulean period the ice ages, or periods of glaciation began, and men were driven into caves for shelter, began to wear clothing, learned to make fires, and in time to bury their dead. Toward the end of this period or the beginning of the next, Neanderthal man emerged[9] and held the stage for some thousands of years. His type of culture was far-flung; he not only flourished in Europe, but in Palestine,[10] and this summer (1939)[11] a skeleton of this race has been discovered in Siberia.

[6] I have followed the chronology of Henry Fairfield Osborne, in his *Men of the Old Stone Age,* New York, 1925, p. 18. Other scholars differ from Osborne in some details; *cf.* A. L. Kroeber, *Anthropology,* New York, 1923, p. 156, and R. A. S. Macalister, *Text-Book of European Archæology,* Cambridge, 1921, p. 593 f.

[7] *Cf.* Osborne, *Op. cit.,* p. 111. For evidence that cultures parallel to those in Europe existed in Egypt during each of the periods from the Chellean onward, see Sanford and Arkell, *First Report of the Prehistoric Survey-Expedition* (*Oriental Institute Communications* No. 3), Chicago, 1928, and *Palæolithic Man and the Nile-Fayum Divide,* Chicago, 1929.

[8] *Cf.* Macalister, *European Archæology,* p. 62; Osborne, *Men of the Old Stone Age,* p. 124.

[9] See the tables of Osborne and Kroeber cited in n. 6.

[10] See Turville-Petrie, *Researches in Prehistoric Galilee,* London, 1927.

[11] *New York Times* of July 8th, 1939.

Neanderthal man persisted through the Mousterian or Middle-Palæolithic period.[12] At the beginning of Upper Palæolithic time—the Aurignacian period—the Cro-Magnon race invaded Europe, displaced Neanderthal man, who had been weakened by the hardships of the glacial periods, and introduced a new and higher civilization.[13] The Cro-Magnons not only made improved tools, but had the ability to express themselves in drawings, paintings, rock-carvings, and sculpture in the round.[14] It is from their culture that the outstanding facts come, which demand a sympathetic understanding and explanation from the student of the history of religion. This race and its successors flourished for some thousands of years until their children or a new race, which displaced them, came, and learned the art of polishing stone and of domesticating animals.[15] Then the men of the Old Stone Age disappeared before those of the New.

Cro-Magnon man, whose coming into western Europe inaugurated the Aurignacian period, drew and painted on the walls of dark caverns numerous animal forms.[16] When first discovered these were thought to have religious significance, but it was soon perceived that their purpose was magical rather than religious.[17] Frequently drawing was superimposed upon drawing as would have been impossible, had a religious motive prompted the artists. To the present day many savages believe that there is a mysterious connection between a living being and a representation of it, so that what is done to its image or picture affects also the living creature. The animal drawings were believed to be an aid to Aurignacian huntsmen. Another series of drawings depict scenes of actual hunting and doubtless were made for a similar purpose.[18] A third series of artistic efforts has, undoubtedly, a religious significance, though its full meaning is not yet understood. It is the purpose of this paper to contribute to the understanding of that significance.

"The Aurignacian stage has left us certain examples of sculp-

[12] Osborne, *op. cit.*, pp. 254–258.

[13] Osborne, *op. cit.*, Ch. IV.

[14] Macalister, *op. cit.*, p. 443 ff.

[15] Macalister, *op. cit.*, p. 517.

[16] *Cf.* Osborne, *op. cit.*, pp. 353–369, and Macalister, *op. cit.*, pp. 461–481.

[17] See the article "L'art et la magie, à propos des peintures et des gravures de l'âge du renne" in *L'Anthropologie*, XIV (1903), p. 257 ff., and Macalister, *op. cit.*, p. 508.

[18] *Cf.*, for example, M. l'Abbé Breuil in *L'Anthropologie*, XXIII, and Macalister, *op. cit.*, Fig. 157.

ture in stone, ivory, reindeer-horn, and bone. These all represent human figures—exclusively, in this stage, of women. They are invariably undraped, and the details of the body associated with the functions of motherhood are almost always exaggerated. The figures are represented as though pregnant, or at least corpulent, but to this rule there are exceptions. The breasts are prominent and generally pendant; the external organs of sex are strongly marked and the hips are also prominent."[19] Many of these figures have been found buried with the dead. One of these was found in 1939 in a palæolithic context in Siberia not far from Irkutsk. It proves that the type of religion represented was not confined to western Europe, but was as widely distributed as the palæolithic culture.[19a] It is significant that no figures of animals or of men are so found. Dr. Lewis has pointed out[20] that in the case of such burials the body of the dead is covered with red earth and often either rests upon or is partly covered with the shells of various kinds of shell-fish. He rightly infers that the image represented the mother-goddess—the goddess of life—that the red earth represented blood, the vehicle of life, and that the shells were emblems of the mother goddess because, like her, they contained life or produced a living creature. Men of the glacial and interglacial times were characterized by an unconquerable passion for life, they believed that their dead did not become extinct at death, they buried them in red earth with food, implements, shells, and at times with images of the "great mother." All this Dr. Lewis has clearly and acutely discerned.

It should also be noted that the ideas expressed in Aurignacian burials and sculpture are much older than the Aurignacian period. When art develops and is called into the service of religion, it invariably expresses conceptions that are already hoary with age and are sacred partly because they are hoary. For many centuries before Cro-Magnon man discovered how to express his thoughts in art, his artistically inarticulate predecessors had been slowly developing the ideas and experiencing the emotions which found expression for the first time through the skill of Aurignacian artists. Thus a skeleton of a Neanderthal man from a tomb of the Mousterian period was covered

19 Macalister, *op. cit.*, p. 443.
19a *New York Times* of Aug. 1, 1939.
20 *The Great Mother*, Ch. I.

with red ochre,[21] symbolical of the blood of the mother-goddess. Similarly the female body found in the Grimaldi cave—a burial dating from the late Mousterian period—was covered with shells.[22] It is safe to conclude, therefore, that the main religious ideas embodied in the facts cited had their origin in the Mousterian period and that the religion which they express had taken shape as early as the time of Neanderthal man.

Have these facts any connection with one another? Were the images of the pregnant woman, the red earth, and the shells associated in the thought of these early men? Dr. Lewis contends that they were. Woman was the source of human life; red blood, suggested by the red earth, was the vehicle of life (births were accompanied by bloody discharges, and when a man or an animal was wounded and bled profusely it died); while shells, like women, gave forth, when opened, living things. Dr. Lewis, in this association of ideas, has rightly discerned the workings of the primitive mind.

Have these facts any religious significance? Several scholars believe that they have. Osborne was quite positive on this point, for the Cro-Magnon race, but held that their religion consisted in awe of the power of nature vaguely conceived after the manner of *Mana* among the Polynesians or Manitou by the American Indians.[23] Macalister holds that Neanderthal man believed in a future life and probably had a religion, but finds himself unable to define its outlines.[24] He holds that Cro-Magnon man may have had animal deities,[25] that they engaged in religious dances,[26] considers but rejects the theory that they were totemistic.[27] Lewis demonstrates that they were devoted to the worship of the Great Mother.[28] Is it possible by comparison with the customs and thoughts of the most backward existing savages to divine any more definite conception of the nature of their religious thoughts and practices? I believe it is.

Before making this attempt, however, it will be necessary to define to ourselves somewhat clearly what it is that constitutes

[21] Macalister, *op. cit.*, p. 299. Sometimes a skeleton was tinged red; *ibid.*, p. 355.

[22] Macalister, *op. cit.*, pp. 353, 355, 502, and 512; also the numerous instances cited by Dr. Lewis.

[23] *Op. cit.*, pp. 272, 358–360.

[24] *Op. cit.*, p. 353 ff.

[25] *Ibid.*, p. 500 ff.

[26] *Ibid.*, pp. 482 and 496 ff.

[27] *Ibid.*, p. 505 f.

[28] Lewis, *The Great Mother.*

religion, as well as to guard ourselves against the danger of attributing to palæolithic man greater knowledge of biological processes than he possessed. These two matters should be discussed separately.

There have been many definitions of religion, but most of them are so abstract as to be hardly applicable to the emerging religious consciousness of men of the old stone age. Religion begins in an experience that exalts the spirit of man. In the earlier stages of mental development it gives him a thrill, an uplift; in the later, the sense of a supersensuous presence, of a spiritual companionship, of peace, of inspiration—the vision of new duties—a higher life. This experience is the mystic element in religion. As "that is not first which is spiritual, but that which is natural, and afterward that which is spiritual," this mystic experience on the lower levels of man's emerging life may be the frenzy from intoxicating drinks as in the Soma- and Dionysiac-cults, the visions of kataleptic ecstasy, or the ecstasy of sexual orgasm. In its higher reaches it embraces experiences like that of Isaiah: "I saw Yahweh, high and lifted up,"[29] or St. Paul's: "it pleased God to reveal his Son in me"[30] —"I can do all things in him that strengtheneth me,"[31] or Wordsworth's: "I have felt a presence that disturbs me with the joy of elevated thoughts . . . whose dwelling is the light of setting suns, and the round ocean and the living air, and the blue sky, and in the mind of man." This experience, whether on its lower or its higher levels, has given man the consciousness of a power which lifted him above the ordinary levels of life and relieved his existence from its wearisome or painful monotony. Organized religion possesses mythologies or theologies which attempt to explain how the experience is possible, liturgies or forms of worship which are designed to refresh or reproduce the experience, and rules of conduct which embody the ritual or ethical demands required by the "presence" that has "disturbed." These, by themselves, do not constitute religion. Theology (or mythology) without experience is but a system of philosophy. Liturgy without mystic realization is only a system of magic. Conduct apart from a sense of the divine is but ethical culture. If we are to find religion in the

29 Isa. 6: 1.
30 Gal. 1: 15, 16.
31 Phil. 4: 13.

old stone age, then, we must look for its mystical content. We must expect its mysticism to be of a crass and sensuous type, for the soul of man was only just taking form in its animal chrysalis, but, if we can find nothing that in that far-off time gave men the thrilling feeling that they were exalted above their ordinary selves and touched with an efflatus of higher power, we must conclude that evidence for religion is lacking.

Again, we must be careful not lazily to interpret the phenomena we find by means of the knowledge we possess, or even of that possessed by men of the earliest historical period—the early bronze age. Macalister, for example, remarks that a drawing of women dancing around a phallic idol of the Aurignacian period "probably" pictures a rite "to secure fertility."[32] This assumes that the part of a father in the procreation of a child was already known some 35,000 or 40,000 years ago. Such knowledge may well be doubted. The savages of Australia, who still live in a palæolithic culture, have no knowledge of physical paternity. A woman becomes pregnant because a spirit, which has been lurking about in some tree or bush for the opportunity, jumps upon her person and enters into her.[33] Similarly the natives of the Trobriand Islands of British New Guinea, not only have no conception of physical paternity, but will not be persuaded of its truth.[34] Like the Australians, they hold that pregnancy is caused by a spirit which has chosen to find its habitation in a woman. Doubtless other tribes could have been found equally ignorant as to this matter, had anthropologists sought their beliefs concerning it. Social paternity is recognized, but physical paternity is denied.

It goes without saying, of course, that there must have been many millennia in the early history of mankind when such ignorance was universal. Dogs and other animals mate from instinct; they have no knowledge that their obedience to instinct has aught to do in perpetuating their kind on the earth. Men began human life at the animal level of knowledge and, by the use of a larger brain, gained little by little larger knowledge and

[32] *Op. cit.*, p. 497.

[33] Spencer and Gillen, *Native Tribes of Central Australia*, London, 1899, pp. 123, 124, 126, 127 and *Native Tribes of Northern Australia*, London, 1914, Ch. VIII. This belief is universal over the whole of central and northern Australia, a region four and a half times the size of Britain.

[34] B. Malinowski, *The Sexual Life of Savages*, London and New York, 1929, Ch. VII.

consequent mastery over nature. That the Cro-Magnon race possessed a brain capacity so great that this biological fact *might* have been discovered, is perfectly true,[35] but that is no guarantee that it *was* discovered. One of the marvels of our remote posterity will probably be that so many of the things we know now went undiscovered for so many centuries—all the way from the ancient Greeks to the nineteenth century. The religious conceptions which we are seeking to interpret go back, however, to Neanderthal man, in whom we cannot assume a higher degree of intelligence than that possessed by Australian savages.[36] It is safe, I believe, to assume that "the worship of the child-bearing mother-goddess of the old stone age was developed by peoples in utter ignorance of physical paternity.[36a]

Though ignorant of physical paternity, these people were not ignorant of the fact that man possesses an inner impalpable nature that is distinguishable from the body. In sleep one dreams; he wanders long distances, he sees and talks with those who have been his companions, but are now dead. When the spirit departs from the body, the body becomes inert; it decays; the man is dead. This was observed, but the departed spirit was believed to survive and to have the same needs that it had in life. By its body food was placed; in burial it was furnished with the tools and weapons it had employed in life.[37] Probably, as in Australia,[38] it was believed that the spirit longed for a body in which it could be reincarnated. If Neanderthal man reflected at all as to where the spirits that made women pregnant came from, he, like the modern Australian, reasoned that they were the spirits of ancestors or friends who had died.

[35] *Cf.* Osborne, *op. cit.*, p. 299.

[36] *Cf.* Osborne, *op. cit.*, p. 136. The Neanderthal brain-capacity was less than that of the lowest Australian races.

[36a] Of the time when men first became aware of the fact of physical paternity we only know that it must have been before the beginning of the bronze age. At that time writing began to be utilized and paternity was well known. A plausible conjecture would be that the discovery occurred in the later part of the neolithic period, when agriculture had been developed and men had begun to keep cattle. Valuing the milk of sheep, goats, or camels, it must have been noticed that, unless one's herd contained males that mated with the females, his cows or nanny-goats gave no milk. A plausible hypothesis is that, just as we suppose that the domestication of animals led to totemism, so dependence on the milk of the herd opened man's eyes to physical paternity. Perhaps the lore presupposed by the story of Jacob's experiments in breeding (Gen. 30: 37–42) is based on experiences that began with the herdmen of the last half of the neolithic period.

[37] *Cf.* Macalister, *op. cit.*, p. 343 ff.

[38] See Spencer and Gillen, *The Native Tribes of Northern Australia*, Ch. VIII.

Among the objects recovered from palæolithic time, there is no evidence of the deification of a man or of an animal; only of woman. To the problem why men were not deified, we shall return at a later point. Why animals were not regarded as gods, is tolerably clear. No animal was as yet domesticated.[38a] Animals were not friends, but enemies. That community of interest that came into existence in Neolithic Time, in which men and animals form one community, was not yet born. It is a serious question whether in the Mousterian Period, when Neanderthal man flourished, man had become animistic—whether he yet believed that rocks, trees, the earth and animals had spirits like his. One who possesses a pet dog knows that dogs dream; the twitching of their feet reveals the fact that they are running in the chase; their dream becomes so real that they bark and awaken themselves. Neolithic man could easily believe from such observation that animals possessed spirits, and might easily make the generalization that everything possesses a spirit, but, as yet, the presumption seems to be that Neanderthal man had not made the animistic generalization. He, like the dog, knew the difference between animals and plants. The eye of a dog will be attracted for a moment by the twig of a tree or the stem of a plant moving in the wind. He will look for a moment intently, but, as soon as he perceives what it really is, he loses interest. It cannot run like an animal; it is not for him edible; it is incapable of affording him the pleasure of a chase. The dog knows the difference, but we have no reason to suppose that he philosophizes as to its cause. Neanderthal man knew the difference, and, even if he attributed spirits to animals, it is safe to conclude that he had not yet attributed them to stationary objects. Animals were his enemies. There is no evidence that he had even employed a hawk as an aid in catching birds. Totemism was apparently still unborn. It is quite true that the modern Australians, whose culture is palæolithic, have a well developed system of totemism, which, like that of the American Indians, is bound up with their clan or sib organization and helps to distinguish the sibs or moieties between members of which marriages may or may not take place.[39] Such organizations have grown out of the repugnance to incest that developed among savages in many parts of the world at a certain stage

[38a] At least no evidence of such domestication is as yet known.

[39] See the works of Spencer and Gillen cited in n. 33.

of their evolution. This repugnance to incest may be strongly developed in a people, like the Trobriands, who are quite ignorant of the fact of physical paternity.[40] It seems to be based on the feeling that those who have grown up in the same family together should not cohabit. It involves the notions of social paternity, but not those of physical paternity. It is altogether probable, however, that in the glacial and inter-glacial epochs the struggle for existence was so severe that men lived in very small communities, and that the feeling against incest had not yet emerged.[41] It seems safe, therefore, to conclude that animals were not yet deified and that the need for the social service to which totemism was later put, had not yet arisen.

We now come to the question, Why did man deify woman? The answer is to be found by reproducing the workings of his mind as he interpreted the most vital facts in his environment. The struggle for existence was tragically hard; he possessed an indomitable will to live. Life in the caverns of Mousterian times afforded no privacy. Men and women were the daily observers of each other's physical forms. Men, we suppose, knew that from the vulva of the woman there was a monthly discharge of blood.[42] At rare intervals from the same source living children emerged. That he had any part in causing this, man never suspected; this companion of his that possessed the ability to discharge regularly blood—mysteriously necessary to life—without serious loss, and that could bring forth also living beings that grew up to be men and women, was the divinest thing in his world. For untold ages emerging men and women had copulated as had also the animals. The thrill of orgasm men had always enjoyed, but now in the light of growing intelligence, since he recognized in woman her divine, creative power, the mystic exaltation of orgasm became his holy communion; it was the divinest thing he knew. Woman's power to produce new life, told the dawning intelligence of Neanderthal man that she

40 See Malinowski, *op. cit.*, II, pp. 494–514, and Ch. XIV.

41 Repugnance to incest is purely a human development. There is no trace of it in the higher animals, and it has not been universal among human beings. At certain periods of Egyptian history marriages between royal sisters and brothers were required; see, for example, J. H. Breasted, *History of Egypt*, New York, 1909, Ch. XV.

42 Physicians observe that from peasant women, who live much in the open air and in general much closer to nature than the protected women of modern civilization, the menstrual discharge is much less in volume than from the more protected women.

was divine; physical union with her confirmed it; it gave him the mystic thrill that transformed his dawning theology or emerging creed into a religion. It was the mystic sensation of which his appreciation of the divine power in woman became the explanation. The mysticism of Neanderthal man was a sexual mysticism.

From this point of view it is easy to see how red earth and shells came to be associated with the mother goddess. Blood is red; red earth must, he thought, be saturated with it. Shells of fish, like pregnant woman, contain living beings; it was natural, therefore, for them to become her symbols.[43] When a member of a family died, they must have observed that the flesh in time decayed. Nevertheless flesh and bones were covered with red earth and shells, that through these symbols of life the Mother might bring forth a new body into which the spirit that had been disembodied might dwell. Then, when in Aurignacian times the Cro-Magnons achieved the making of images of a mother, these were buried with the dead. Like the pictures of animals, these images were supposed to have some vital connection with the being they represented; in the thought of primitive man they were probably potential mothers. So palæolithic men, bereaved of their dear ones, sought to help them back to life, and the mute tokens by which their loving ministrations were carried out, now reveal to us the dawn of religion among mankind. It is not such a dawn as philosophers have postulated, but it was such a dawn as it was possible for an emerging soul, equipped with a large animal, but humanly rudimentary, intelligence to appreciate.

One other question presses for an answer. Did Neanderthal man identify the great mother with the earth or with a superhuman spirit? Was he capable of conceiving an ideal spirit-mother? We should, I think, answer both these questions with a negative. In the glacial and interglacial epochs, before agriculture was ever thought of, the earth must have seemed to man anything but a life-giving mother. She was not even a

[43] If the shells found with the skeletons of this period had been bi-valves, such as oysters or clams, it might be inferred that the resemblance of their opening to the lips of the female vulva might have suggested the association with the mother goddess, but, so far as I have found descriptions of them, they are shells spiral or elliptical in form (shells of the *trochus* or *nassa*-shells) the forms of which are quite different.

step-mother, even if such a relationship had then been invented. She did so little to foster the life of puny man, that it is most unlikely that it should ever occur to him to think of her as possessing the qualities of that life-giving companion who shared with him his cavern and gave and nourished children. It seems equally improbable that he was capable of projecting an ideal, supernatural mother apart from flesh and blood. A dog attaches himself to a man. Man becomes in a sense his god, but the dog does not create in imagination and ideal, impersonal man and adore him; his adoration is given to a particular man existent in the flesh. There are other men that other dogs worship—Rover knows that—but for him there is one particular man that receives his affection, his devotion, and for whom he will die. Doubtless the intelligence of Neanderthal man was greater than that of the dog, but doubtless (especially at that early stage of human experience) the workings of his mind were nearer to those of the dog than to those of a modern thinker. The probability is that it was the individual woman who shared his cave whom he reverenced as the highest expression of the divine which he knew and, in connection with whom he enjoyed his divinest experiences, that became his goddess. This does not mean that he always obeyed her, or even uniformly treated her well. How many modern Christians, or devotees of any religion, always reverence and obey their Deity? It is related of a certain son of an Arabian Sheik, who lived in pre-Islamic days, that he went to consult the oracle of his tribal god to see whether the god would assure him victory, if he went to avenge the murder of his father. Three times he consulted the oracle by casting lots [44] and three times he drew a negative reply. Becoming angry, he then broken up the arrows which he had employed in divination, threw them into the face of the idol, shook his fist at the god, and said: "Had it been your father, you would not have answered so," and rode away to kill his father's slayer. Not altogether a stranger to his spirit was that colored preacher who announced at the close of a service: "There will again be a service in this house in two weeks, if the Lord will, and in three weeks, whether or no."

[44] *Cf.* Prov. 16: 33.

"The lot it cast into the lap,
But the whole disposing thereof is with the Lord."

Neanderthal man was no more of a saint than his modern successors. Doubtless he often treated woman like the brute that he was. Nevertheless he deified woman, and thus inaugurated the earliest religion that we know, which was probably the earliest religion of the human race.

Man deified woman. Did woman make any contribution to religious thought? Did she deify anything? To this one must answer: "yes," but she did not regard as divine the whole of her male companion; only his *membrum virile erectum*. There are no traces in palæolithic religious remains of a deified man, but there is evidence that the erect phallus was regarded as a divine symbol. From Cogul in northern Spain comes a mural drawing of nine women dancing around a figurine of a mannekin with phallus erect.[45] That it was the phallus and not the man that aroused the enthusiasm of the dancing women, is attested by the fact that in many parts of the world—in countries as far removed as Britain and Japan[46]—representations of the erect phallus have been found as religious emblems. These include the "pillars" denounced in the Old Testament as well as the rude pillars in North Africa out of which Egyptian obelisks were developed. Perhaps they exist also in the New World.[47]

As we cannot suppose that palæolithic man knew of physical paternity, the motive which led these Aurignacian women to dance about a phallus cannot have been to secure fertility. Doubtless these women were as guiltless as their Australian and Trobriand sisters of knowledge that *coitus* had aught to do with giving them children. *Coitus* gave them, however, as it does their modern Trobriand counterparts, a mystic thrill as great as if not greater, than that experienced by men.[48] Their whole body was pervaded by its ecstatic influence. It gave them the mystic element of this communion which was the sublimest that the people of that time knew. In their life, which was devoid of privacy, they knew that the male phallus possessed ordinarily

[45] *Cf.* l'Abbé Breuil, *L'Anthropologia*, XX, p. 1 ff., and Macalister, *op. cit.*, p. 496.

[46] For these "pillars" and their distribution see the references in Barton's *Archæology and the Bible*, 7th ed., Philadelphia, 1937, p. 143, Aston, *Shinto*, 1905, pp. 187, 189, 191, 197, 305, and 340, Barton, *Semitic and Hamitic Origins*, p. 150 f., Sir John Marshall's *Mohenjo-daro*, London, 1931, Pl. XIII, 3, and Pl. XIV, 2, 4, and 5, and E. A. Speiser, *Excavations at Tepe Gawra*, Philadelphia, 1935, Pl. XLVI, b.

[47] *Cf.* T. A. Joyce, *South American Archæology*, New York, 1912, Pl. XXIV.

[48] See Malinowski, *op. cit.*, II, p. 341.

no power thus to exalt the feelings. It was an inert, uninteresting organ. At times, however, a sort of divine efflatus came upon it, such as the Hebrews later believed empowered heroes to extraordinary warlike deeds; [48a] then the phallus became erect and able to impart a divine ecstasy—to give woman the mystic element [48b] in her religion. She, accordingly, deified, not man, but the erect phallus. This became her contribution to palæolithic theology. This theology of the old stone age had the advantage of being first in the field. The workings of the ordinary human mind follow everywhere similar, if not identical, patterns. What we can trace in Europe happened among men of the old stone age in all parts of the world. The mother goddess and phallic emblems, developed in various ways, crop up in Semitic, Egyptian, Babylonian, Hittite, Phrygian, Cretan, Indian, Chinese and Japanese religions as well as those of the Greeks, Slavs, Celts, and Teutons, and probably those of the New World.[49] It is the sub-soil on which all religion has fed. As man's mental powers expanded and his culture developed, animism, totemism, tribal life, agriculture, and many other elements came in to modify and expand the simple ideas of palæolithic man, but until about the time of the Babylonian exile [50] it was not eliminated from the religion of Israel. It continued sporadically in other Mediterranean countries until the beginning of the Christian era, and still survives among some of the obscure sects of India.[51]

If we are right in our interpretation of the content of palæolithic religion (and we believe we are), religion did not originate

48a See Judges, 3: 10; 6: 34; 14: 6, 19; 15: 14, and 39.

48b This seems to be psychologically normal. There is "a stage of development at which the emotions are centered on a body-part of the object rather than on the person in his totality." See Abraham Karl, "A Short Study of the Libido," translated in *Selected Papers*, London, 1927, p. 489.

49 For Semitic and Egyptian territory, see G. A. Barton, *Semitic and Hamitic Origins*, Philadelphia, 1934, passim; for India, Sir John Marshall, *Mohenjo-daro*, London, p. 51 ff., and Pls. XII, XCIV, and XCV, G. F. Moore, *History of Religions*, I, New York, 1913, p. 345, and E. W. Hopkins, *History of Religions*, New York, 1918, pp. 207, 209; for various European countries, Hopkins, *op. cit.*, pp. 144, 162, and 538; for Chinese, Hopkins, *ibid.*, p. 246, and Japanese, *ibid.*, p. 283; in New Zealand phallicism survived (Hopkins, *ibid.*, p. 60) and the influence of the mother goddess in Polynesia is shown in the fact that sacred trees are adorned with red ochre (Hopkins, *ibid.*, p. 60). For Mexico, see *Lewis Spence, The Gods of Mexico*, New York, 1923, pp. 178 and 181. For the whole European and Asiatic field, J. Lewis, *The Great Mother*, when published.

50 2 Kgs. 23: 6–20.

51 *Cf.* G. F. Moore, *History of Religions*, New York, 1913, p. 343.

in animism, in fear, in appreciation of the *mysterium tremendum*, and much less in ancestor worship. It had its origin in the *mysterium feminium*. Its beginnings antedate animism and the recognition of the importance of ancestors. Palæolithic men like animals had their fears, but these fears gave them no thrill of exaltation that could be counted the mystic heart of religion. Grant that the *mysterium feminium* is a *mysterium tremendum*, it is not the *mysterium tremendum* of which Otto was thinking. In the later history of religion all the three factors just enumerated have functioned and have played their part in shaping different phases of religious experience. Fear has intensified devotion in many religions and in some languages has been the word for worship. Among some peoples ancestor worship has played its part. Where power of reflection has been highly developed, the magnificent mysteries of nature have intensified both devotion and mystic experience, but religion began in something much more elementary, tangible, and concrete than any of these.

Doubtless it will be most repugnant to many to think that religion—the sphere of our holiest experiences, the inspirer of our sublimest aspirations, and the ground of our immortal hopes—had its beginnings in the appreciation of anything so material and sensuous as sex. No doubt that, had the invention of a way to make human bodies been left to refined ladies and gentlemen of our modern era, they would have sought long and arduously for some less materialistic and obscene way to accomplish the end.[52] "To the pure, all things are pure," it has been said. Purity cannot be denied to God, and the perpetua-

[52] J. H. Leuba, a non-theist, sought in his *Psychology of Religious Mysticism*, New York, 1925, Ch. V, to discredit all mystical experience by explaining it as the result of neuroses created by thwarted sex-desire. He makes much of the love-terms and metaphors employed by mystics and by many Christian hymns. That those terms are not employed by accident is clear. The endearing metaphors of parent and child and lover and beloved express, as no other terms could, the most precious, intense, and universal experiences of the race. That there is a genetic connection between the materialistic and mystical use of the terms, I believe has now been demonstrated. That, however, in no way invalidates the high spiritual significance of the terms as employed by Christian mystics. Leuba assumes that there is no God—no First Cause —no Spiritual Being—who has been revealing Himself to emerging man as man has been able to comprehend him. The evidence collected by Leuba does not prove this assumption; neither does that collected by me prove the opposite. Leuba's assumption, however, leaves the universe and all human life inexplicable. The assumption that mystics have felt such a presence fills the universe with hope and purpose. What the highest mystics have done is to transmute the primitive sex-terminology into something higher.

tion of the race through the functioning of sex is God's way. It need not surprise us, therefore, to learn that the initial acts by which God has revealed Himself to man—of that spiritually creative process by which he is forming the inner life of man—the man within the man—He accomplished through the mental experiences caused by the exercise of those creative functions by which He creates the physical frame of man.

This is not the place to follow the subject in detail into later history. That has been done in many works.[53] It may not be out of place, however, to record a few observations. As long millennia passed and man's intelligence developed, he began to subjugate the powers of nature to his service and to make generalizations in thought. Animism and totemism became phases of his mental outlook, he learned to till the soil, he domesticated animals, he learned that physical paternity was a reality. Through all this the worship of the great mother and communion with her through the enjoyment of sex persisted. She might be explained as mother earth, she might be embodied in a tree, or in grain, she might in her temples need hordes of consecrated hierodules to bring her communion within the reach of ordinary men or of sodomites to bring it within the reach of women, but its essential features survived. She might be called by a different name in every country or in every shrine—by the use of epithets she might be split up into a multitude of goddesses, but the core of her worship went back everywhere to the concepts born in palæolithic time. When male gods developed, they might be her son or her husband, but in either case they needed her. Forms of social organization—matrilinear,[54] patrilinear—variations of marriage customs—exogamy, endogamy, monogamy, polygamy—might develop; her importance and functions, however explained and however in this locality or that a male deity might be regarded as head of the pantheon, were never in total eclipse. Here and there family affection, ethical sense, and the birth of a social conscience, raised the general level of morals above the sex-rituals embalmed in her cult; nevertheless

53 The writer's *Semitic Origins*, his *Semitic and Hamitic Origins*, and Lewis's *The Great Mother* need only be mentioned here.

54 If the explanation of palæolithic religion set forth above be true, ignorance of paternity and belief in the mother goddess produced matrilinear descent or "matriarchy" and not polyandry as I formerly thought; *Semitic Origins*, Chs. II and III. The mother goddess created matriarchy. *Cf. Semitic and Hamitic Origins*, pp. 110, 111.

such was the cult's appeal and so hoary its associations that it persisted. Not until the eighth century B.C. did men anywhere become able to think of God as a pure spirit apart from a physical embodiment.[55] The perception of God as a spiritual and ethical being began in Israel in the eighth century B.C. and during the five centuries following was also made by Hindoos, Persians, Chinese, and Greeks. Coincident with this perception Israel's prophets began to denounce the sex-worship of this primeval cult, and in the reform of king Josiah, in 621 B.C., it was eliminated from Israel's worship. It forms no part of Judaism. In India, from time immemorial individuals have renounced family and sex, and have sought through ascetic renunciation and contemplation to come into unity with Atman or Brahman. Sex features found no place in Jainism and Buddhism, the two great heresies which began in the sixth century B.C. Nevertheless in that land of multitudinous races and creeds, in the Siva cult the sex motive still survives and its ritual is practiced in obscure places. Outside of Israel the cults of the mother goddess survived in the Mediterranean area until the beginning of the Christian era. As the teaching of Jesus lifted sex-morality to a level higher than even that of Judaism, during the early centuries of our era many, in revolt from the license afforded by sex-cults, conceived an aversion even to monogamous marriage, and fled to monasteries and nunneries. The pendulum swung too far. Nevertheless, just as in his after life a child forgets the umbilical cord and even the experiences of nursing, so religion, to become spiritual and to fulfil its function of creating ideal ethical persons, has had to move far away from its physical palæolithic beginnings.

One advantage of recovering the real story of religious origins and of tracing the tortuous upward course of religious history, is that it gives ground for optimism. If a cult so material, so sensuous, although it appealed so strongly to human passion and was hallowed with hoary associations, has been outgrown and sloughed off, it gives ground for belief that the many things, material and petty, which hamper religion today may in the future be in like manner left behind.[56] "It doth not yet

[55] For proof see G. A. Barton, *Christ and Evolution*, Philadelphia, 1934, p. 31.

[56] Another example of a once universal fundamental religious practice that has been left behind by all the higher religions is animal sacrifice, which has no place in Christianity, Buddhism, Confucianism, or Mohammedanism.

appear what we shall be; we know that when He shall be manifested, we shall be like him.'' [57]

[57] By seeking the *Sitz im Leben* of prehistoric religious ideas, we may, as a result of our investigation, postulate the following hypothetical, but probable, sequence for the emergence of religious conceptions.

I. Palæolithic Period.
Woman and the phallus deified.

II. Early Neolithic Period.
Hunting was still the means of livelihood. The dog and hawk were domesticated. Animism was born. Spirits were believed to live in trees and springs. Totemism was born. Animal deities followed.

III. Later Neolithic Period.
Agriculture was developed. The earth was conceived as a great mother. Physical paternity was discovered. Water was thought to be *spermata zoa*. With it masculine river and sky deities fertilized the great mother. All those ideas sprang into being which greet us in the next period, the early bronze age, when religion became literate.

THE TORTURE OF CAPTIVES BY THE INDIANS OF EASTERN NORTH AMERICA

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ABSTRACT

The Indians of eastern North America evinced great emotional satisfactions from the prolonged tortures often inflicted upon war captives. Such behavior must be evaluated in terms of the motivations imposed by the various cultures of the several tribes, particularly with respect to the social and religious connotations of the war patterns. This analysis suggests that many groups tortured primarily in retaliation against the whites, who had immediately introduced the common European practise of burning at the stake, and against the Iroquois. Both along the lower Mississippi River and among the Iroquois torture seems to have had strong religious significance with the concept of human sacrifice underlying the act. However the complexes were otherwise so markedly unlike that direct diffusion between the Iroquois and lower Mississippi River groups is not indicated. Both had definite, but different, parallels with sacrifice in Middle America which may indicate a distant common origin for them.

INTRODUCTION

SATISFACTIONS derived through the infliction of physical pain upon a human being are of great psychological significance. In our own culture pleasure arising from the suffering of another is not socially acceptable. When it is experienced, it must be disguised, and even though openly manifest must be rationalized. Torturing, literally twisting, was ingeniously developed at least as early as Greek and Roman times, and perhaps reached its height during the Inquisition of the Middle Ages. The practice had obvious utilitarian motives. As a punishment for certain crimes, torture probably acted as a strong influence for the preservation of order. Of possibly more importance, first to the State and later to the Church, was the information obtained on treason and heresy, from those subjected to it. In addition, the necessity for confession, and hence the ultimate salvation of the soul of the victim, was one of the rationalizations of the Inquisition. Elaborate legal regulations were considered necessary so that the infliction of torture would be purposeful and not merely represent individual brutality. Nevertheless, per-

sonal satisfactions, not only to the actual torturers but also vicariously to much of the population, were undoubtedly present although culturally condemned.

Many American Indian tribes did not so inhibit the enjoyment of physical suffering, and the pleasure of whole groups of people was apparently unrestrained during the long tortures inflicted upon a captive. However, it must be realized that overt manifestations of behavior can be properly evaluated only after an understanding of the underlying motivations, which implies a detailed functional and historical analysis of the culture. Not until the integration of torture in the culture as a whole is known can the factors affecting individual acts of cruelty be appreciated. The psychological significance of pleasure derived from the sufferings of another is not within the scope of this study, which is to determine, as far as the available material permits, the integration of torture in the cultures under consideration and the significance to the individual implied by such relationships.

There were two specific, material trophies of a successful war party recognized by the eastern Indians. These were scalps and captives. Attainment of honors through the coup as practised by the Plains Indians was entirely unknown. Important as these trophies were, their value did not outweigh the loss of a man, and a leader who failed to bring back his party intact was condemned as unsuccessful. Plunder was of little significance as a motive for war since all the tribes were approximately at the same level of material culture. Naturally, after white contact, knives, guns and other European goods of great value to the Indians were taken as booty. The horse was of slight importance in the woodlands and did not become an object of raids such as occurred on the plains of the west. Territorial aggrandizement per se does not seem to have been a cause of war, but rather to have resulted from tribal movements originating from other motives. The region was rich in natural resources such as game, fish and edible plants, and agriculture was practised, yet the population was sparse. Consequently, there would have been little economic reason for the usurpation of lands. White interference changed the original war pattern greatly, as all the tribes were brought into the Spanish, French and English race for empire. Trade relations for goods, slaves

and scalps were established, and it became advantageous for the Indian to side with one or the other of the European nations and to fight for his benefactor with the European weapons which were provided.

Supernatural elements were intimately associated with the war complex over the entire region. Purificatory rites were usually required both before and after war, and there were many strict prohibitions while actually on the war-path. Sexual continence on the part of the warriors might be demanded during the entire period of hostilities. A sacred bundle might be carried to war by the leader, or the warriors might have individual talismen for supernatural protection and assistance. Communications from the supernatural world through the medium of omens or dreams were carefully noted and acted upon, even to the extent of forcing a return from the war-path after days of travel. Failure would usually be attributed to lack of purity on the part of the leader or some other member of the party. Scalps and captives often had significance either in connection with mourning and the appeasement of ghosts, or as offerings to the supernatural. In general, success in war meant the satisfaction of supernatural compulsives which in turn led to social recognition, and was about the only way such recognition could be obtained.

While scalps and captives were the basis of the war trophy pattern of all the tribes within the region, distinctly different attitudes towards these trophies may be discerned. Therefore, torture, which was primarily one method of disposal of one kind of trophy, that is captives, can be evaluated properly only in terms of the more inclusive war trophy complex.

Historical considerations are obviously pertinent to a full understanding of cultural phenomena, and especially so with respect to traits associated with warfare, an activity implying group contacts and often involving the assimilation of captives. History reconstructed from a comparative analysis of cultures at a specific point of time is merely suggestive, and must be used cautiously to supplement conclusions based upon internal evidence. This is particularly true where only small segments of the total culture are under consideration, as in this paper. However, it is hoped that detailed studies of enough more material will eventually be forthcoming so that evidence which

can now be treated as no more than historical possibility will be confirmed, thereby further clarifying the specific behavior patterns under discussion here.

The utilization of actual historical records is an entirely different matter. In so far as these either contain descriptions of torturing or fail to mention it at all, they are important and have been discussed at some length.

The accounts contained in the Spanish, French, English and American records of exploration, colonization, trading and missionary activity in the Southeast are almost the sole sources of information. By the first half of the nineteenth century the aboriginal population had been thoroughly defeated and the remnants removed to reservations. Consequently, there are no full accounts of any of the cultures by competent ethnologists, except in so far as traits survived almost inextricably mixed with white and negro elements. The entire war complex, including torturing, obviously would remain as nothing more than a vague tradition. Although the early records of untrained observers cannot be expected to furnish fully satisfactory accounts of cultural details, nevertheless some are reasonably full and often surprisingly acute. Religious beliefs, social organization and ceremonial procedures are poorly described, but on the other hand the torture of captives is a trait which would seem to be particularly subject to casual observation, at least in its main outlines. The conflict between the invading whites and the "barbarians" was of constant concern to the missionaries, colonists and traders. The brutalities practised upon anyone so luckless as to fall into their hands were an ever present threat. We might well expect an overdrawn picture of Indian cruelties to serve as an excuse to wipe out these "savages" who resisted the depredations of the whites, and in general this may well be true. On the contrary, understatements of the dangers would be unusual except possibly in a few cases of colonization propaganda.

Additional sources may be tapped although it is unlikely that further data of particular significance will be forthcoming. Archæological evidence may possibly be uncovered which will throw some light on the subject of torture, but it should be realized that too much cannot be expected. Little can be deduced from charred bones and burnt stakes which will permit

the reconstruction of torture or human sacrifice. Scalps have not been found in the mounds, and it is unlikely that they will be uncovered. Most of the material associated with the war complex was of a perishable nature, thus precluding the possibility of archæological evidence.

The infliction of pain upon another person may not be confined solely to the treatment of war captives, and other such acts may have a bearing upon any final psychological interpretation of torture. However, it has been thought desirable to limit this paper to the torture of war captives. Such behavior as individual brutality to wife, child or other person, institutionalized punishment for adultery or other crimes, infliction of pain in ceremonies such as initiation rites, and self-torture of the Plains Sun Dance type, are not of immediate concern.

The specific region under consideration is that commonly known as the Southeastern Culture Area of North America. This may be defined geographically as lying east of the Mississippi River and south of the northern parts of Virginia and Kentucky. In addition, the southern Caddoans of Louisiana and eastern Texas and the Iroquois of Pennsylvania and New York have been included. Material pertaining to the adjacent groups, such as the Algonkians, the Plains Area and Middle America, will be used only in so far as it contributes to the clarification of the problem. The material has thus been limited to a geographically contiguous area within which actual contacts of all the groups can be predicated and the uncertainties inherent in spatially discontinuous distributions eliminated. The various manifestations of torture have been correlated, as far as the data permit, with other phases of the war complex and the sacrifice of human life. Not only is an essentially continuous distribution of torture involved, but it seems to have been absent in adjacent regions except where it had penetrated in relatively recent times as a retaliation against enemies who used it.

Unfortunately the material is extremely scanty for many tribes, and for others which would normally be included, such as the Atakapa and Chitimacha, the insufficiency of data has compelled their exclusion from consideration.

I. Descriptive Material on Torture

Detailed descriptions of the tortures inflicted by the Indians of Southeastern North America are relatively rare; in most cases their acts are merely referred to as cruel or horrible without any indication whether or not prolonged tortures rather than either brutal slaying or mutilation of the dead were involved. In striking contrast to this situation is the abundance of detailed accounts of the prolonged agonies inflicted by the Northern Iroquois upon their enemies.

The following descriptions, arranged in approximately chronological order, include any behavior towards captives which might indicate that torturing was involved. As the absence of the practise at a particular time and place is also pertinent to an understanding of the torture complex in the region, data which indicate that it was lacking have been included.

The first unquestioned landing of the Europeans upon the mainland of the New World was the expedition of Ponce de Leon [1] which first touched Florida in the neighborhood of the present St. Augustine [2] and then coasted southward without attempting to make land again owing to the hostility of the natives. While off the southwest tip of the Florida peninsula Ponce de Leon was addressed in Spanish by an Indian,[3] thus making it apparent that the atrocious cruelty of the Spanish for some twenty years in the West Indies had become known to the inhabitants of the mainland prior to the discovery of the continent by the whites. Their determined resistance and lack of superstitious fear of the white man indicated, according to Lowery, an appreciation of the treatment to be expected.[4]

During the succeeding years numerous expeditions such as those of Miruelo and Cordova which were driven off by the Indians,[5] Alvarez de Pineda which remained some forty days at a large town at the mouth of a great river,[6] Gomez,[7] Verazzani,[8]

[1] The records are not clear as to the extent of prior explorations, including those of the Cabots and Americus Vespucci. For a discussion of the whole problem see Lowery, 1911, especially pp. 124, 127, 130 and Winsor, II, p. 232.

[2] Fairbanks, p. 16.

[3] Lowery, 1911, p. 142.

[4] Lowery, 1911, p. 144.

[5] Lowery, 1911, p. 149.

[6] Mississippi, Lowery, 1911, p. 150—Mobile Bay, Swanton, Southeastern Indians, p. 5.

[7] Lowery, 1911, p. 169.

[8] Verarzanus, John, Relation of.

and most likely other unauthorized and therefore unrecorded explorations were in all probability largely in the nature of slave raids.[9] Scant information on the aboriginies is found in any of these accounts but they do emphasize the possible extent of Spanish cruelty. The treatment accorded the inhabitants[10] is perhaps typified by the following action of an expedition sent out by Ayllon in 1519:

> Although Ayllon had charged Gordillo to cultivate friendly relations with the Indians of any new land he might discover, Gordillo joined with Quexos in seizing some seventy of the natives, with whom they sailed away. . . .[11]

An account of the Indians[12] met by a later Ayllon expedition to the east coast obtained from a captive does not specifically refer to torture but the following description is perhaps indicative of its absence:

> The inhabitants of this country do not eat human flesh; the prisoners of war are enslaved by the victors.[13]

Much of what later was assumed to be first contact with the Indians had unquestionably been preceded by marauding expeditions which would account for the immediate hostility towards later colonizers.[14]

With Cabeza de Vaca's account of the Narvaez expedition of 1527 we begin to find more solid ethnographic fact. Narvaez and his 600 colonists and soldiers probably landed just west of Tampa Bay.[15] Probably many unknown Spaniards survived the destruction of this expedition and lived for many years among the Indians. The effect of such residence, as well as the journey of the four men to the Pacific Coast, must have had definite repercussions on the aboriginal cultures. Vaca, who has furnished some excellent ethnographic material on the tribes he encountered, fails to mention torture, although he refers to

[9] Lowery, 1911, p. 171, suggests that many such unrecorded raids as far north as Chesapeake Bay in quest of slaves may have occurred.

[10] Identified as Cusabo by Swanton, *Early History*, p. 131.

[11] Winsor, II, p. 239. Also see Martyr, II, p. 256.

[12] Identified as Siouans by Swanton, *Early History*, p. 42.

[13] Martyr, II, p. 261.

[14] Mooney's conclusion probably typifies the situation along the entire coast: "Jamestown Colonists landed among a people who already knew and hated the whites." *Powhatan Confederacy*, p. 129.

[15] For a discussion of this location see Lowery, 1911, pp. 453–455.

Indian slayings of Spaniards and of other Indians, and makes observations on war practices.[16]

John Ortis, a member of the Narvaez expedition, was found twelve years later by De Soto soon after he landed in Florida. From him comes probably the earliest account of the treatment of a captive by the Indians of the New World, in this case Timucuans:[17]

> Ucita commanded to bind John Ortis hand and foot upon four stakes aloft upon a raft, and to make a fire under him, that there he might be burned.[18]

Almost an identical account is given by Garcilaso de la Vega:

> . . . cacique, who, enraged that Ortis could endure so many divers hardships, ordered, on a day of entertainment, that they should kindle a fire in the middle of the public square, that they should put a griddle upon the fire; and that they should put his slave upon it, in order to burn him alive.[19]

Neither Biedma, Ranjel, nor De Soto refer to this incident although they all speak of finding Ortis. He was saved through the intervention of the wife and daughter of the Chief.

Three companions had been seized with Ortis but had not survived the initial abuse:

> Harriga guarded with care his prisoners, in order to increase by their death the pleasures of a feast which he was to celebrate, in a few days, according to the custom of the country. The time of the ceremonies arrived, he commanded that the Spaniards, entirely naked, should be compelled to run by turns from one extremity of the public square to the other, that at times arrows should be shot at them, in order that their death might be the slower, their pain the more exquisite, and the rejoicing more noted and of longer duration.[20]

Apart from the above there is no mention of Indian torture in any of the De Soto chronicles.[21] This is quite amazing for if torture in any form had been practised there is every reason to assume that it would have been noted and commented upon by the Spaniards. Swanton in discussing this striking omission writes:

16 Bandelier, Ad. f., *The Journey of Alvar Munez Cabeza de Vaca.*

17 According to Swanton, *Early History,* p. 379.

18 Elvas, p. 125.

19 Vega, pp. 259–260.

20 Vega, p. 259.

21 The Indians of Alabama mimicked throwing a man into the fire. As they first hit him on the head, it does not resemble torture, Elvas, p. 165.

. . . I am inclined to think it indicates that much of this cruelty was of later introduction.[22]

Although accounts of treatment accorded to those captured are not common in the chronicles, yet in several cases of Spaniards taken by the Indians there are no suggestions of torture.[23] An observation on the treatment of prisoners by the Guachoia [24] is interesting:

But they principally exercised their cruelty upon the suckling infants and upon old men; they first tore from the latter their clothes, and shot them to death with arrows which they generally aimed at the parts which show the difference of sex. As for the infants they threw them by the legs into the air, and shot them to death with their arrows before they fell to the ground.[25]

This is an excellent description of brutality without torture and suggests that had the torture practices noted more than 150 years later in the lower Mississippi region been present they would have been seen and commented upon by the Spaniards with De Soto.

The absence of reference to torture by the Indians does not complete the picture for the "Civilized" invaders unquestionably did practise it on the Indians for the customary European motives of abstracting information or supplies and as a punishment for refractory behavior. Some examples of this seem pertinent:

All the rest he commanded to be put to death, being tied to a stake in the midst of the market-place; and the Indians of Paracossi did shoot them to death.[26]

This treatment was a punishment for revolt. During the search for Cofitachequi:

. . . and he brought four or five Indians, and not one would show any knowledge of his lord's village or discover it, although they burnt one of them alive before the others, and all suffered that martyrdom for not revealing it.[27]

At Coste De Soto seized some chiefs:

22 Swanton, *The Ethnological Value,* p. 576.
23 Vega, p. 327.
24 Identified by Swanton at Natchez, *Relation of the Southeast,* p. 63.
25 Vega, p. 436.
26 Elvas, p. 133. Also see Ranjel, p. 76.
27 Ranjel, p. 97. Also see Elvas, p. 42.

. . . and he threatened them, and said that he would burn them all because they had laid hands on the Christians.[28]

While wintering the first year at Apalachee, Indians ambushed them often and:

. . . although the Spaniards pursued them and burned them they were never willing to make peace. If their hands and noses were cut off they made no more account of it. . . .[29]

Biedma mentions that they came to a large river which entered into the bay called Chuse and:

In the meanwhile the Indians killed one of the governor's guards. The governor punished the cacique for it, and threatened to burn him alive if he did not deliver up the murderers.[30]

While he was west of the Mississippi:

This Indian led him two days out of the way. The Governor commanded to torture him.[31]

At the time of boat building in the Natchez area the Governor feared a plot by some visiting Indians and consequently he ordered one to be detained secretly:

The Governor commanded him to be put to torture, to make him confess whether the Indians practise treason or no.[32]

Furthermore the Spaniards had fierce dogs used to track down and then tear the natives to pieces.[33] It is not difficult to believe that De Soto employed such methods on the Indians many times not recorded in the narratives. The stake and fire were common in Europe at that period. From the time of the earliest settlements in the West Indies Spanish brutality was a fact. Champlain on his trip to the West Indies and Mexico in 1599 not only speaks of Spaniards burning Indians but even furnishes a sketch showing it.[34]

Following De Soto there was a period of years during which many shipwrecks occurred on the southern end of Florida and the survivors were either slain or enslaved.[35] Among them was

[28] Ranjel, p. 110.
[29] Ranjel, p. 80.
[30] Biedma, p. 102. The threat against the cacique but not the mode of punishment is mentioned in Elvas, p. 199.
[31] He confessed and was cast to the dogs. Elvas, p. 199.
[32] Elvas, p. 204.
[33] Ranjel, p. 60, and Vega, p. 275, give descriptions of this.
[34] Champlain, I, p. 63 and Plate LX.
[35] Lowery, 1911, p. 352.

a boy, Fontaneda, who lived some seventeen years with the Indians until finally rescued in 1566. From him we have almost the only available account of the Calusa. No torture is mentioned and the following suggests its absence:

> For the natives who took them (shipwrecked Spaniards) would order them to dance and sing; and as they were not understood and the Indians themselves are very artful—they thought the Christians were obstinate, and unwilling to do so. And so they would kill them, and report to the cacique that for their disobedience they had been slain.[36]

In 1559 a fleet of 13 vessels and 1500 people led by Tristan de Luna attempted to colonize northeastern Alabama after a preliminary exploration of the coast in 1558. They covered much of the Gulf Coast by land and water and finally fortified a settlement on Pensacola Bay.[37] Remains of towns destroyed by De Soto were seen and stragglers from his army found. An attempt was made in 1560 to penetrate into the interior in accordance with the original plans. They met with the Cocas[38] and aided them in a war against their enemy, the Napochies.[39] In a deserted Napochies village the Cocas:

> . . . went from house to house looking for someone like infuriated lions and they found only a poor strange Indian . . . they tortured the poor Indian till they left him dying.[40]

This seems to have been rather a slaying than torture. Lowery uses the expression "despatched with blows" for the act.[41]

This colonization scheme failed, and when in 1561 Villafane offered to take those who wished to go with him to Saint Elena, the colony was deserted.[42] Villafane explored the coast of Georgia and Carolina for a short time but no contacts were made with the natives.[43]

French Hueguenots under the command of Ribault reached Florida[44] in 1562 and coasted northward to a large river named by them the River May and now identified as the St. Johns River.[45] Here they had friendly contact with the Indians and

36 Fontaneda, p. 22.
37 Lowery, 1911, pp. 358–359.
38 The Coosa, a Creek tribe according to Swanton, *Early History*, p. 230.
39 A Choctaw speaking tribe according to Swanton, *Southeastern Indians*, p. 5.
40 Swanton, *Early History*, p. 237—citing Davila Padilla, *Historia*, pp. 205–217.
41 Lowery, 1911, p. 367.
42 Lowery, 1911, p. 374.
43 Lowery, 1911, pp. 374–375.
44 Near the present St. Augustine, Lowery, 1905, p. 32.
45 Swanton, *Early History*, p. 48.

then proceeded north, finally establishing a small settlement at Port Royal near the present Beaufort, South Carolina,[46] which was soon abandoned. Little data on the Indians is found in the account of this voyage except that they were friendly and had greeted the French "... very gently and with great humanitie."[47]

A second French expedition under Laudonnière, who had accompanied Ribault previously, erected a fortified settlement on the St. Johns River in Florida in 1564. The artist Le Moyne accompanied this colony and his illustrations and descriptions together with the descriptions of Laudonnière furnish valuable material on the Timucua Indians. In spite of much information on war practices and ceremonials, torturing is not noted. On the contrary there is much to suggest its absence. A certain chief is spoken of as:

> . . . a man cruell in warre, but pitiful in the execution of his furie. For hee tooke the prisoners to mercy, being content to mark them on the left arme with a great marke like unto a seale, . . . then hee let them goe without any more hurt.[48]

Immediate death for those taken in war was apparently customary[49] although some, particularly women and children, might be taken prisoners.[50] Charlevoix sums up the Florida Indians, not however from personal experience, as follows:

> They are not as cruel to their prisoners as those of Canada; and although, like the latter, cannibals, they do not carry inhumanity so far as to make the sufferings of a miserable wretch a spectacle of pleasure, or torture an art. Women and children taken in war, they are satisfied with keeping in slavery; men they immolate to the sun, and deem it a religious duty to eat the flesh of these victims.[51]

Ribault arrived in Florida on a second voyage in 1565 bringing much needed aid to the year-old colony but was not able to prevent its destruction by the Spanish under Mendenez. In 1567 Gourges led a French expedition to avenge the massacre and succeeded in destroying a Spanish settlement in the vicinity of the present St. Augustine. Both of these expeditions used

46 Lowery, 1905, p. 402—Swanton, *Early History*, p. 49.
47 Ribault, p. 101.
48 Laudonnière, p. 458.
49 Le Moyne, p. 7—Laudonnière, pp. 413, 494.
50 Laudonnière, pp. 464, 469.
51 Charlevoix, *History and General Description*, I, p. 138.

Indian allies in their ruthless warfare but there are no accusations of Indian cruelty.[52]

Mendenez made extended explorations along the coast of Florida and the Pardo expedition penetrated far into the interior of Georgia. Forts and Missions were established by force of arms. Little ethnography is found in the records and there are no accounts of Indian cruelty.[53] The Missions established by Mendenez among the Calusa and Timucua of Florida, and among the Guale of the coast of Georgia, suffered many privations and sustained much conflict with the Indians in which numerous whites were slain. Any idea of retaliation by torture on the part of the Indians was apparently lacking. The Jesuit Missionaries left on Chesapeake Bay in 1570 initially had peaceful relations with the natives but were later slaughtered, as far as can be ascertained, without torture. When Mendenez attempted to succor this Virginia Mission in 1571 he caught eight Indians whom he hung for the murder of the Jesuits.[54]

Upon the return of the first voyage sent to America under the command of Captain Barlowe, at the instigation of Sir Walter Raleigh in 1584, the Indians in the vicinity of Pamlico Sound, North Carolina, were reported as kind and loving.[55] While little weight can be attached to a statement which was propaganda for colonization, nevertheless there is no indication in any of the accounts of the later voyages in 1585, 1586 and 1587 under Lane and White or in the descriptions of Hariot, all of which contain excellent ethnographic material, of torture being practised by the Indians.

The Indians of Guale, Georgia, rose in rebellion against the Spanish Missions in 1597 and killed many priests and Christianized Indians, apparently without torture except in the case of a Father who had been kept as a slave and then:

"They tied him to a post and put much wood under him."[56] He was spared finally and exchanged for a prisoner held by the Spaniards.[57]

52 For an account of this French-Spanish conflict see Laudonnière, *A Notable Historie,* and Lowery, 1905.

53 See Lowery, 1905, for an account of these explorations.

54 For a discussion of these Missions see Lowery, 1905, pp. 339 f.

55 Barlowe, *The First Voyage.*

56 Swanton, *Early History,* p. 87—citing Barcia.

57 For an account of this rebellion see Swanton, *Early History,* pp. 85 f.—citing Barcia.

The accounts of Spelman, Percy, Strachey, Smith, and later Beverley, of the English settlements in Virginia give no indication that any of the Southeastern Algonkians used torture on captives. Fear on the part of the whites of suffering such a fate seems to have been entirely absent. There is however a suggestion that these Indians were familiar with the Iroquois torturing:

For the heads of all those rivers, especially the Pattawomekes, the Pauturuntes, the Sasquesahanocks, the Tockwoughes, are continually tormeted by them (the Massawomekes, or Five Nation Iroquois): of whose crueltie, they generally complained. . . .[58]

Percy likewise reports that Powhatan:

. . . described also upon the same Sea (to the west), a mighty Nation called Pocoughtronack, a fierce Nation that did eate men, and warred with the people. . . .[59]

Men were evidently seldom taken captive by Southeastern Algonkians but Smith remarks:

Yet the Werowances, women and children, they put not to death, but keepe them captives.[60]

Slaughter of all enemies seems to have been the more general rule:

. . . they destroy man, woman, and child, to prevent all future resentments.[61]

Smith, and later Beverley, emphasized that neither age nor sex was spared in the massacre of the English instigated by Oppechancanough in 1622 but that all were immediately slain.[62] Smith further elaborates upon this tragedy of 1622:

. . . they fell againe upon the dead bodies, making as well as they could a fresh murder, defacing, dragging, and mangling their carkases into many peeces, and carrying some parts away in derision, ...[63]

The resentment on the part of the Indians indicated by such behavior would surely have resulted in torturing if the practice had been known to them.

[58] Smith, *A Map of Virginia,* p. 105.
[59] Percy, p. 49.
[60] Smith, *A Map of Virginia,* p. 106. Also mentioned by Strachey, p. 107.
[61] Beverley, p. 150.
[62] Smith, *The Generall Historie,* p. 358. Beverley, p. 40.
[63] Smith, *The Generall Historie,* p. 359.

Smith himself cites a case where he applied torture to an Indian:

The Counsell concluded, that I should terrifie them with some torture, to know if I could know their intent. The next day, I bound one in hold to the maine Mast: and presenting sixe Muskets . . . forced him to desire life. . . . I affrighted the other, first with the rack, then with Muskets; . . . .[64]

A letter dated 1611 from the Rev. Alexander Whitaker, who was in Virginia, attempts to dissuade Dale from exploration by citing the dangers:

. . . otherwise he (Powhatan) threatened to destroy us after a strange manner. First hee said hee would make us dumbe and then kill us. . . .[65]

Had torture been practised it would have made a much more efficacious deterrent.

Exploration of Carolina by Bland in 1650 seems to have been without any fear of torture from the Nottaway, Meherrin and Tuscarora, all of whom were Iroquois speaking. Lederer in his travels during 1669 and 1670 does not seem to have observed torturing. By 1674 English traders were penetrating far inland and one of them, Gabriel Arther, was captured by the Tomahitans, who were probably Yuchi, and:

. . . tied . . . to a stake and laid heaps of combustible canes about him to burn him. . . .[66]

In a letter dated 1675 a Bishop of Cuba describes the Indians of Florida but gives only one incident, and that from hearsay, which might be construed as torture. This referred to a tribe along the northern borders of Florida, the Chichimecos, identified by Swanton as probably Yuchi.[67]

. . . so savage and cruel that their only concern is to assault villages, Christian and Heathen, taking lives and sparing neither age, sex nor estate, roasting and eating victims.[68]

As this is at best a second-hand account of a non-Christian tribe by a Spanish Bishop, too much weight cannot be attached to it. Furthermore, straight cannibalism rather than torture may be indicated.

64 Smith, *A True Relation*, p. 67.
65 Bushnell, *Virginia from Early Records*, p. 36.
66 Alvord, p. 218.
67 Wenhold, p. 4.
68 Wenhold, p. 11.

An account of a war between the Apalachee and the Yuchi contained in a letter dated 1678 does not suggest the use of torture on the vanquished Yuchi by the Apalachee.[69]

It is only with the account by Lawson of his journey in Carolina in 1700 that we get any definite torture referred to in this section. He states that few prisoners of the Saponi, who were Eastern Siouans, escaped having lighted splinters stuck in their bodies, and then being made to dance around a fire until dead.[70] Failure to so treat prisoners in one case resulted in a terrific storm sent by the "devil." [71] Speaking in general of the Indians of North Carolina Lawson says they invented horrible cruelties prolonged as long as possible. Ignited pine splinters stuck in the body of the victim, dancing, derision, buffeting until death, and final dismemberment were usual.[72] A Huguenot traveler met the Saponi in 1715 and accuses them of inhumanly murdering all their prisoners, but does not specifically describe torture.[73] Criminals were cruelly executed by a special official in front of the rejoicing population.[74]

Lawson was himself killed by the Tuscarora in 1711. His companion Graffenreid, who was spared, does not report the method used other than to say that he was condemned to have his throat cut with his own razor.[75]

In 1704 the English waged a war of extermination against the Indians during which the Apalachee tied some other Indians to stakes and burned them alive.[76]

The earliest descriptions of Creek torturing are contained in Dr. Hewit's accounts of the cruelties practised upon captives by the Yamassee who were then raiding the Spanish settlements in Florida from their home on the north side of the Savannah River just prior to the war of 1715:

The Yamassees possessed a large territory lying backward from Port-Royal island, on the north-east side of Savanna river, ... For many years they had been accustomed to make incursions into the Spanish territories, and to wage war with the Indians within their bounds. In their return from those southern expeditions, it had been a common practice

69 Swanton, *Early History,* pp. 299 f.
70 Lawson, p. 47.
71 Lawson, p. 49.
72 Lawson, pp. 197 f.
73 Fontaine, p. 279.
74 Lawson, pp. 195 f.
75 Williamson, I, pp. 285–286.
76 Swanton, *Early History,* p. 123—citing a letter from a Spanish Governor.

with them to lurk in the woods round Augustine, until they surprised some Spaniard, and brought him prisoner to their towns. On the bodies of these unfortunate prisoners they were accustomed to exercise the most wanton barbarities; sometimes cutting them to pieces slowly, joint by joint, with knives and tomahawks; at other times burying them up to the neck underground, then standing at a distance and marking at their heads with their pointed arrows; and, at other times, binding them to a tree, and piercing the tenderest parts of their naked bodies with sharp-pointed sticks of burning wood, which last, because the most painful and excruciating method of torture, was the most common among them.[77]

However, the Yamassee and their allies united with the Spanish to drive out the English in 1715. The English captives were sometimes tortured:

John Cochran, his wife, and four children; Mr. Bray, his wife, and two children; and six more men and women, having found some friends among them, were spared for some days; but, while attempting to make their escape from them, they were retaken and put to death. Such as had no friends among them were tortured in the most shocking manner, the Indians seeming to neglect their progress towards conquest on purpose to assist in tormenting their enemies. We forbear to mention the various tortures inflicted on such as fell into their merciless fangs.[78]

After the English had driven the Yamassee south into Florida they still continued raiding into Carolina:

One party of them catched William Hooper, and killed him by degrees, by cutting off one joint of his body after another until he expired. Another party surprised Henry Quinton, Thomas Simmons, and Thomas Parmenter, and, to gratify their revenge, tortured them to death.[79]

No other cases of torture are found in the documents collected by Carroll, and none of the ones cited above were reported by eye-witnesses. The Creek and Cherokee wars, which were extremely bitter against the whites, did not bring out any incidents of torturing.

The French explorers and missionaries gradually pushed westward from the St. Lawrence during the 17th century and journeyed down the Mississippi River. Marquette and Joliet in 1673 made many ethnological observations, especially on the Illinois, but without noting any torture practices.

Tonty's accounts of La Salle's journey down the Mississippi starting in 1678, and of the search until 1691 for La Salle after

77 Carroll, I, pp. 189–191.
78 Carroll, I, p. 197.
79 Carroll, I, p. 199.

his fatal attempt to find the mouth of the river, lack any reference to torture by the tribes along the lower reaches of the Mississippi or among the Caddo to the west. Illinois [80] and Iroquois [81] were, on the contrary, both accused of the practice. Hennepin, who accompanied La Salle part of the time, likewise reported torturing by the Iroquois,[82] but also does not refer to it further south. La Salle's attempt to locate the mouth of the Mississippi from the Gulf of Mexico is described in the accounts of Joutel from which most of the early ethnography of the Caddo of eastern Texas and Louisiana is derived. The Hasinai apparently slew most of their enemies immediately. There is, however, an incident of a woman captive who:

> . . . was kept to fall a sacrifice to the rage and vengeance of the women and maids; who, having armed themselves with thick sticks, sharp pointed at the end, conducted that wretch to a by-place, where each of those furies began to torment her, sometimes with the point of their staff, and sometimes laying on her with all their might. One tore off her hair, another cut off her finger, and every one of those outrageous women endeavored to put her to some exquisite torture, to avenge the death of their husbands and kinsmen, who had been killed in the former wars; so that the unfortunate creature expected her death stroke as mercy.
>
> At last, one of them gave her a stroke with a heavy club on the head, and another ran her stake several times into her body, with which she fell down dead on the spot. Then they cut that miserable victim into morsels, and obliged some slaves of that nation they had been long possessed of, to eat them.[83]

This is the only account in Joutel which could be interpreted as indicative of torture.

Excellent descriptions of the Southern Caddoans are furnished in the letters of early Spanish Missionaries to eastern Texas in 1691, 1716, and 1722, but only one slight reference to torture is mentioned by these men who lived in intimacy with and constant danger from the Indians. In 1691 Fray Franciso Casanas de Jesus Maria wrote of the Tejas (Hasinai) as follows:

> In conclusion it may be said that these Indians practice no greater cruelty than their enemies do. They tie a captive's feet and hands to a post like a cross. Here they tear him to pieces, drinking the blood and eating the flesh half roasted.[84]

80 Tonty, *Memoir*, p. 65.
81 Tonty, *Memoir*, p. 56.
82 Hennepin, p. 198.
83 Joutel, *Historical Journal*, p. 160. See also *Relation*, pp. 379–380.
84 Hatcher, 30, p. 217.

This has interesting connotations and will be discussed more fully elsewhere. It confirms Joutel's observations on cannibalism in this region but it is questionable whether it should be identified as torture. Hidalgo and Espinosa do not mention it.[85]

An interesting account is furnished by Morfi for the Taovayas, a Southern Caddoan tribe:

. . . are excessively cruel with captives; . . . and this cruelty on their part is more an impudent reprisal, . . . than a ferocious spirit, because they only exercise it with those nations who treat their prisoners with the same barbarity; and up to the present it is not known. On the contrary, they feast them and hold them to esteem in proportion to the valor they show in their defense, ...[86]

When Gravier voyaged down the Mississippi about 1700 he commented upon the torture practices of the Iroquois,[87] but on none lower down the river. He specifically exempts the Houma from such cruelty.[88]

The accounts of d'Iberville of the first French Settlements on the Gulf beginning in 1699, which were under his command, give little ethnographic data and no indication of torture. The missionary Ru who accompanied him is much more informative, but has no remarks on any kind of torture, although several cases of human sacrifice, which will be discussed later, were noted by him. He does not suggest that any cruelty was associated with the deaths of fifty Colapissa warriors at the hands of the Chickasaw.[89]

Penicaut visited this region about 1704. He gives a description of cannibalism by the Hasinai during which the victim was secured to a frame. Torture, aside from the eating of the victim while still alive, apparently did not accompany it.[90] He also mentions that seventeen Spaniards were taken as prisoners to Mobile in 1719 and clubbed to death, which is perhaps evidence that torture was not used by this group.[91] However, Penicaut furnishes the earliest description of torture known for this region:

85 See Hatcher.
86 Morfi, p. 11.
87 *Jesuit Relations*, 65, pp. 35–37.
88 *Jesuit Relations*, 65, p. 151.
89 Ru, p. 66.
90 Penicaut, *Annals*, p. 121.
91 Penicaut, *Annals*, p. 148.

These savages who are named Coroas, are the most cruel of all those of Louisiana. They are almost always hunting or at war, and when they have taken one of their enemies alive, they fasten him to a frame, which is composed of two poles 8 feet in height, 5 feet apart, the two hands being well bound above and the two feet below, in the form of a St. Andrew's cross. The poor wretch being fastened thus completely naked, the entire village collects around him. They have a fire lighted in this place, where they have placed pieces of iron such as old gun barrels, shovels, or the iron part of axes and other similar things, to make them red hot, and when they are thoroughly reddened they rub them upon his back, arms, thighs, and legs; they then lay bare the skin all around his head as far as the ears, tearing it off from him by force. They fill this skin with burning coals, which they replace on his head; they put the ends of his fingers into their lighted pipes, which they smoke, and tear out his nails, tormenting him thus until he is dead.[92]

The Coroas, or Koroa, were a group speaking the Tunican language. The fact that their cruelty impressed Penicaut as unusual suggests the rarity if not the absence of torture in the region. The related Tunica were accused of burning a Natchez woman on a frame set up in New Orleans with the permission of the French.[93]

La Harpe, who settled in Louisiana in 1718, is a reasonably full source for the Caddo and other Texas tribes. He gives nothing on torture for this region, although he does repeat a report that the Quapaw had burned some Iroquois alive in 1706.[94]

Charlevoix, visiting the lower Mississippi in 1721–1722, states that it was the fate of all Natchez captives to be burned. He supplies no details other than that these captives were first compelled to dance in front of the temple.[95] The Quapaw are accused by him also of burning those prisoners not saved for adoption.[96]

Le Petit repeats the same brief description of Natchez torture as recorded by Charlevoix.[97] It is only with the very full account by Du Pratz that we get a satisfactory picture of the actual treatment of a victim:

If they are able to carry away any of the enemies of their nation they are received honorably. If these are women or children they are en-

92 Penicaut, *Relation*, pp. 458–459.
93 Romans, p. 96.
94 Harpe, p. 35.
95 Charlevoix, *Historical Journal*, p. 167.
96 Charlevoix, *Historical Journal*, p. 128.
97 *Jesuit Relations*, 68, p. 149.

slaved. They serve in this capacity after their hair has been cut extremely short. But if it is a man that they have made prisoner the joy is general and their glory is at its height. On arriving near their nation they make the war cry three times repeated, and in this case, however wearied the warriors may be, they go at once to hunt for the three poles which are necessary for the construction of the fatal instrument on which they are going to make the enemy they have taken die, I mean the frame (cadre) on which they cruelly immolate the unfortunate victim of their vengeance.

Of these three poles which are about ten feet long, two are set in the earth. They are straight and a good pace apart from each other. They assure themselves that they are firmly placed. The third is cut in halves in order to cross the two that are already planted. The first is 2 feet above the earth and the other 5 feet above the first. These poles, thus adjusted and bound together as strongly as possible as is necessary, form, indeed, a frame, and it is from that fact that the French have taken a name of this gallows machine. The natives tie the victim to the foot of this frame,[98] and when he is there he sings the death song until his scalp is taken. After the warriors have thus tied him they are permitted to go to eat. The victim, if he so desires, may then take his last meal. The old warriors guard him. Each one can look at him, but he is not allowed to speak to him, still less to insult him.

When the warriors have finished their meal they come to the place where the frame is planted to which the victim is tied. They make him advance a little and turn his entire body around in order that the people may see him. The one who has taken him gives a blow of his wooden war club below the back part of his head, making the death cry while removing the scalp in the best manner he is able without tearing it.

After the scalp has been taken from the victim, they tie a cord to each of his wrists, throw the ends of the cords over the crosspiece, which many take and draw in order to pull him up while others lift him, placing his feet on the crosspiece below and tying him to the corners of the square. They do the same to his hands at the upper corners of the square in such a manner that the victim in this position has his body free and entirely bare, and the four limbs form a St. Andrew's cross.

From the time they begin to take the scalp from the victim the young people go in search of dry canes, crush them, and make packages or bundles of the entire length of the canes which they bind in many places. They bring other dry canes, also, which have been neither crushed nor bound which the warriors make use of against the victim.

The one who took him is the first one to take a single crushed cane, light it and burn the place he may choose. But he devotes himself especially to burn the arm with which he (the prisoner) had best defended himself. Another comes and burns another place. These, with their pipes filled with dry and burning tobacco, burn him about the foot. Those heat a nail red hot, with which they pierce his foot. All, in fact,

[98] For an illustration of this "cadre" see Du Pratz, II, p. 429. Dumont, p. 78, states that the frame was located in front of the temple.

one after the other, revenge themselves as best they are able on this victim, who, so long as strength remains to him, employs it in singing the death song, which, when closely examined, is found to consist of grievous cries, tears, and groans. Usage decides and governs everything.

Some have been seen to sing and suffer continually during three days and three nights without anyone giving them a glass of water to quench their thirst, and it is not permitted to anyone to give it to them, even should they ask for it, which they never do, without doubt, because they know that the hearts of their enemies are inflexible. In fact, it must be admitted that if the natives are good friends during peace, they are in war irreconcilable enemies.

It sometimes happens that a young woman who may have lost her husband in war, seeing the victim when he arrives completely naked and without means of concealing his defects, if he has any, demands him for her husband, and he is granted to her on the spot.

It also happens that when he suffers too long a pitying woman lights a cane torch, and when it is burning well, makes him die in an instant by putting this torch to the most sensitive place, and the tragic scene is in this way ended.[99]

In his account of the 1729 massacre of the French by the Natchez, Dumont does not indicate that any were tortured, but a few days later a white was burned on the frame, evidently subjected to the treatment described by Du Pratz.[100] In 1730 a French woman was burned.[101]

Bossu does not refer to the Natchez as torturing, but he gives an account of Illinois cruelty to both Fox Indians and Englishmen.[102] He likewise accuses the Quapaw of torturing.[103]

The French seem not only to have countenanced cruelty on the part of their Indian allies, but even to have indulged in such behavior themselves, for in a letter of Governor Perier after the Natchez rebellion there is the following remark:

Latterly I burned four men, and two women, and sent the rest to St. Domingo.[104]

There are conflicting reports about the Choctaw during the eighteenth century. Le Petit reports that they burned some Negroes alive in 1730,[105] and an early anonymous account of

[99] Du Pratz, II, pp. 428–432.
[100] Dumont, pp. 78 f.
[101] Dumont, p. 98.
[102] Bossu, I, pp. 130, 186.
[103] Bossu, I, pp. 105 f.
[104] Gayarre, I, p. 438.
[105] *Jesuit Relations*, 68, p. 199.

Louisiana, translated by Swanton, dating from the early part of the century states:

When they are able to bring home prisoners, they have them burned at their village, and it is a great joy to them when this happens.[106]

On the contrary, Romans clears the Choctaw of any such cruelty:

They never exercised so much cruelty as the other savages; they almost always brought them home to show them, and then dispatched them with a bullet or hatchet.[107]

He contrasts this conduct with that of the Creeks whom he accuses of permitting hardly a captive to escape terrible tortures.[108] Dumont gives an incident of the Choctaw burning a Natchez woman whom they tied to a bundle of canes when they were aiding the French to avenge the massacre of 1729.[109]

In 1756 a Colapissa was surrendered to the Choctaw to replace a murdered man. This man was killed in great anger but without torture.[110]

In 1722 four Frenchmen and an Indian slave were captured by the Chickasaw. They wrote that their captors were treating them well:

This is surely a sign that the Indians want peace, for when a prisoner cannot work, it is their custom to kill them.[111]

The earliest account of Chickasaw torture refers to the time of the French war against them in 1736 when twenty-six French soldiers and seven officers were tied to stakes and burned by slow fires.[112]

By far the most complete account of Chickasaw torture, and, in fact, for any Mushkogean tribe, is given by Adair who traded in this region after 1736 and upon whom we must rely for much of our data on Southeastern warfare. The following is his description of the Chickasaw torture of a prisoner:

It has been long too feelingly known, that instead of observing the generous and hospitable part of the laws of war, and saving the unfortunate

106 Swanton, *An Early Account,* p. 67.
107 Romans, p. 75.
108 Romans, p. 97.
109 Dumont, p. 89.
110 Bossu, I, p. 171.
111 Mereness, pp. 31, 33.
112 Bossu, I, p. 311—Dumont, p. 114.

who fall into their power, that they generally devote their captives to death, with the most agonizing tortures. No representation can possibly be given, so shocking to humanity, as their unmerciful method of tormenting their devoted prisoner; and as it so contrary to the standard of the rest of the known world, I shall relate the circumstances, so far as to convey proper information thereof to the reader. When the company return from war, and come in view of their own town, they follow the leader one by one, in a direct line, each a few yards behind the other, to magnify their triumph. If they have not succeeded, or any of their warriors are lost, they return quite silent; but if they are all safe, and have succeeded, they fire off the Indian platoon, by one, two, and three at a time, whooping and insulting their prisoners. They camp near their town all night, in a large square plot of ground, marked for the purpose, with a high war-pole fixed in the middle of it, to which they secure their prisoners. Next day they go to the leader's house in a very solemn procession, but they stay without, round his red-painted war-pole, till they have determined concerning the fate of their prisoners. If any one of the captives should be fortunate enough to get loose, run into the house of the arch-magus, or to a town of refuge, he by ancient custom, is saved from the fiery torture—these places being a sure asylum to them if they were invaded, and taken, but not to invaders, because they came to shed blood.

The young prisoners are saved, if not devoted while the company was sanctifying themselves for their expedition; but if the latter be the case, they are condemned, and tied to the dreadful stake, one at a time. The victors first strip their miserable captives, and put on their feet a pair of bear-skin maccaseenes, with the black hairy part outwards; others fasten with a grape-vine, a burning fire-brand to the pole, a little above the reach of their heads. Then they know their doom—deep black, and burning fire, are fixed seals of their death-warrant. Their punishment is always left to the women; and on account of their false standard of education, they are no way backward in their office, but perform it to the entire satisfaction of the greedy eyes of the spectators. Each of them prepares for the dreadful rejoicing, a long bundle of dry canes, or the heart of fat pitch-pine, and as the victims are lead to the stake, the women and their young ones beat them with these in a most barbarous manner. Happy would it be for the miserable creatures, if their sufferings ended here, or a merciful tomahawk finished them at one stroke; but this shameful treatment is a prelude to future sufferings.

The death signal being given, preparations are made for acting a more tragic part. The victim's arms are fast pinioned, and a strong grape-vine is tied around his neck, to the top of the war-pole, allowing him to track around, about fifteen yards. They fix some tough clay on his head, to secure the scalp from the blazing torches. Unspeakable pleasure now fills the exulting crowd of spectators, and the circle fills with the Amazon and merciless executioners. The suffering warrior however is not dismayed; with an exulting manly voice he sings the war song! And with gallant contempt he tramples the rattling gourd with pebbles

in it to pieces, and outbraves even death itself. The women make a furious onset with their burning torches; his pain is soon so excruciating, that he rushes out from the pole, with the fury of the most savage beast of prey, and with the vine sweeps down all before him, kicking, biting, and trampling them, with the greatest despite. The circle immediately fills again, either with the same, or fresh persons; they attack him on every side—now he runs to the pole for shelter, but the flames pursue him. Then with champing teeth, and sparkling eye-balls, he breaks through their contracted circle afresh, and acts every part, that the highest courage, most raging fury, and blackest despair can prompt him to. But he is sure to be overpowered by numbers, and after some time the fire affects his tender parts. Then they pour over him a quantity of cold water, and allow him a proper time of respite, till his spirits recover, and he is capable of suffering new tortures. Then the like cruelties are repeated till he falls down, and happily becomes insensible of pain. Now they scalp him, in the manner before described; dismember, and carry off all the exterior branches of the body (pudendis non exceptis), in shameful, and savage triumph. This is the most favorable treatment their devoted captives receive; it would be too shocking to humanity either to give, or peruse, every particular of their conduct in such doleful tragedies—nothing can equal these scenes, but those of the merciful Romanish inquisition.

Not a soul, of whatever age or sex, manifests the least pity during the prisoner's tortures; the women sing with religious joy, all the while they are torturing the devoted victim, and peals of laughter resound through the crowded theatre—especially if he fears to die. But a warrior puts on a bold austere countenance, and carries it through all his pains;—as long as he can, he whoops and outbraves the enemy, describing his own martial deeds against them, and those of his nation, who he threatens will force many of them to eat fire in revenge of his fate, as he himself had often done to some of their relations at their cost.[113]

Except for the above, Adair gives few details of actual torturing, merely mentioning that all Indians practised such cruelties.[114] All captives were not tortured, for Adair indicates that only those who were pretty well advanced in life were so treated.[115] However, the enemy might be "devoted" to death prior to the setting out of the war party. The warriors made a vow to kill all met on a certain trail, or during a certain time, or belonging to a certain nation.[116] This devotion to death did not specifically require torture but perhaps might sometimes include it. A prisoner, whether or not condemned to torture, could be saved by escaping to a town of refuge or to the house

[113] Adair, pp. 388–391.
[114] Adair, p. 154.
[115] Adair, p. 389.
[116] Adair, p. 155.

of the Arch-magus.[117] These towns of refuge, or White Towns, offered a haven to criminals as well as to captives [118] in contrast to the Red, or War, Towns which Swanton has suggested were a recent addition by migration to the culture.[119]

Adair furnishes the following account of the Cherokee torturing a party of Mohawks who had raided southward in 1747, his information coming from a trader supposedly among the Choctaw at the time:

But they were overpowered by numbers, captivated, and put to the most exquisite tortures of fire, amidst a prodigious crowd of exulting foes. . . . when they were tied to the stake, the younger of the two discovering our traders on a hill pretty near, addressed them in English, and entreated them to redeem their lives. The elder immediately spoke to him, in his own language, to desist—on this he collected himself, and became composed like a stoic, manifesting an indifference to life or death, pleasure or pain, according to their standard of martial virtue: and their dying behavior did not reflect the least dishonor on their former gallant actions. All the pangs of fiery torture served only to refine their manly spirits; and as it was out of the power of the traders to redeem them, they according to our usual custom retired, as soon as the Indians began the diabolical tragedy.[120]

A traditional account of Cherokee torture of a Seneca chief furnishes perhaps the most detail:

They (the Cherokee) tied him (the Seneca chief) and carried him to two women of the tribe who had the power to decide what should be done with him. . . . They decided to burn the soles of his feet until they were blistered, then to put grains of corn under the skin and to chase him with clubs until they had beaten him to death.

They stripped him and burnt his feet. Then they tied a bark rope around his waist, with an old man to hold the other end, and made him run between two lines of people, with clubs in their hands. When they gave the word to start . . . (he escaped).[121]

The above account is interesting mainly for the reference to the power of deciding the fate of captives invested in the "Be-

117 Adair, pp. 156, 161, 417.

118 Schoolcraft, V, p. 279—Bartram, *Travels*, p. 389.

119 Swanton, *Social Organization*, pp. 274 f.

120 Adair, p. 384.

121 Mooney, *Myths*, p. 360. Dr. Fenton has kindly communicated to the writer a practically identical tradition obtained by him in 1934 from John Jimmerson, except that it applied to the Choctaw, not the Cherokee. About the time of the wars of 1750–1760 a Seneca chief was captured, had both his feet blistered, and then escaped from the row of warriors. The principal difference between these traditions, other than the identity of the enemy, is in the condemnation to torture. In the case of the Choctaw this was done by a council of warriors.

loved Women'' and is confirmed by Timberlake who was with the Cherokee about 1760:

Old warriors likewise, or war-women, who can no longer go to war, but have distinguished themselves in their younger days, have the title of Beloved. This is the only titles females can enjoy; but it abundantly recompenses them, by the power they acquire by it which is so great that they can, by the wave of a swan's wing, deliver a wretch condemned by the council and already tied to the stake.[122]

In 1776 they threatened to torture a white woman:

She was bound, taken to the top of one of the mounds, and was about to be burned, when Nancy Ward, then exercising in the nation the functions of the Beloved or Pretty Woman, interfered and pronounced her pardon.[123]

During this same campaign instigated by the British the Cherokees captured a boy:

Moore (the boy) was carried prisoner to the Indian towns, and was tortured to death by burning.[124]

The ferocious reputation of the Cherokee among the Whites of Carolina is perhaps best expressed by Logan:

. . . the midnight alarms and horrid butcheries of helpless women and children, and the terrible scenes of their more dreadful tortures in captivity and at the stake, have not yet received due notice at the hands of any chronicler.[125]

Nevertheless, there is no adequate description of the torture practices available. It cannot be too explicitly assumed that a stake was actually used because Indian torture was often characterized as burning at the stake although in most cases there was no actual binding of the captive.

Adair also refers to an English narrative stating that the Shawnee tortured men, women and children, but did not attempt the chastity of women for fear of offending their god.[126] Shawnee torture of a Creek is mentioned without details.[127] A rather full description of the torture of an Iroquois appears in Adair:

[122] Timberlake, p. 71.
[123] Ramsey, p. 157.
[124] Ramsey, p. 158.
[125] Logan, p. 205.
[126] Adair, p. 164.
[127] Adair, p. 392—Logan, pp. 250–251.

The Shawano also captivated a warrior of the Anantooeah, and put him to the stake, according to their usual cruel solemnities. Having unconcernedly suffered much sharp torture, he told them with scorn, they did not know how to punish a noted enemy, therefore he was willing to teach them, and would confirm the truth of his assertion, if they allowed him the opportunity. Accordingly he requested of them a pipe and some tobacco, which was given him: as soon as he lighted it, he sat down, naked as he was, on the women's burning torches, that were within his circle, and continued smoking his pipe without the least discomposure—on this a head warrior leaped up, and said they had seen plain enough, that he was a warrior, and not afraid of dying; nor should he have died, only that he was both spoiled by the fire, and devoted to it by their laws: however, though he was a very dangerous enemy, and his nation a treacherous people, it should appear that they paid a regard to bravery, even in one who was marked over the body with war streaks, at the cost of many lives of their beloved kindred. And then by way of favor, he, with his friendly tomahawk, instantly put an end to all his pains:—though the merciful but bloody instrument was ready some minutes before he gave the blow, yet I was assured, the spectators could not perceive the sufferer to change, either his posture, or his steady erect countenance, in the least.[128]

Shortly after Mary Jemison's capture by the Shawnee in 1755, she saw the fragments of burnt bodies hanging on a pole in a Shawnee village on the banks of the Ohio.[129] She describes how the Shawnee tortured a white captive in 1759 for what she believed to be the sole purpose of exulting at his distress:

They first made him stand up, while they slowly pared his ears, and split them into strings. They then made a number of slight incisions in his face, and bound him on the ground, rolled him in the dirt, and rubbed it in his wounds, some of them at the same time whipping him with small rods.[130]

Mary Jemison, at this time an adopted Iroquois, persuaded them to release him.

In 1782 the Shawnee burned the British Officer Crawford. A council decided his fate which was to be fastened by the wrists to a thick post in the midst of a circle of fire and slowly roasted. Squaws also threw embers on him. This proceeding was watched with pleasure by the renegade Simon Girty.[131] A captive was tied to a tree in 1790 and embers applied to deep cuts made in

[128] Adair, pp. 392–393.
[129] Seaver, p. 56.
[130] Seaver, p. 75.
[131] Seaver, pp. 194 f.

his body. He was then released and killed by torches applied to his bowels.[132]

Ridout describes the torturing by the Shawnee of a captive who had been given to one of the warriors in 1788. This warrior wished to kill him instead of adopting him. The victim ran out of a house, entirely naked and with his ears cut off. The Indians chased him to a hill where he was tortured by fire for three hours until dead. Later the wigwams were "beaten" to drive away the spirits of the dead.[133]

In 1790 Hay was surprised that a captive taken to avenge the murder of a Shawnee had been adopted rather than burned, a fate which he believed was inflicted in all such cases.[134]

The Prophet told Trowbridge that he had witnessed the burning of two white men about 1794 near Fort Wayne. No fire was kindled to hasten their deaths but they were slowly killed with brands. All prisoners painted black before their arrival in the village were tortured unless released by the great peace woman.[134a] A tradition states that a Cannibalistic Society burned and ate prisoners although it is not clear that the burning took place while the victim was alive.[134b]

A pseudo-historical account of Shawnee torture was obtained in 1935 by Dr. E. Voegelin from her informant. The bravery of the Catawba victims is emphasized, and the method of burning them is unique in this region. They were compelled to walk back and forth in a small space surrounded by fire. It was also stated that one division of the Shawnee were not allowed to burn captives.[135]

When Milfort arrived among the Creeks in the latter part of the eighteenth century he said he found them still practising torture by burning their captives and that he persuaded them to abandon the custom.[136] Bartram describes the Chunky Yards of the Creeks with their "slave posts" to which captives condemned to be burned had once been tied but which were stated to be no longer in use although some old traders remembered such burnings.[137] He saw no such Chunky Yards in the Chero-

[132] Spencer, p. 16.
[133] Edgar, pp. 363–364.
[134] Hay, p. 248.
[134a] Voegelin, p. 21.
[134b] Voegelin, pp. 53 f., 64.
[135] Personal communication through the kindness of Dr. E. Voegelin.
[136] Milfort, p. 219.
[137] Bartram, *Observations,* pp. 35–36.

kee towns but found remains of them in the ancient sites.[138] In another place, Bartram was assured by the oldest traders that they had never seen an instance of burning though they said it had occurred formerly.[139] He was told by an old Spaniard that both the Creeks and the Spaniards had been cruel to prisoners, and that the Indians had burned captives to appease the spirits of their slain relatives.[140]

Swan does not describe Creek torture but its occurrence may be inferred from his statement that captives who succeeded in reaching a town of refuge escaped such a fate.[141] In Hawkin's accounts of the Creeks in the period between 1796 and 1806 no references are made to torture. Recent informants of Swanton said they still remembered the Creek slave posts for securing torture victims,[142] and also told him a tradition that the Indians of Alabama had once tortured one of their own tribe who had been adopted by the Choctaw and had then fought against his own people.[143]

Speck was told by informants in 1906 that the Yuchi had burned captives at the stake in the square grounds, the captor having the right to determine the fate of his prisoner.[144]

Archæological evidences of torture or human sacrifice from the mounds is very inconclusive. Thomas thought that there were some indications of the practice in Illinois and Tennessee, based upon occasional remains of burnt stakes and associated charred bones.[145] He did not believe that this indicated human sacrifice,[146] but the basis for such a fine distinction is not evident. More recently Shetrone concluded:

> Despite the fact that early writers attributed human sacrifice to the Hopewell and other highly evolved mound-building peoples, there is no real evidence and scant probability that it was practised among them.[147]

Contact of Indian and White in the north was much more

138 Bartram, *Observations*, p. 36—Swanton was told by informants that these posts were still faintly remembered as being in the shape of a war club. Swanton, *Social Organization*, p. 437.

139 Bartram, *Travels*, p. 213.

140 Bartram, *Travels*, pp. 488–489.

141 Schoolcraft, V, p. 279.

142 Swanton, *Social Organization*, p. 437.

143 Swanton, *Social Organization*, p. 426.

144 Speck, *Yuchi*, pp. 85, 116.

145 Thomas, p. 676.

146 Thomas, p. 675.

147 Shetrone, p. 100.

gradual than in the Southeast and without the accompanying slavery. The explorations of the Cabots in 1494 and 1497 furnish practically no data on the Indians. The contacts made by Cartier on his three voyages to the St. Lawrence between 1534 and 1541 were essentially friendly and consequently the war practices and the treatment accorded captives by the Iroquois and Algonkians were not observed. Except for these slight contacts, the entire sixteenth century was a period of neglect of this part of the New World by the Europeans.

With the opening years of the seventeenth century the Algonkians and Iroquois of the region adjacent to the St. Lawrence quickly became familiar to the French Explorers and Missionaries. Champlain in 1603 and the succeeding years vividly describes the tortures of the Montagnais, Huron, Five Nation Iroquois, and even the Susquehanna of Pennsylvania in terms no different than those employed in the great number of cases cited in the Jesuit Relations all during the century. Death by torture was clearly a factor of extreme unpleasantness to be reckoned with in every contact with these northern Indians. The details varied somewhat from group to group but the following description of Huron treatment of one of the Five Nation Iroquois from Le Jeune's Relation of 1637 is reasonably typical of the tortures inflicted by Iroquoian speaking people of upper New York State and Canada:

Meanwhile the sun which was fast declining, admonished us to withdraw to the place where this cruel Tragedy was to be enacted. It was in the cabin of one Atsan, who is the great war Captain; Therefore it is called "Otinotsiskiaj ondaon," meaning, "the house of cut-off heads." It is there all the Councils of war are held; as to the house where the affairs of the country, and those which relate only to the observance of order, are transacted, it is called "Endionrra Ondaon," "house of the Council." . . . Towards 8 o'clock in the evening, eleven fires were lighted along the cabin, about one brass distant from each other. The people gathered immediately, the old men taking places above, upon a sort of platform, which extends, on both sides, the entire length of the cabin. The young men were below, but were so crowded that they were almost piled upon one another, so that there was hardly a passage along the fires. Cries of joy resounded on all sides; each provided himself, one with a firebrand, another with a piece of bark, to burn the victim. Before he was brought in, the Captain Aenons encouraged all to do their duty, representing to them the importance of this act, which was viewed, he said, by the Sun and by the God of war. He ordered that at first they should burn only his legs, so that he might hold out until daybreak; also for that

night they were not to go and amuse themselves in the woods. He had hardly finished when the victim entered. I leave you to imagine the terror that seized him at the sight of these preparations. The cries redoubled at his arrival; he is made to sit down upon a mat, his hands are bound, then he rises and makes a tour of the cabin, singing and dancing; no one burns him this time, but also this is the limit of his rest—one can hardly tell what he will endure up to the time when they cut off his head. He had no sooner returned to his place than the war Captain took his robe and said, "Oteiondi"—speaking of a Captain—"will despoil him of the robe which I hold"; and added, "The Atachonchronons will cut off his head, which will be given to Ondessone, with one arm and the liver to make a feast." Behold his sentence thus pronounced. After this each one armed himself with a brand, or a piece of burning bark, and he began to walk, or rather to run, around the fires; each one struggled to burn him as he passed. Meanwhile, he shrieked like a lost soul; the whole crowd imitated his cries, or rather smothered them with horrible shouts. One must be there, to see a living picture of Hell. The whole cabin appeared as if on fire; and, althwart the flames and dense smoke that issued therefrom, these barbarians—crowding one upon the other, howling at the top of their voices, with firebrands in their hands, their eyes flashing with rage and fury—seemed like so many demons who would give no respite to this poor wretch. They often stopped him at the other end of the cabin, some of them taking his hands and breaking the bones thereof by sheer force; others pierced his ears with sticks which they left in them; others bound his wrists with cords which they tied roughly, pulling at each end of the cord with all their might. Did he make the round and pause for a little breath, he was made to repose upon hot ashes and burning coals. . . . But God permitted that on the seventh round of the cabin his strength should fail him. After he had reposed a short time upon the embers, they tried to make him rise as usual, but he did not stir; and one of these butchers having applied a brand to his loins, he was seized with a fainting fit, and would never have risen again if the young men had been permitted to have their way, for they had already begun to stir up the fire about him, as if to burn him. But the Captains prevented them from going any farther, and ordered them to cease tormenting him, saying it was important that he should see the daylight. They had him lifted upon a mat, most of the fires were extinguished, and many of the people went away. Now there was a little respite for our sufferer, and some consolation for us. . . . While he was in this condition, their only thought was to make him return to his senses, giving him many drinks composed of pure water only. At the end of an hour he began to revive a little, and to open his eyes; he was forthwith commanded to sing. He did this at first in a broken and, as it were, dying voice; but finally he sang so loud that he could be heard outside the cabin. The youth assembled again; they talk to him, they make him sit up—in a word they begin to act worse than before. For me to describe in detail all he endured during the rest of the night, would be almost impossible; we suffered enough in forcing ourselves to see a part of it. Of the rest

we judged from their talk; and the smoke issuing from his roasted flesh revealed to us something of which we could not have borne the sight. One thing, in my opinion, greatly increased his consciousness of suffering—that anger and rage did not appear upon the faces of those who were tormenting him, but rather gentleness and humanity, their words expressing only raillery or tokens of friendship and goodwill. There was no strife as to who should burn him—each one took his turn; thus they gave themselves leisure to meditate some new devise to make him feel the fire more keenly. They hardly burned him anywhere except in the legs, but these, to be sure, they reduced to a wretched state, the flesh being all in shreds. Some applied burning brands to them and did not withdraw them until he uttered loud cries; and, as soon as he ceased shrieking, they again began to burn him, repeating it seven or eight times—often reviving the fire, which they held close against the flesh, by blowing upon it. Others bound cords around him and then set them on fire, thus burning him slowly and causing him the keenest agony. There were some who made him put his feet on red-hot hatchets, and then pressed down on them. You could have heard the flesh hiss, and seen the smoke which issued therefrom rise even to the roof of the cabin. They struck him with clubs upon the head, and passed small sticks through his ears; they broke the rest of his fingers; they stirred up the fire all around his feet. No one spared himself, and each one strove to surpass his companion in cruelty. But, as I have said, what was most calculated in all this to plunge him into despair, was their raillery, and the compliments they paid him when they approached to burn him. This one said to him, "Here, uncle, I must burn thee"; and afterwards this uncle found himself changed into a canoe. "Come," said he, "let us caulk and pitch my canoe, it is a beautiful new canoe which I lately traded for; I must stop all the water holes well," and meanwhile he was passing the brand all along his legs. Another one asked him, "Come, uncle, where do you prefer that I should burn you?" and this poor sufferer had to indicate some particular place. At this, another one came along and said, "For my part, I do not know anything about burning; it is a trade that I never practised," and meantime his actions were more cruel than those of the others. In the midst of this heat, there were some who tried to make him believe that he was cold. "Ah, it is not right," said one, "that my uncle should be cold; I must warm thee." Another one added, "Now as my uncle has kindly deigned to come and live among the Hurons, I must make him a present, I must give him a hatchet," and with that he jeeringly applied to his feet a red-hot hatchet. Another one likewise made him a pair of stockings from old rags, which he afterwards set on fire; and often, after having made him utter loud cries, he asked him, "And now, uncle, hast thou had enough?" And when he replied, "onnachouaten, onna," "Yes, nephew, it is enough, it is enough," these barbarians replied, "No, it is not enough," and continued to burn him at intervals, demanding of him every time if it was enough. They did not fail from time to time to give him something to eat, and to pour water

into his mouth, to make him endure until morning; and you might have seen, at the same time, green ears of corn roasting at the fire and near them red-hot hatchets; and sometimes, almost at the same moment that they were giving him of the ears to eat, they were putting the hatchets upon his feet. If he refused to eat, "Indeed," said they, "dost thou think thou art master here?" and some added, "For my part, I believe thou wert the only Captain in thy country. But let us see, wert thou not very cruel to prisoners; now just tell us, didst thou not enjoy burning them? Thou didst not think thou wert to be treated in the same way, but perhaps thou didst think thou hadst killed all the Hurons?"

Behold in part how passed the night, ... One thing that consoled us was to see the patience which he bore all this pain. In the midst of their taunts and jeers, not one abusive or impatient word escaped his lips. . . . He himself also entertained the company for a while, on the state of affairs in his country, and the death of some Hurons who had been taken in war. He did this as easily, and with a countenance as composed, as anyone there present would have showed. This availed him at least as so much dimunition of his sufferings; therefore, he said, they were doing him a great favor by asking him many questions, and that this in some measure diverted him from his troubles. As soon as day began to dawn, they lighted fires outside the village, to display there the excess of their cruelty to the sight of the Sun. The victim was lead thither. . . . Meanwhile, two of them took hold of him and made him mount a scaffold 6 or 7 feet high; 3 or 4 of these barbarians followed him. They tied him to a tree which passed across it, but in such a way he was free to turn around. There they began to burn him more cruelly than ever, leaving no part of his body to which fire was not applied at intervals. When one of these butchers began to burn him and to crowd him closely, in trying to escape him, he fell into the hands of another who gave him no better reception. From time to time they were supplied with new brands, which they thrust, all aflame, down his throat, even forcing them into his fundament. They burned his eyes; they applied red-hot hatchets to his shoulders; they hung some around his neck, which they turned now upon his back, now upon his breast, according to the position he took in order to avoid the weight of this burden. If he attempted to sit or crouch down, someone thrust a brand from under the scaffolding which soon caused him to arise. . . . They so harassed him upon all sides that they finally put him out of breath; they poured water into his mouth to strengthen his heart, and the Captains called out to him that he should take a little breath. But he remained still, his mouth open, and almost motionless. Therefore, fearing that he would die otherwise than by the knife, one cut off a foot, another a hand, and almost at the same time a third severed the head from the shoulders, throwing it into the crowd, where someone caught it to carry it to the Captain Ondessone, for whom it had been reserved, in order to make a feast therewith. As for the trunk, it remained at Arontaen, where a feast was made of it the same day. . . . On the way (home) we encoun-

tered a Savage who was carrying upon a skewer one of his half-roasted hands.[148]

Captives were usually treated brutally from the moment of capture. The physical condition of the prisoner due to this abuse might be such that he was not acceptable for adoption.[149] At times, particularly if the captive had been tentatively assigned for adoption, there might be a period prior to torturing when he was treated handsomely and feasted.[150] A Shawnee captive of the Iroquois was said to have been unharmed from the time of capture and upon arrival in the village had not been greeted with blows but had been dressed and given to three women to replace a kinsman. Adoption had apparently been in the minds of the captors from the beginning, although in this case torture was finally inflicted upon the Shawnee.[151] An exception to these reports of initial harsh treatment is the remark by Colden that he knew of no case where the captive was offered the least abuse,[152] but this is undoubtedly an overstatement. The journey to the village of the captors might take some time, and it was customary to stop at each village passed and to force the captives to run between two lines of women and children, who beat them with clubs, to a platform where they were exhibited for the amusement and abuse of the inhabitants.[153]

Women seem to have rarely been tortured, but were rather kept to repopulate the villages decimated by constant warfare.[154] There may have been some tribal differences in this respect. The Neutrals were accused of torturing women in contrast to the Hurons who, supposedly, did not do so.[155] The Mohawks may have burned only the old women.[156] The Susquehanna were accused of burning a woman too injured to be of value as a captive.[157] The Onondaga apparently made little distinction between age and sex in their torture victims,[158] even torturing a boy of about ten years.[159] In 1667 the Oneida burned four Sus-

148 *Jesuit Relations,* 13, pp. 59–79.
149 *Jesuit Relations,* 13, pp. 39 f.
150 *Jesuit Relations,* 13, pp. 37; 42, p. 177—Beauchamps, *A History,* p. 178.
151 Galinee, p. 183.
152 Schoolcraft, III, p. 188.
153 *Jesuit Relations,* 39, pp. 57, 175.
154 Beauchamps, *A History,* pp. 196 f.
155 *Jesuit Relations,* 21, p. 195.
156 Megapolensis, p. 174.
157 *Jesuit Relations,* 42, p. 189.
158 *Jesuit Relations,* 47, p. 147.
159 *Jesuit Relations,* 42, p. 189.

quehanna women,[160] and there are several other accounts of the Five Nations torturing women.[161] Father Joques gives a description of the sacrifice of a woman by the Mohawk. She was first burned and then thrown into the fire as an offering to the war god Aireskoi to assure further victories over their enemies. Pieces of this woman were then distributed to various villages to be eaten in solemn feasts during the winter.[162]

The selection of victims for torture seems to have depended largely upon their desirability for adoption. According to one of the earlier Relations, a council of old men determined the disposal of all trophies of war, including the allocation of captives for adoption and the selection of the town in which the others were to be burned.[163] It has also been stated that the uterine family through its matron decided the fate of the captive.[164] However, should a captive be adopted either through such assignment or by choice expressed by relatives of deceased warriors [165] or by others,[166] and then prove unsatisfactory he could still be given over to torture.[167] Perhaps the most common form of adoption was for a widow to replace a lost husband,[168] but should this man prove unsatisfactory after adoption he might be tortured.[169] An Iroquois chief was reputed to have adopted and subsequently burned 40 prisoners because they did not prove worthy to succeed his dead brother.[170] Another chief reputedly tortured 80 captives to the shade of his brother.[171] In 1669 an old Seneca woman gave the captive alloted to her to replace her dead son over to torture because she could not bear to see him alive.[172] So great was the power of a relative over the disposal of a captive that the wishes of the tribe might be ignored even in the face of a possible ensuing war.[173]

160 Beauchamps, *A History,* p. 219.

161 *Jesuit Relations,* 47, pp. 53–65, 35.

162 *Jesuit Relations,* 39, pp. 219–221.

163 *Jesuit Relations,* 13, p. 37.

164 Hewitt, *The League of the Iroquois,* p. 533.

165 Morgan, I, p. 333—*Jesuit Relations,* 13, p. 37 f.; 22, p. 259 f.; 31, p. 53; 42, pp. 177, 191–195—Megapolensis, p. 179—Schoolcraft, III, p. 188—Galinee, p. 183.

166 Morgan, I, p. 332.

167 Morgan, I, p. 277—Schoolcraft, III, p. 189.

168 *Jesuit Relations,* 42, p. 177—Beauchamps, *A History,* p. 199.

169 *Jesuit Relations,* 42, pp. 177–179.

170 *Jesuit Relations,* 42, pp. 191–195.

171 *Jesuit Relations,* 48, p. 169.

172 Galinee, p. 184.

173 *Jesuit Relations,* 42, p. 177.

Formal torture seems to have fallen into two distinct parts. The first was enacted in a large cabin and lasted all night.[174] This cabin has been identified, in one case, as that of the war chief which was used for war councils.[175] The victim was compelled to run around a row of fires while the young men applied brands and other forms of torture to him, reviving him when he fainted, and taking care not to cause his death, for it was essential that he last until dawn.[176] It is probable that only men were present, the younger ones taking the more active role and the elders watching the proceedings.[177] Any levity on the part of the participants, or going out into the woods for "amusement," was strictly prohibited, because the Sun and the God of War viewed the application of torture.[178]

At dawn the victim would be taken outside, where fires had been lighted, and forced to mount a platform 7 or 8 feet high. He was fastened loosely to this and tortured to death in front of the entire population.[179] Torture on the platform might occur, however, without the earlier cabin torture.[180] On one occasion, the Susquehanna made the victim mount the platform and then shoved him off into a fire, from which they rescued him, and contined to torture him.[181] Relative freedom of movement, not binding to a stake, seems to have been the general rule. The Seneca once, after six hours of applying hot irons to the victim on the platform, compelled him to run through the village for two more hours, while they beat him with brands.[182]

The victim was expected to sing and dance at all times. He did this when a farewell feast was given before torture began, in the case of his having been previously adopted, and in the cabin during the night of torture.[183] Songs were largely boasting and emphasized the lack of fear on the part of the singer. On the return journey after capture, songs were required in each village visited, and upon arrival at the home village more were

174 *Jesuit Relations,* 13, p. 59–22, p. 259—Murray, p. 75.
175 *Jesuit Relations,* 13, p. 59.
176 *Jesuit Relations,* 13, pp. 61, 65; 22, p. 263.
177 *Jesuit Relations,* 13, p. 61.
178 *Jesuit Relations,* 13, p. 61; 17, p. 159.
179 *Jesuit Relations,* 13, p. 77; 22, p. 263; 17, p. 65.
180 *Jesuit Relations,* 45, p. 257—Galinee, p. 184.
181 Lindestrom, p. 242.
182 Galinee, p. 186.
183 *Jesuit Relations,* 13, p. 37 f.

insisted upon.[184] Songs were expected all during the torture.[185] There is one account of an Algonkian who ate his own flesh, which was fed to him by an Iroquois, without showing signs of repugnance.[186] There is not enough evidence to show clearly the attitude expected of the torturers to the bravery of the victim. They seem to have expected him to sing, and forced him to do so as much as possible. Bravery might be rewarded by a form of heart or blood cannibalism. However, there are accounts which suggest that it was considered an evil omen should no pain be shown.[187] Such competition between the victim and the torturers, the torturers being under urgent need to break the spirit of the victim, appears to have been relatively rare. A tradition of a Seneca warrior captured, tortured, and finally escaping from the Choctaw, perhaps indicates the attitude expected by the Seneca of one of their own warriors in the hands of the enemy. He boasted of feeling no pain, glorified his own record for martial deeds, told them what the Seneca would do in revenge, and sang his farewell song.[188]

The methods of torturing varied considerably and showed quite a bit of ingenuity. Included among them were: applying brands, embers, and hot metal to various parts of body; putting hot sand and embers on scalped head; hanging hot hatchets about neck; tearing out hair and beard; firing cords bound around body; mutilating ears, nose, lips, eyes, tongue, and various parts of the body; searing mutilated parts of the body; biting or tearing out nails; twisting fingers off; driving skewers in finger stumps; pulling sinews out of arms; etc.[189] Mary Jemison mentions certain other methods which apparently did not appear until after 1755, as they have not been noted in the earlier sources. These included firing of pine splinters stuck in the victim, and, in the case of Thomas Boyd, pulling out the intestines.[190]

There seems to have been a definite feeling that death should

[184] *Jesuit Relations*, 22, p. 259; 39, p. 57.

[185] *Jesuit Relations*, 10, pp. 227–229; 39, pp. 57, 175; 4, p. 201—Megapolensis, p. 174—Champlain, IV, p. 100.

[186] Champlain, V, p. 311.

[187] *Jesuit Relations*, 22, p. 259.

[188] The author is indebted to Dr. W. N. Fenton for this account.

[189] Some of the more complete descriptions of these methods are found in the *Jesuit Relations*, I, pp. 271–273; 10, pp. 227–229; 13, pp. 37–79; 22, pp. 259–267; 39, pp. 57–77; etc.

[190] Seaver, pp. 37, 122.

not occur by fire or directly under torture. The sacrifice of the woman to the war god, previously noted, was an exception to this. Death by the knife was commonly required,[191] and likewise beating in the head of the victim.[192] Cutting off the head or limbs of the still living, but unconscious, victim is mentioned,[193] and the heart would sometimes be torn out before death.[194] The Susquehanna are mentioned as having released a tortured captive just before death, in order to allow the boys to shoot him while he attempted to run away.[195]

Cannibalism apparently invariably accompanied torture among all Iroquois speaking people. It was also most important to eat at solemn feasts the flesh of the woman sacrified to the war god, and it is significant that the Iroquois made a feast of bear meat to the war god as atonement for not eating captives, and promised to do so in the future if success in war were granted.[196] "Eating" of the enemy was even used as a threatening expression.[197] The Mohawks were accused, by the Dutch, of eating slain enemies,[198] and the Iroquois supposedly ate war victims as late as 1756.[199] In addition to general cannibalism with every case of torture,[200] emphasis was placed particularly upon the heart, which might be torn from the still living victim, roasted and eaten.[201] The heart might be fed to the young men of the tribe,[202] or the captive might be forced to eat his own body or that of his comrades.[203] The blood might also be drunk,[204] or fed to children,[205] or put directly into the veins through a cut.[206] This seems to have been done only if the victim had been espe-

191 *Jesuit Relations*, 13, p. 79; 17, p. 65.

192 *Jesuit Relations*, 10, pp. 227–229. Galinee, p. 186.

193 *Jesuit Relations*, 10, p. 227; 13, pp. 61, 79.

194 *Jesuit Relations*, 22, p. 265; 34, p. 27; I, p. 273.

195 Lindestrom, p. 242.

196 *Jesuit Relations*, 39, pp. 219 f.—Megapolensis, p. 177.

197 Tonty, *Memoir*, p. 57.

198 Wassenaer, p. 84.

199 Heckwelder, p. 54.

200 *Jesuit Relations*, 4, p. 201; 10, pp. 227–229; 22, pp. 253, 255, 259; 13, pp. 79, 283; 39, pp. 57, 81; 17, p. 75—Megapolensis, p. 174—Galinee, p. 186—Murray, p. 75.

201 *Jesuit Relations*, 22, p. 259; 34, p. 27.

202 *Jesuit Relations*, 10, pp. 227–229; I, p. 273.

203 Champlain, IV, p. 100, V, p. 310—*Jesuit Relations*, 34, p. 27—Hennepin, p. 198.

204 *Jesuit Relations*, 34, p. 27.

205 Hennepin, p. 198.

206 *Jesuit Relations*, 10, pp. 227–229.

cially brave. A Dutch report states that the Mohawk reserved the head and heart for the chief, while the common people ate the trunk.[207] Just the opposite was averred for the Huron who, while normally presenting the head of game to the chief, as the choice morsel, gave the head of a human victim to the meanest person in the tribe.[208] However, another observer, a few years later, states that the head of the victim was given to the chief for a feast.[209] Le Jeune mentions an interesting form of cannibalism which has a curious resemblance to the Endo-cannibalism around the Gulf of Mexico. In the feast preparatory to war, a new-born child was shot with arrows, burned, and the ashes consumed.[210]

II. Torture Practices of Contiguous Groups

The Montagnais, as allies of the Huron, tortured captives taken from the Five Nations, employing the same methods as their reputed teachers, including cannibalism, but with perhaps more restrictions on the movements of the victim, less ceremonialism, and no use either of the preliminary torture in the large cabin or of a platform.[211] The Lenape usually adopted captives and torture was, according to Heckewelder, relatively rare and only done under extreme provocation from the Iroquois whom they accused of inventing the practice.[212] There does not seem to be any good evidence that torturing was indulged in by other Eastern Algonkians. It is, therefore, quite probable that neither the New England Algonkians nor those of Canada not in contact with the Iroquois practised torture as distinct from individual brutalities. After the arrival of the Iroquois in this region certain Algonkian tribes in direct conflict with them tortured from purely retaliatory motives. Flannery reaches a similar conclusion regarding Eastern Algonkian torture:

> Lacking from Northern Algonkian. More probably due to Iroquois influence among the Coastal Algonkian.[213]

207 Megapolensis, p. 174.
208 *Jesuit Relations*, 10, p. 229.
209 *Jesuit Relations*, 13, pp. 61, 79.
210 *Jesuit Relations*, 19, p. 71.
211 *Jesuit Relations*, 5, pp. 27 f., 51 f.—Champlain, II, pp. 136 f., V, pp. 231 f.
212 Heckewelder, pp. 217 f., 343.
213 Flannery, p. 126.

Jenness attributed torture among the Indians of Canada entirely to the Iroquois.[214]

It is also probable that the Central Algonkians learned torturing from the Iroquois, and that it was not a trait of their culture prior to the Iroquois raids in the seventeenth century. The Illinois, who used approximately the same techniques, including eating the victim, as did the Iroquois, stated that they had learned from them, and did it only in retaliation.[215] Tonty was threatened with fire by the Tamaroas, an Illinois tribe, because he was mistaken for an Iroquois.[216] Penicaut credited them with killing captives with clubs and, therefore, as being less cruel than other tribes.[217] According to Bossu, referring to a later period, they burned Fox prisoners, and, in 1756, an Englishman brought back from Virginia.[218] The Ojibwa did not torture,[219] nor, apparently, did the Menomini[220] or the Sauk.[221] Forsyth reported that the Sauk and Fox treated enemies, if they did not kill them immediately, with the greatest humanity.[222] The Potawatomi seem to have tied those condemned by the council to a stake and shot them.[223] The Miami were accused of still burning captives as late as 1812, but the older practice is not clear. A Cannibalistic Society to which prisoners were given has been mentioned, but the mode of death is not stated.[224] The Shawnese Prophet told Trowbridge that he had seen the Kickapoo burn a white man about 1812. He had first been led into the village, painted black, and the next day tortured about three miles away from the village in a manner very similar to that described by Adair for the Chickasaw. His body was eaten by the torturers.[224a] This was a very late occurrence and was done in order to avenge the murder of a chief by the whites.

Torturing was certainly not characteristic of the Plains

214 Jenness, *Indians of Canada*, p. 279.
215 *Jesuit Relations*, 67, pp. 173 f.
216 Tonty, *Memoir*, p. 65.
217 Penicaut, p. 110.
218 Bossu, I, pp. 130, 186.
219 Jenness, *Indians of Canada*, p. 279.
220 Skinner, *War Customs*, p. 311.
221 Skinner, *Observations*, p. 72.
222 Blair, II, p. 97.
223 Skinner, *The Mascoutens*, pp. 40 f.
224 Trowbridge, pp. 23 f., 29.
224a Voegelin, pp. 20 f.

tribes. A recent study of the Plains war complex does not refer to such behavior,[225] and a cursory examination of some of the material pertaining to groups adjacent to the Woodlands does not indicate that it was present in the cultures. The Dakota were said to have sent captives home unharmed,[226] and, according to Perrot, while they might tie them to stakes for the boys to shoot, they never burned them except in reprisal against the Iroquois.[227] Dorsey's source for mutilation of captives at the stake by Dakota women is not given.[228] The Mandan [229] and the Omaha [230] did not torture. Mrs. Kelly, a captive among the Oglalla Sioux, writes of the horrible tortures which she not only expected but which she actually went through, although, aside from some death threats, there is no indication that she was even particularly mistreated.[231] The Winnebago seldom took prisoners unless for adoption, and an enemy not selected for this was slain at once.[232] The Pawnee are accused by Dunbar of delighting in tortures "like all Indians," but captives were said to have been unusual and no specific details are furnished.[233]

The Quapaw, who should probably have been included among the tribes of the lower Mississippi River, did torture those not selected by the women for adoption. After compelling captives to dance and sing, they were scalped and fastened to a frame made of two posts and a crosspiece on which they were tormented by the young people.[234] In 1706 they burned some Hurons.[235]

The non-Caddoan tribes of Eastern Texas and the Gulf Coast have been accused of torturing, but, except for the Karankawa, no details are available. The Karankawa, according to the only description found, seem to have had cannibalism of the still living victim as the motivation for cruelty:

[225] Smith, Marian W.
[226] *Jesuit Relations,* 55, p. 181—Blair, I, p. 161, footnote.
[227] Blair, I, p. 169.
[228] Dorsey, *Omaha Sociology,* p. 313.
[229] Will and Spinden, p. 123.
[230] Dorsey, *Omaha Sociology,* p. 332—Fletcher, p. 603.
[231] Kelly.
[232] Schoolcraft, IV, p. 53.
[233] Dunbar, Par. 5.
[234] Bossu, I, pp. 105 f.
[235] Harpe, p. 35.

. . . they drive a big strong stake deep into the ground; to this they securely tie the unhappy prisoner; they build a log fire all around him; all of the rancheria, the tribe or confederation arrive, and when they sound the funeral instrument called a cayman, all begin to dance in a circle carrying in their hands well sharpened knives of iron (fierro) or flint, or a piece of shell. When they see fit they go up to the patient, cut off a piece of his flesh, pass it over the fire and dripping with blood, they eat it in sight of the victim, ... In this way they go on tearing the victim to pieces until he dies. . . . After they eat all of the flesh, they divide the bones among themselves, and those who are able to get a piece, go about continually gnawing and sucking it, until they consume it. Sometimes they hang the prisoner up by his feet, building a fire under him, let him roast, and then slowly eat him. Others make little pegs from the pine of which there is an abundance on the coast, and stick them into the body of the captive, set them on fire, and when they are burned off, eat the larded corpse.[236]

III. Summary of Torture Material

The material which has been presented seems to indicate that four patterns of torture can be defined within the region and that these were geographically delimited. They may be classified on the basis of the method of securing the victim as Frame torture among the tribes of the lower Mississippi River, Platform torture of the Northern Iroquois, Pole torture of the Chickasaw and Stake torture by several of the remaining groups. There is little descriptive data on Stake torture other than mere statements that the victim was tied to a stake and burned to death. As will be pointed out later, there is evidence to suggest that Pole and Stake torture probably do not represent distinct complexes. The following table defines the more significant differences between these torture patterns, certain aberrations being temporarily ignored. Other elements of interest in the torture complex are not subject to such comparative treatment but will be discussed separately.

[236] Morfi, pp. 51–52.

	Frame	Platform	Pole	Stake
Method of securing victim	Bound by wrists and ankles to frame	Free to move at all times	Fastened to pole with large radius of movement	Bound to a stake
Place of torture	In front of temple (?)	In cabin of War Chief all night. On platform outside at dawn	At last camp outside village	Within village or where convenient
Treatment prior to torture	Neither abused nor insulted	Continuously abused	Continuously abused	Continuously abused
Duration of torture	Up to several days	Many hours	Few hours	Moments (?)
Torture inflicted by:	Men only	Men in cabin, entire population on platform at dawn	Women only	Entire population (?)
Manner of death	Under torture	By knife or blow. Not under torture	Under torture	Consumed by fire
Scalping of victim	Before torture began	Part of torture	After death	
Cannibalism on victim	Lacking	Customary	Lacking	Lacking
Ceremonial elements	Ceremonial scalping. Touching with brands by captor. Dancing in front of temple. Lack of abuse. Use of frame. Inflicted by men only	Circumspect behavior by torturers. Dancing and singing of victim. Death on platform at sunrise. Death by knife or blow. Cardiac features. Cannibalism	Lacking	Lacking

IV. War Trophy Patterns

Torture was basically one method of disposing of war trophies, specifically captives. Scalps might also serve as trophies. Instead of being tortured, captives might be slain soon after seizure, kept as slaves, adopted into the tribe, serve as human sacrifice victims or be devoured, without any deliberate torture accompanying these acts. The different attitudes expressed by such a variety of behavior patterns were reflected in the treatment of scalps which might serve as sacrifice offer-

ings, as a means of appeasing ghosts of slain warriors and closing the mourning period of relatives, or merely as symbols of bravery worn by the scalper. The act of torturing cannot be evaluated abstracted from its context in the total war trophy pattern and it will therefore be necessary to examine the related elements in order to clarify the behavior exhibited in the torture of captives.

An analysis of the available material, unquestionably scanty in many respect, indicates that three basically distinct patterns of behavior towards war trophies were exhibited in the region under consideration. These may be defined broadly as Old Southeastern, Intrusive Southeastern and Iroquois.

Old Southeastern Pattern

The tribes grouped under this pattern are the Southern Caddoans, Natchez, Taensa, Tunica, Koroa, Calusa, Timucua, Eastern Siouans, Southeastern Algonkians and Yuchi.

Scalps, or "Heads," seem to have been treated primarily as sacrificial offerings to the supernatural rather than for ghost appeasement or as badges of war prowess. The connections of scalp trophies with the temples emphasize this sacrificial attitude. After singing to the "heads" hung in trees the Hasinai buried them in the ashes of the perpetual fire maintained in the temple and offered food to them.[237] Scalps were also hung on poles during the dance prior to war [238] and were exhibited in front of the houses.[239] Food and tobacco were offered the scalps which were carried by the women in processions.[240] It is only in the Plains-like culture of the Wichita that scalps seem to have had the function of closing the mourning period.[241] The Pawnee used scalps ceremonially as offerings.[242]

This sacrificial attitude towards scalps and their connection with the temples was observed among the lower Mississippi River tribes as early as the time of De Soto:

237 Hatcher, 31, pp. 174, 217—Morfi, pp. 39, 40. Joutel may have seen temples as he remarked on the large separate huts used for ceremonies and public gatherings. Joutel, *Historical Journal*, p. 148 and *Relation*, p. 343.

238 Hatcher, 30, p. 124.

239 Hatcher, 31, p. 57.

240 Joutel, *Historical Journal*, p. 161.

241 Dorsey, George A., pp. 15–16.

242 Dunbar, Par. 3.

> . . . that they were going to cut their (Spaniard's) throats and put their heads upon lances at the entrances of the temples, ...[243]

Heads on lances at the doors of temples were also seen among the Pacaha [244] by the De Soto expedition,[245] and 150 years later around Taensa temples by Tonty.[246] In 1699 the Mongoulaches and Bayagoulas were reported by d'Iberville as decorating their temples with scalps.[247]

Although captives were desired as war trophies and 10 prisoners counted the same as 20 scalps towards the title of "Great Man Slayer," [248] scalps seems to have played the more significant part in the trophy pattern. The torture victim was ceremoniously scalped prior to torture.[249] Scalps, not captives, were the stated objective of a war party and were the trophy boasted about upon its return.[250] They seem to have been required for the privilege of tattooing the body.[251] After taking a scalp the warrior was compelled to submit to a period of continence and obey certain food taboos.[252] There is one reference to the use of scalps to dry the tears of relatives of slain warriors by the Natchez.[253]

Scalping was not reported for the Calusa of Florida but among the Timucua it was important.[254] If neither scalps nor captives were brought back as a sign of victory, an innocent Indian might be beaten to remind them that they must lament for past losses.[255] Scalps were immediately removed and carried home on lances where they were set up around the Chief's house, crowned with laurel, and became the center of the victory celebration.[256] This is suggestively similar to the Caddo and Natchez practices of associating scalps and temples. Old women carried the scalps in dances prasing the Sun for victory.[257]

243 Vega, p. 457.
244 Identified as Tunica by Swanton, *Relation of the Southeast*, pp. 62–63.
245 Vega, p. 411.
246 Tonty, *Memoir*, p. 61—*Jesuit Relations*, 68, p. 125.
247 Iberville, *Historical Journal*, p. 74.
248 *Jesuit Relations*, 68, p. 151.
249 Du Pratz, II, p. 429.
250 Du Pratz, II, pp. 421 f.
251 Du Pratz, II, p. 199.
252 *Jesuit Relations*, 68, pp. 151–153—Charlevoix, *Historical Journal*, pp. 167 f.
253 *Jesuit Relations*, 68, p. 149—Charlevoix, *Historical Journal*, p. 167.
254 Laudonnière, p. 413.
255 Laudonnière, p. 462.
256 Laudonnière, p. 464.
257 Laudonnière, pp. 413–414.

Scalping may have been performed by special men appointed for the purpose who furthermore mutilated and burned the corpses.[258] Scalps were set up on sticks along trails as a declaration of war.[259] Before taking the war-path the Sun was asked for victory, and the women beseeched the leader to avenge the deaths of their husbands.[260]

Little is known of the Eastern Siouan trophy pattern. Teeth as well as scalps were taken.[261] A curious use of scalps, perhaps as a substitute for human sacrifice, was reported by Lederer in 1669 for the Watary:

> . . . his (the King's) barbarous superstition, in hiring three youths, and sending them forth to kill as many young women of their enemies as they could light on, to serve his son, then newly dead, in the other world, as he vainly fancyed. These youths during my stay returned with the skins torn off the heads and faces of three young girls, which they presented to his majestie, and were by him gratefully received.[262]

Scalping by the Southeastern Algonkians is not prominent in the accounts of the Jamestown colonists. There is one reference to Powhatan ordering scalps hung on a line between trees near his cabin.[263]

The Yuchi in 1906 recalled that scalps had been carried stretched on hoops by the women relatives of the scalper in the victory celebration.[264] No earlier descriptions have been found.

The evidence which has been presented strongly suggests that scalps were looked upon primarily as sacrificial offerings to supernatural beings by the groups included in the Old Southeastern Pattern. It is perhaps significant that the Aztec likewise impaled the heads of sacrifice victims around the temples.[265]

It is difficult to distinguish slavery from adoption on the basis of some of the accounts but it is probable that true slavery did exist in the Southeast. Captives with the tendons of their feet cut to prevent escape were seen by the De Soto expedition along the Mississippi.[266] Lawson made a similar observation

[258] LeMoyne, p. 7.
[259] LeMoyne, p. 13.
[260] LeMoyne, p. 13.
[261] Lawson, p. 198.
[262] Lederer, p. 19.
[263] Strachey, p. 36.
[264] Speck, *Yuchi,* p. 85.
[265] Sahagun, pp. 61, 110.
[266] Vega, p. 419.

for Carolina 150 years later.[267] The same method for restricting the movements of slaves was employed by the Aztec.[268] Slavery, probably among Eastern Siouans, was noted as early as 1525.[269]

It is probable that the Calusa treated Fontaneda and other shipwrecked Spaniards as slaves. Cabeza de Vaca speaks of his life in the vicinity of the Gulf Coast as slavery. Ortis likewise appears to have been enslaved by the Indians of Florida. The Timucua took men, women and children captive [270] but there is no account of their final disposal except the statement of Charlevoix that women and children were enslaved and men sacrificed.[271]

Powhatan was said to have kept women and children, and even chiefs, as captives but their exact status is not clear.[272] However immediate death seems to have been more usual for all enemies regardless of sex or age.[273]

Adoption as distinct from slavery is not mentioned for any of the groups in this Old Southeastern Pattern with the possible exception that a Natchez woman could perhaps demand a captive to replace her husband.[274] There is no indication that such adoption was at all an important factor in the disposal of captives as most reports state that all men were tortured.[275] There are no accounts of either slavery or adoption among the Southern Caddoans although the related Wichita apparently had slaves.[276] Such negative evidence is not very conclusive.

Human sacrifice was an important element among the groups included in the Old Southeastern Pattern. Much of this sacrifice did not involve captives directly or exclusively but included relatives of a decreased person, children and slaves.

While the idea of offerings in the temples, including scalps, was found among the Southern Caddoans, only one case of human sacrifice has been mentioned. This involved the immolation of children when a "house" was burned.[277]

[267] Lawson, p. 198.
[268] Bandelier, *On the Art of War*, p. 139.
[269] Martyr, II, p. 261.
[270] Laudonnière, pp. 464, 469.
[271] See p. 162.
[272] Smith, *A Map of Virginia*, p. 106—Strachey, p. 107.
[273] Smith, *The Generall Historie*, p. 358—Beverley, pp. 40, 150.
[274] Du Pratz, II, p. 432.
[275] Du Pratz, II, p. 428—Charlevoix, *Historical Journal*, p. 167—*Jesuit Relations*, 68, p. 149.
[276] Dorsey, George A., pp. 7, 13.
[277] Hatcher, 30, p. 303.

On the lower Mississippi the extensive practice of human sacrifice was observed as early as the time of De Soto:

. . . commanded two young and well proportioned Indians to be brought thither; and said, that the use of the country was, when any lord died, to kill Indians to wait upon him and serve him by the way . . . and prayed Luys de Moscoso to command them to be beheaded. . . .[278]

An Indian boy, who had followed the Spaniards when they left Guachoia, said that he, being an orphan, had been adopted by the chief, but that:

. . . when his generous benefactor had taken sick and died, they chose him to be buried alive with him; because they said he was loved by him so much that he ought to accompany him to the other world in order to serve him there in his wants.[279]

Tonty noted the sacrifice of wives and retainers in 1681.[280] Ru, with d'Iberville in 1699, not only refers to Natchez sacrifices upon the death of a chief,[281] a practice which he also accuses the Taensa and Colapissa of indulging in, but in addition notes that they threw children into the fire because the temple had been struck by lightning.[282] This is confirmed by Penicaut.[283] Gravier mentions that children were sacrificed to appease a spirit made angry because no one had been slain on the death of the last chief.[284]

At the sacrifices upon the death of a Female Sun, scaffolds were erected to hold the bodies of the victims strangled by their relatives after they had danced in front of the temple.[285] Charlevoix states that the victims each mounted a separate scaffold in a public place from which they descended to dance in front of the temple from time to time. After three days each was strangled by relatives of the Woman Chief.[286] Families who sacrificed children might thus obtain the rank of nobility.[287]

There is no indication that captives were sacrificed except in so far as their position as slaves made this inevitable.

278 Elvas, p. 192.
279 Vega, p. 441.
280 Tonty, *Memoir,* p. 61.
281 Ru, pp. 35, 37.
282 Ru, p. 41.
283 Penicaut, *Annals,* p. 58.
284 *Jesuit Relations,* 65, p. 139.
285 Penicaut, *Annals,* pp. 92 f.
286 Charlevoix, *Historical Journal,* pp. 163 f.
287 Du Pratz, III, p. 44—Dumont, I, p. 181—Mereness, p. 148.

The Calusa were noted for human sacrifice and captives were specifically allotted for this purpose:

The chief of the Caloosa immolated every years one person, usually a Christian, to the principle of evil, as a propitiating offering; . . . .[288]

. . . the cacique and his father had sacrificed (shipwrecked Christians) to their idols.[289]

Sacrifices were made upon the death of a leading person as well as to idols:

On the death of a child of a chief, his subjects sacrificed some of their sons and daughters to accompany it on its journey after death. On the death of the chief, his servants were killed. The Christian captives were annually offered up as food to the idols, who were said to feed upon their eyes, and a dance was performed with the head of the victim.[290]

Two Spaniards who had spent 15 years with the Calusa were brought to Laudonnière at St. Johns River in 1564:

Moreover they tolde me, that every year in the time of the harvest, this savage King sacrificed one man, which was kept expressly for this purpose, and taken out of the number of the Spanyards which by tempest were cast upon that coast.[291]

Ortis was twice threatened with death by the Indians of Florida. One of these threats may possibly have involved torture and not human sacrifice[292] although the context is not clear. The second time sacrifice was definitely the motive:

These people being worshippers of the devil, are wont to offer up unto him lives and blood of their Indians, or of any other people they can come by; and they report that when he will have them do sacrifice unto him, he speaketh with them, and telleth them that he is athirst, and willeth them to sacrifice unto him.[293]

The sacrifice of children on the death of a King, probably by the Timucua, is mentioned by LeMoyne, as well as a curious sacrifice of a stuffed deerskin to the sun.[294] The sacrifice of prisoners to the sun by the Indians of Florida is referred to by Charlevoix whose information is perhaps based upon the above accounts.[295]

288 Brinton, p. 94—citing Barcia.
289 Lowery, 1905, p. 229.
290 Lowery, 1905, p. 230—Swanton, *Early History*, p. 389.
291 Laudonnière, p. 482.
292 See p. 158.
293 Elvas, p. 126.
294 LeMoyne, p. 13.
295 See p. 162.

The sacrifice of scalps to accompany the body of a deceased warrior among the Eastern Siouans [296] was perhaps a substitute for actual human sacrifice. Likewise Lawson's observation previously cited, that they believed the "Devil" would send a calamity if captives were not tortured implies a human sacrifice attitude towards torturing. Lederer's reference to human sacrifice on the death of a great man among the Indians of western Carolina and Virginia probably applies to the Siouans:

When their great men die, they likewise slay prisoners of war to attend them.[297]

There are several accounts of the sacrifice of children or strangers to a supernatural being by the Southeastern Algonkians.[298] One description of "sacrifice" was probably nothing more than an initiation ceremony involving feigned death by clubbing.[299]

Lederer's rather vague reference to the Oustack sacrificing enemies to their idols is the only early account of Yuchi human sacrifice.[300] In 1906 an informant told Speck that Yuchi captives were not only tortured in the square grounds but were also kept to serve as a sacrifice to the sun.[301]

Cannibalism does not seem to have been a practice of any of the tribes with the exception of the Southern Caddoans. Charlevoix does mention that men sacrificed to the sun were eaten [302] but his authority for the statement is not clear. Although cannibalism was charged against the Chichimecos [303] as early as 1675 [304] the source is very questionable in that the evidence was in the nature of a rumor about unknown "heathen" Indians outside the borders of Florida. Aside from such doubtful cases there seems to be little basis for assuming any cannibalism among the tribes of the lower Mississippi River or eastwards, and there is specific denial by Vega that the Indians of Florida practised it.[305] There are, however, several descriptions

296 See p. 197.
297 Lederer, p. 8.
298 Strachey, p. 83—Spelman, pp. cv–cvl.
299 Smith, *A Map of Virginia*, pp. 111–112—Strachey, pp. 93–94.
300 Lederer, p. 21.
301 Speck, *Yuchi*, pp. 85, 116.
302 See p. 162.
303 Identified by Swanton as Yuchi, Wenhold, p. 4.
304 Wenhold, p. 11.
305 Vega, p. 242.

of voracious cannibalism among the Southern Caddoans, and the brutalities practised on captives [306] seem to have been rather a prelude to cannibalism than true torture. Joutel states that the woman brutally killed was served up to two boys of her nation and that they also ate dried tongues of their enemies.[307] The non-Caddoan tribes of eastern Texas and the Gulf coast were undoubtedly voracious cannibals and it is possible that the Southern Caddoans learned the usage after coming into contact with them, perhaps due to a movement out of the southeast.

There is little to suggest that torture was a part of the war trophy pattern of the tribes along the Atlantic seaboard. As has been indicated in an earlier part of this study, no references to torturing have been noted in this region until almost 200 years after white contact. Furthermore, it seems even more significant that there are no expressions by the early explorers and colonizers indicating any fear of such treatment. The Europeans were only too willing in most cases to call attention to the barbarity of the Indians, and thus justify their need for either salvation or extermination. This is, of course, negative evidence for the most part, although there are several specific denials of torture, but the situation is striking in contrast to the terror inspired at the time of the first White-Iroquois relations in Canada. One possible exception to the absence of torture in this region of the southeast might be the treatment of Ortis. Details are lacking, and he may well have been threatened the first time, as he was later, with human sacrifice without torture, a practice undoubtedly present among the Timucua. The few incidents of burning at the stake, referred to long after the early Spanish contacts, were in the form so well known in Europe, which had been used on the Indians by De Soto, and undoubtedly by many others. It is not difficult to believe that the Indians would learn to retaliate in kind. The Saponi torture described by Lawson in the early eighteenth century was not European in type, but was very like that of the Iroquois with whom the Carolina Siouans were in contact at that time. The probability that neither Siouan nor Algonkian speaking peoples tortured except as retaliation against the Iroquois lends weight to the suggestion that it was absent among the Siouan and Algonkian tribes of the Southeastern Area. The evidence seems

[306] See p. 168.

[307] Joutel, *Historical Journal*, p. 161 and *Relation*, p. 380.

to be quite conclusive that none of the tribes along the Eastern Seaboard tortured captives until long after white contact, and when it finally appeared it was in retaliation against similar treatment at the hands of the whites or the Iroquois.

In the lower Mississippi region, captive torture had many ceremonial aspects, and was strongly oriented towards religion. Dancing in front of the temple by the victim was one feature of this ritual connection, a practice required of certain sacrificial victims by the Aztec.[308] Likewise respect shown to the victim before torture began, torture probably inflicted in front of the temple, participation by men only, and the ceremonial touching with a brand seem to indicate that torture had an important religious significance. Attachment of the victim to a frame was unique in the Southeast and this, coupled with the ceremonial application of the brand, perhaps suggests Mexican and Pawnee contacts.[309] Whether or not the Pawnee could have obtained the frame from the Natchez is beyond the scope of this paper. Other Caddoan tribes apparently lacked it. The parallels between Pawnee Morning Star Sacrifice traits and the Natchez torture practices are not impressive. The resemblances of the Natchez practice to that of the Mexican sacrifice to a war god or the sun are likewise not strikingly close, but seem slightly more cogent than the other. Natchez torture was of a man, and the association with the temple indicated sacrificial motivations. The frames were nearly identical, but the possibility for variations in these is not great. Otherwise there are few similarities. Cardiac features, cannibalism, and the use of the flayed skin of the victim were not associated with the Natchez torture, nor was death under agonies comparable to being shot by arrows. Nevertheless, the basic concept of men being sacrificed to a god may well have been of common origin, and the possible historical connection cannot be ignored. It is also somewhat significant chronologically, for if the Aztecs only received this ceremony of frame sacrifice in 1506 it is quite possible that it would not have reached the Mississippi until after the time of De Soto.[310] His failure to note torture, although he did find human sacrifice of a different sort, is not very reliable negative evidence, however. The supposition has been advanced that the late mention

308 Sahugun, p. 43.
309 Linton, p. 463.
310 See Wissler and Spinden, and Linton.

of Natchez torture indicates that it was acquired as a retaliatory measure against the Iroquois.[311] There seems to be no adequate grounds for such an hypothesis. A review of the material shows practically no similarity between the two torture complexes, but many striking differences. Perhaps more pertinent evidence for the late appearance of Natchez torture is that it does not seem to have spread to the neighboring Choctaw and Chickasaw.

Except for the brutal killing of a woman captive by the women of the tribe, an act which bore little resemblance to institutionalized torturing, the Southern Caddo treatment of captives seems to have been cannibalism, with the infliction of pain as merely an incidental accompaniment.

To summarize in broad terms, it may be said that there is evidence for an older culture in the Southeast extending from eastern Texas to the Atlantic seaboard with a war trophy complex, including related traits, which had as a basis:

Scalps as offerings to the supernatural, not associated with mourning or ghosts.

Temples closely connected with warfare.

Human sacrifice to accompany the dead, or propitiate the supernatural, but only incidentally related to warfare or captives.

Slavery, not adoption, for war captives.

Torturing absent, excluding late development in retaliation against White and Iroquois, except on the lower Mississippi where it had religious motivations and may have been of relatively late origin.

Cannibalism absent, except among the Southern Caddoans, where a late diffusion from the surrounding tribes is indicated.

Swanton has remarked on similarities between the Atlantic Coast and the lower Mississippi region, in contrast to the intervening area, which:

> . . . suggests rather strongly that the Creeks were comparatively late intruders into the section where they were found by the Europeans and that, in the process of settling there, they had displaced some cultural features which formerly extended unbrokenly from the Atlantic Ocean to the Mississippi.[312]

311 Blair, I, 169, footnote.

312 Swanton, *Aboriginal Culture of the Southeast*, p. 718. See also p. 726.

Some of the traits taken to illustrate this were the use of ossuaries, artificial head deformation, use of poison to destroy enemies, matrilinear descent without clans, and human sacrifice.[313] Other traits could undoubtedly be included, such as absence of mother-in-law taboo and a developed nobility with considerable power in the hands of the chiefs.[314]

The data on the war trophy patterns of the Atlantic Coast and lower Mississippi regions seem to confirm the underlying unity of an older widespread culture in the Gulf region of North America suggested by these parallels. It is perhaps identifiable with the early Proto-Muskhogean of Bushnell which grew out of a Yuchi-Siouan culture, and was basic to that of the Natchez, Timucua, and Calusa.[315] The unity of this culture is also indicated by pottery similarities between early Caddo, Moundsville of Alabama, Etowah of Georgia, and the northern Gulf Coast of Florida.[316] The pottery of east-central Texas has been considered more like that of the Gulf Coast of Florida than of the closer Coast of Texas.[317]

In addition to the tribes which have been specifically included, the Choctaw should perhaps be added instead of being placed in the Intrusive Southeastern Pattern. The information is not complete enough on their war trophy patterns to indicate clearly their position. Other groups, such as the Chitimacha, were undoubtedly part of this older culture but their war patterns are practically unknown.

Intrusive Southeastern Pattern

Into this underlying widespread culture in the Southeast intrusions of other people with entirely distinct attitudes towards war trophies occurred. These later arrivals may be roughly identified as the Muskhogean Proper, although the exact tribal conformation is confused. The infiltration of Choctaw, Chickasaw, and Creek may have been gradual and accompanied by a considerable amount of fusion with the earlier culture. It is quite possible that the distinctive character of the new culture was due to the Creeks alone.

313 Swanton, *Aboriginal Culture of the Southeast*, p. 718.
314 Swanton, *Aboriginal Culture of the Southeast*, pp. 696, 700.
315 Bushnell, *Tribal Migrations.*
316 Stirling, p. 21.
317 Pearce, p. 55.

. . . the Creeks, who are believed to have entered the region in very late times, but the de Soto narratives show us that in the early 16th. century part of the Creeks did not conform to this pattern. I am therefore inclined to attribute the standardization of Creek culture to a Creek tribe which arrived from the northwest at a late period, ...[318]

The earliest reference to scalping by these tribes is that the Apalachee prized the scalps of De Soto's followers to hang upon their bows.[319] In 1559 the members of the de Luna expedition saw a pole about fifteen feet high full of scalps standing in the center of the square of a Napochies (Choctaw) village.[320] Scalps rather than captives seem to have been the significant trophy of war and even in the case of a tortured captive the scalp was carefully protected and used later in the victory ceremony held in the town square.[321] Whole scalps were not necessary. One cut up into many pieces would suffice as a trophy for the entire war party and enable each warrior to advance in rank.[322] A scalp shared with a nephew or child, who was compelled to sing under the blows of the donor, gave him recognition as a warrior.[323] Scalps varied in value and their relative merits were pronounced by the chief.[324] So important was the scalp that the Creeks were accused of killing members of their own tribe to obtain the necessary trophy,[325] and to prevent their own scalps from falling into the hands of the enemy they scalped their own dead.[326] Ghosts of slain warriors who had either lost their scalps or had remained unburied were refused admission to the "Mansions of Bliss" until surviving friends retaliated upon the enemy.[327] This relationship between scalps and ghosts was characteristic of all the tribes. They might be placed upon the houses of the relatives of those slain without being avenged,[328] on poles near the houses[329] or on the top of the ceremonial sweathouse.[330] The souls of slain warriors were

318 Swanton, *Relation of the Southeast*, pp. 64–65.

319 Ranjel, p. 152.

320 Swanton, *Early History*, p. 236—citing Davila Padila.

321 Adair, p. 397.

322 Adair, pp. 167, 298, 388, 397—Schoolcraft, V, pp. 297 f.—Hawkins, *Sketch*, p. 70.

323 Swanton, *An Early Account*, p. 66.

324 Milfort, pp. 249–250.

325 Adair, pp. 258–259.

326 Milfort, p. 253—Adair, p. 387—Dumont, p. 46.

327 Pope, pp. 63–64.

328 Adair, pp. 167, 397.

329 Cushman, p. 254.

330 Romans, p. 75.

supposed to haunt the eaves of their former dwellings[331] and not to leave until the scalps were placed there.[332] A report for the Choctaw states that all war booty, which probably included scalps, was given to relatives of deceased warriors to dry their tears.[333]

The ultimate disposal of the scalps is not clear. Undoubtedly they were often kept permanently on the poles or houses.[334] There are some references to their use in ceremonies.[335] They were sometimes worn as a headdress during the Busk ceremony at which time there was ceremonial scalping of effigies.[336] An informant told Swanton that the Alabama had buried a scalp under the ceremonial ball post.[337]

The religious attitude expressed by the emphasis upon the connection between scalps and ghosts is further brought out in the requirement of ritual purity on the part of the warriors. This involved sweating, fasting and drinking the emetic, or Black Drink, both before and after war.[338] Such ritual purity was even practised by Creeks during the civil war.[339] The insistence upon continence during the war period protected captured women from violation.[340] Another use of the Black Drink in association with mourning was observed by Ru in 1669. He noticed that Houma women drank water mixed with herbs and attempted to spew it up as part of the mourning ritual.[341] War was definitely connected with the idea of pollution and failure of a war party was blamed upon the impurity of the leader or some member of the party, or even upon those who had stayed at home.[342] "Holy" men were forbidden to slay and no warrior could officiate at religious ceremonies.[343] However, to counter-

331 Schoolcraft, I, p. 210.

332 Adair, p. 151.

333 Bossu, I, pp. 294 f.

334 Bartram, *Observations*, p. 35—Schoolcraft, V, p. 265.

335 Adair, pp. 310, 421.

336 Swanton, *Religious Beliefs*, p. 567 (citing Stiggins MS.)—pp. 572 f. (citing Hitchcock MS.).

337 Swanton, *Religious Beliefs*, p. 544.

338 Bossu, I, pp. 298 f.—Adair, pp. 119, 160, 166, 167—Milfort, pp. 238 f.—Hawkins, *Sketch*, p. 79—Speck, *Taskigi*, pp. 109, 118—Schoolcraft, V, pp. 538, 543, 265.

339 Swanton, *Social Organization*, p. 436.

340 Adair, p. 164—Schoolcraft, V, p. 272.

341 Ru, pp. 28–29.

342 Adair, pp. 163, 164, 166, 382, 416, 421 f.

343 Adair, p. 152.

act impurity contracted by attacking women in their menstrual lodges, special herbs were carried.[344]

Except for one reference to eating the heart of a brave enemy, cannibalism is denied for these tribes.[345] Mutilations of the dead in addition to scalping did occur [346] but seem rather to have been a form of insult, similar to sodomy on a dead enemy,[347] with no cannibalistic connotations.

The placating of ghosts by the presentation of scalps was quite distinct from the idea of using the trophies as offerings to the supernatural at the temples found in the Old Southeastern Pattern. Consistent with this difference was the absence of temples and of any form of human sacrifice among these tribes. The satisfaction of the religious demands through the taking of scalps led to social advancement. Titles and war honors depended upon them [348] and they were about the only way in which such recognition could be obtained.

The emphasis upon scalps seems to have inhibited the enslavement or adoption of captives for the religious obligations required the scalp itself and could not be satisfied by captives. There was probably a certain amount of adoption [349] but it seems never to have reached sizeable proportions. The Choctaw were said to have "enslaved" women and children [350] but the connotations are not clear. This relatively slight weight attached to captives was of course changed by the introduction of the white man's slave complex.

There is strong probability that torture was not originally a part of this culture. As in the case of the Atlantic Coast tribes, there is no indication of torture until almost 200 years after European contact, and fear of such treatment from the Creeks was apparently entirely foreign to the early whites. Even at a late date, the evidence for the practice of torture is extremely scanty. Both Bartram and Milfort report that it had once been customary, but do not elaborate further. It is unfortunate that there is not more information on the torture at the slave posts, merely mentioned by Bartram. The reports

344 Adair, p. 124.
345 Adair, p. 135.
346 Romans, p. 75—Adair, p. 37.
347 Romans, p. 70.
348 Adair, pp. 147, 151, 193—Bossu, II, p. 42.
349 Adair, p. 154.
350 Swanton, *An Early Account*, p. 66.

on the Yamassee torturing Spaniards and English in 1715 are very sketchy. These acts were apparently retaliatory in nature. Burning captives at the stake by the Apalachee in 1704 and by the Chickasaw in 1736 were typically European in pattern. Only the single account of Adair for the Chickasaw is reasonably complete, and there is much in it to indicate that torture was not integrated with the rest of the culture, but that the scalp was the dominant motive in the war trophy complex. The victim was turned over to the women to torture upon the return of the war party. There is no suggestion that he was presented to any one specifically with the idea of replacing slain relatives or satisfying mourners. He was beaten and tortured outside the village, and before the victory ceremonies were held in the square grounds within the village. The scalp of the victim, carefully protected during the tortures, was an important element in the later ceremonies. The entire act of torture apparently had no ceremonial connotations. The captive was not expected to dance or sing at any time, and seems to have had no function in satisfying religious emotions or in providing social recognition for his captor.

This particular Chickasaw torture complex has been distinguished from the stake torture on the basis of method of securing the victim and the kind of torments employed. The underlying attitude of pure retaliation without ceremonial elements would however indicate that it had much the same basis as stake torture with perhaps the addition of a certain amount of ingenuity possibly learned at this relatively late date from the Iroquois.

Most descriptions of torture definitely suggest the white pattern of burning at the stake. Retaliation as a motive appears to be prominent in the few cases of torturing by the Muskhogean Proper which have been recorded. The evidence seems quite conclusive that torture in this culture was a late development originating as a result of white, and perhaps some Iroquois, contacts. It is possible that burning captives as a retaliation against whites was sometimes fitted into the scalp complex for an old Spanish resident of Florida told Bartram that the Indians had formerly burned Spaniards to appease the spirits of slain relatives.[351]

351 Bartram, *Travels*, p. 489.

The entire war complex of the Muskhogeans strongly suggests the Plains in many particulars. This is especially true of the great emphasis upon the scalp in connection with mourning, a function of much significance in Plains warfare.[352] Such elements as purification, absence of torture and cannibalism, and the relatively few captives taken also resemble the Plains pattern, and lend additional weight to Swanton's conclusions on the movements of the Muskhogeans based upon other evidence:

> It is evident that the culture of the central region (of the Southeast) had been markedly modified by influences and probably invasion from the northwest.[353]

The Natchez may well have taken over the idea of using scalps to dry the tears of mourners from these tribes, particularly the Choctaw, and added it to the older pattern in which the significance of scalps was in their importance as offerings to the supernatural. Purification may have been closely identified with the scalp relationship to the dead and been taken over simultaneously by the Natchez. It is not, of course, yet clear that the Choctaw were not a part of the older culture who were more influenced by the intrusive culture than were the Natchez.

Iroquois Pattern

The third distinctive trophy pattern appeared among the Northern Iroquois. Scalping was a feature of Iroquois warfare but not perhaps as significant as it was farther south where Friederici placed its origin.[354] Scalps do not seem to have had any connection with the souls of the dead warriors or to have been given to their relatives. Perhaps an exception to this is furnished in the account of Mary Jemison for the period after 1755 where the Seneca are said to have given scalps to mourners to dry their tears if no captives were available.[355] Furthermore the idea of scalps as offerings to the supernatural is likewise lacking.

Scalps seem to have been prized rather as badges of merit, a sort of proof of valor, without any further connotations. The scalper was evidently not permitted to make several out of one

[352] Smith, M. B., pp. 452 f.
[353] Swanton, *Southeastern Indians,* p. 20.
[354] Friederici, p. 428.
[355] Seaver, pp. 59–60.

by cutting them up, and he was esteemed for the number he could show, which implies that they were not given away.[356] Scalps were painted and carried on poles,[357] and might serve to indicate the number of slain enemies to the village upon the return of the war party.[358] A report of 1634 states that the Oneida put the scalps on Images carved like men on top of the gate to their village.[359] Scalps were carried in preparatory war dances and in the victory celebrations, and a post was "scalped" by each warrior before setting out on the war-path.[360] The torture victim might be scalped as a part of the brutality and without any deeper significance.[361]

There are practically no references to trophies of war other than scalps and captives. An early Dutch account speaks of the Mohawk carrying home leg and arm bones,[362] and a Jesuit in 1626 noted heads being carried home.[363] At late as 1776 the Wyandots and Migoes in Ohio were supposedly seen putting a head on a post and dancing around it.[364]

Captives rather than scalps were the more desirable war trophy and except for a relatively small number who were tortured they were adopted by relatives of slain warriors as a means of appeasing the dead. A vivid picture of a substitution is furnished by the case of Father Poncet who had been captured by the Mohawk in 1653 and allotted to a widow for adoption:

> So soon as I entered her cabin, she began to sing the song of the dead, in which she was joined by her two draughters. . . . and then I was in the place of the dead, for whom these women renewed the last mourning, to bring the deceased to life again in my person, according to their custom.[365]

A captive might be given to a relative of a deceased person although not necessarily of the same sex. The thirteen-year-old

356 Heckewelder, p. 216.

357 *Jesuit Relations,* 39, p. 57; 53, pp. 145–147—Zeisberger, p. 105—Heckewelder, p. 216.

358 Schoolcraft, III, p. 188.

359 Unknown, p. 148.

360 Heckewelder, pp. 209 f.

361 *Jesuit Relations,* 17, p. 65; 22, p. 259; 34, p. 27—Champlain, IV, p. 100—Lindestrom, p. 242.

362 Wassenaer, p. 85.

363 *Jesuit Relations,* 4, p. 201.

364 Leeth, p. 38.

365 Beauchamps, *A History,* p. 199.

Mary Jemison was adopted by two Seneca women to replace a dead brother, for they considered her as sent by him to stand in his place and help them.[366] The relatives of slain warriors might even originate a war party by offering presents to someone to organize one.[367]

The policy of adopting individuals and even entire tribes to repopulate villages [368] carried the idea considerably further than any other group in eastern North America:

> It was not confined to captives alone, but was extended to fragments of dismembered tribes, and even to the admission of independent nations into the League.[369]

After adoption the captives were treated as Iroquois in full standing. Colden expresses this policy as follows:

> It has been a constant Maxim with the Five Nations, to save the Children and Young Men of the People they Conquer, to adopt them into their own Nation, and to educate them as their own Children.[370]

Should the adopted persons be unhappy it was possible to return home,[371] but usually they would be content to consider themselves as true Iroquois.[372] Morgan's conclusion that prisoners were virtually slaves for years does not seem to be substantiated by the data available.[373]

However, allocation for adoption to replace a deceased relative did not necessarily mean escape, for, as has been indicated under the discussion of torturing, many such individuals were later given over to tortures.[374] Also it was necessary for the candidate for adoption to have his hardihood tested by running the gauntlet, an act which has been referred to by Morgan as the adoption ceremony. Only those reaching the house of their adoptors by running through lines of people armed with clubs were saved. Those who fell were instantly slain.[375] These houses were not refuge places in the same sense as found among the Creeks. There is one doubtful allusion to a refuge town.[376]

[366] Seaver, pp. 57 f.
[367] *Jesuit Relations*, 16, p. 205; 10, pp. 227 f.
[368] *Jesuit Relations*, 43, p. 267.
[369] Morgan, I, p. 332.
[370] Colden, p. 110.
[371] Morgan, I, p. 332.
[372] Morgan, II, p. 277.
[373] Morgan, II, pp. 277 f.
[374] See p. 186.
[375] Morgan, I, pp. 333, 334. Hewitt, *The League of the Iroquois*, p. 342.
[376] Mooney, *Myths*, p. 208.

Many of the Iroquois war trophy practices were characteristic of the surrounding Algonkians. The Pottawatomi and Ottawas,[377] and the Sauk and Fox[378] extensively adopted captives to replace the deceased, as did the Lenape and other Eastern Algonkians. The Dakota Sioux did likewise in the case of Hennepin,[379] and the Winnebago[380] and Kansa[381] had a similar practice. The Iroquois adoption pattern seems to have been a widespread trait shared by the Algonkians and western Siouans but not significant in the Southeast. Running the gauntlet upon arrival at the village was in the nature of a test of the virtues of the captive preliminary to adoption and, like adoption, the Iroquois practice was characteristic of the Algonkians and Siouans but absent in the Southeast.

The great fear in which the Iroquois were held by their enemies was undoubtedly due in large part to the likelihood of being tortured if captured, and in this sense torture acted as an incentive towards submission. Its use to spread terror may have been a secondary development which was, nevertheless, consciously utilized, but there is much to indicate that human sacrifice was the underlying motivation. The woman burned and eaten as an offering to the war god was definitely such an act.[382] In many cases of the more customary torture of men the sacrificial motivation was indicated by the insistence on circumspect behavior on the part of the torturers, the dancing and singing required of the victim, kindness to and feasting of the victim, the requirement that death should occur only upon the platform at sunrise, death by a knife, cardiac features, and cannibalism. The feast of bear meat with the accompanying apology to the war god for not having tortured and eaten captives is an excellent illustration of this motive. Most of the above features were associated more or less completely with all torturing and strongly suggest that, while lust for vengeance and the spreading of terror may have become an important element, the concept of a sacrificial offering underlay the act, a possibility mentioned by Linton.[383] This type of human sacrifice, however, would seem to have little in common with that of the Southeastern groups

377 Blair, II, p. 162.
378 Blair, II, p. 197.
379 Hennepin, pp. 210–211.
380 Schoolcraft, IV, p. 53.
381 Hunter, p. 328.
382 See p. 186.
383 Linton, p. 462.

either at the death of a chief or periodically to propitiate the supernatural.

Cases have been mentioned in which the life of a dog could be taken in lieu of that of a man, and it has been suggested by some that the White Dog Sacrifice was a late substitution for human sacrifice.[384] The ceremonial significance of the dog in Iroquois culture was closely associated with war. An interesting substitution is mentioned in one of the earlier Relations. A man, being tortured by his friends in order to fulfill a dream, substituted a dog, which was killed, burned, and eaten, "just as were captives."[385] Other animals might be similarly sacrificed.[386] Dogs were eaten in the feasts preparatory to going to war,[387] and in the adoption feast of a captive.[388] A "madman" in search of a man's head, and threatening a missionary, was persuaded to substitute that of a dog.[389] The White Dog Sacrifice in the mid-winter ceremonies was observed in the seventeenth century. These dogs were strangled on the first day of the ceremonies, and burned and eaten about the fifth day, after being decorated to represent a god.[390] The sacrifice of a dog, with the subsequent eating of it, was a part of the curing ceremony.[391]

The ceremonial importance of eating dog flesh at the feast preparatory to war had a wide distribution. It has been mentioned for the Natchez[392] and likewise occurred among the Abnaki,[393] Menomini,[394] Ojibwa,[395] Winnebagoes,[396] Oglalla,[397] Quapaw[398] and in Mexico.[399] The Micmacs sacrificed dogs as a part of the mourning ceremony.[400] There does not seem to be any particular reason why the ceremonial connotations attached to the dog

384 Beauchamps, *A History,* pp. 131 f.
385 *Jesuit Relations,* 23,, pp. 172–173.
386 *Jesuit Relations,* 13, p. 159.
387 *Jesuit Relations,* 9, p. 113.
388 *Jesuit Relations,* 42, p. 191; 13, pp. 43 f.
389 *Jesuit Relations,* 42, p. 43.
390 Hewitt, *White Dog Sacrifice,* pp. 940, 943—Fenton, pp. 7, 11.
391 *Jesuit Relations,* 13, p. 31; 57, p. 147.
392 Du Pratz, II, pp. 421 f.
393 *Jesuit Relations,* 67, p. 205.
394 Skinner, *War Customs,* p. 306.
395 Jenness, *Ojibwa Indians,* p. 102.
396 Schoolcraft, IV, p. 52.
397 Kelly, pp. 197 f.
398 Bossu, I, p. 99 f.
399 Sahagun, pp. 58, 177.
400 *Jesuit Relations,* 2, pp. 93–95.

among the Iroquois should be assumed to imply its use as a substitute for human sacrifice. Furthermore, it was observed as early as sacrifice and may well have been older.

While the only cannibalism in the Southeast, that of the Southern Caddoans, gave evidence of having been acquired from the neighboring voracious man-eaters, that of the Iroquois had much resemblance to the eating of a sacrificial victim so prominent in Aztec human sacrifice ceremonials.[401] It has been suggested that the Aztec acquired a liking for human flesh through the religious compulsions to eat these victims,[402] and this might well have been true of the Iroquois. It must also be remembered that the Algonkians, such as the Miami and Shawnee, had Cannibalistic Societies to which captives were given, and the Sauk and Fox were said to have eaten captives at one time, all these cases presumably being without torturing.

Thus the Iroquois war trophy complex does not seem to have had any very close similarity with either the older Southeastern culture or with the intrusive Muskhogean culture. Caddoan-Iroquois relationships based on linguistic and presumed pottery resemblances are not confirmed by the war complex. On the contrary, it is very close to the typical Algonkian pattern. The exception to this would be torturing which, as has been suggested, was probably taken up by the Algonkians as a retaliatory measure against the Iroquois.

The Iroquois torture pattern had certain traits in common with human sacrifice in Mexico. These included cardiac emphasis, death by a knife, eating of the victims, and perhaps dancing and the use of a platform. None of these similarities, except possibly the dancing of the victim, are found associated with the frame torture of the lower Mississippi where Mexican resemblances were of an entirely different order. Furthermore, the evidence seems to point to a fairly recent accentuation of torturing by the Iroquois, as it apparently was not shared by the Cherokee until late wars with the Northern Iroquois, and had not diffused to the Algonkians much before the early white contacts.

401 Sahagun, pp. 43, 52, 62, 75.
402 Loeb, p. 11.

Southern Iroquois and Shawnee

Certain Southeastern tribes have not been classified under the three patterns as they seem to have occupied an intermediate position between the Northern Iroquois and the Muskhogean peoples to the south. Observations on these groups came relatively late and are very scanty.

The extremely warlike Shawnee obtained a reputation for cruelty but as has been indicated few descriptions of actual torturing are available.[403] Scalping was undoubtedly important although its significance is not known. Scalps were cleaned, dried, stretched on hoops and painted red.[404] A tradition obtained by Trowbridge from the Prophet mentions the scalping of a warrior by one of his own tribe. This scalp was later sent in a pot filled with blood to their enemies as a challenge for war.[404a] Cannibalism seems to have occurred only in connection with the Cannibalistic Society.

Many captives were adopted, perhaps the most famous being Daniel Boone who was their prisoner, and treated like a son, for several months in 1778.[405] Kenton was forced to run the gauntlet about that time. Six hundred Indians with sticks, tomahawks, and knives lined up for one-half a mile across a level plain and compelled Kenton to run between them to a council house.[406]

Torture among the Cherokee [407] was apparently influenced by the white pattern of burning at the stake and also by Iroquois methods. Here again the specific information is very scanty. It is also difficult to evaluate their treatment of captives. How extensive the holding of slaves was, before the development of the slave trade by the whites, is difficult to say. They kept many white prisoners as slaves after the war of 1760. These were said to have been the property of their individual captors.[408] Adoption was probably more customary than slavery on the aboriginal level, although there might always have remained the danger, to which Timberlake was exposed, of being killed to revenge depredations committed against the adoptors by rela-

[403] See pp. 177 f.
[404] Seaver, p. 48—Spencer, pp. 44 f.
[404a] Voegelin, p. 7.
[405] Galloway, pp. 260–261.
[406] Galloway, p. 256.
[407] See pp. 176 f.
[408] Timberlake, pp. 40, 90.

tives of a slain warrior.[409] Captain Bonnefoy and four other Frenchmen, one a negro, were captured in 1741. They were well treated and adopted into families, except for the negro who was set free because he had been wounded. However, as he did not use his freedom, but continued to follow them, they gave him over to the young people who killed and scalped him. Before the adopted captives were permitted to enter the village they were compelled to sing for hours. Then they were tied together two by two and marched around a great tree in the village at the foot of which some of their hair was buried. They were then taken to the council house where each was made to sing four songs. After doing this they were washed and fed, and were then considered as brothers.[410]

Scalps were an important war trophy, so much so that they have been accused of killing one of their own tribe and pretending that the scalp was from a Shawnee.[411] A war party returned in 1761 with four painted Shawnee scalps on a pole which was carried three times around the Town House and then placed near the door.[412] Scalps were carried by women in the victory celebration held in the Town House.[413] Speck was told that the scalps had been carried by singing and dancing warriors in the Victory Dance, a part of the Eagle Dance, and afterwards collected by the leader and put carefully away.[414] Scalps were also said to have been carried on peace missions.[415]

There are no accounts of special purificatory rites upon the return from war. However, success in war, as in hunting, depended on the moral purity of the warrior.[416] Warriors underwent strenuous rites for strengthening their physical and spiritual powers under the guidance of the medicine men, and a certain class of consecrated warriors used no other weapon than the heavy oak or hickory war club.[417]

Practically nothing is known of the Tuscarora war trophy

409 Timberlake, p. 84.

410 Mereness, pp. 242–246.

411 Mooney, *Myths*, pp. 375 f.

412 Timberlake, p. 92.

413 Mooney, *Myths*, p. 376.

414 Speck, MS.

415 Speck, MS.

416 Logan, p. 26.

417 Speck, MS. The author is indebted to Dr. Frank G. Speck not only for full access to his valuable manuscript material but also for many suggestions and criticisms.

pattern except that they killed Lawson.[418] Captives were apparently given to the "priest" and the woman of highest rank, who compelled them to dance as a sign that they had become subjects. During the victory celebration each family occupied a separate scaffold which they had erected near the execution grounds. A fire was built near the scaffold and when a woman became tired of dancing she returned and sat on the scaffold to eat with her husband.[419]

V. Conclusions

1. *Frame Torture*

The Frame Torture of the lower Mississippi had many elements with sacrificial connotations. Such traits, however, were entirely distinct from those associated with the Iroquois torture pattern, and the resemblances with Mexico were consequently not parallel. Whether or not the use of a frame indicates relationship with Mexico is not obvious. Should this be so, the complex was probably acquired subsequently to 1506. Other indications of its relatively late appearance in this region are the absence of torture among the other peoples who were a part of the Older Southeastern Culture, the failure of Frame type torture to diffuse to contiguous tribes such as the Choctaw and Chickasaw, and the lack of reference to it in the chronicles of the De Soto expedition which do contain descriptions of human sacrifice of a different type. On the other hand, the emphasis upon war trophies, specifically scalps or "heads," as offerings to the supernatural and associated with the temples, and the importance of human sacrifice, not, however, connected with warfare, might well have evolved indigenously into the sacrifice of war captives. Whether the complex diffused essentially as such from Mexico or was of independent local growth from the same basic underlying concepts cannot be resolved until corroborative evidence is brought out by many detailed analyses of the cultures. The present evidence seems to favor the latter assumption. It may be fairly confidently asserted, nevertheless, that human sacrifice was the motivation for the Frame torturing of captives, and that there were no connections, except perhaps

418 See p. 166.

419 Graffenreid, Excerpts, Vol. I. Furnished through the kindness of Dr. John R. Swanton of the Bureau of American Ethnology who suggested many pertinent sources.

very remotely through a common Mexican origin, with the human sacrifice motive underlying Iroquois torture.

2. Pole and Stake Torture

Tortures in the Southeast which have been designated as Pole and Stake Torture strongly suggest a European origin. The absence of torturing among peoples of both the Old Southeastern Culture and the later Intrusive Culture until long after the first White contacts is indicated by the barrenness of the early sources, both in respect to positive descriptions and failure to suggest that the Indians had been feared because of the possibility of torture. When torture finally did get into the accounts almost 200 years after the arrival of the Europeans, it does not seem to have been integrated into the cultures or to have borne any religious or social connotations. Furthermore, the common European pattern of the stake heaped about with combustibles, unquestionably often inflicted upon recalcitrant Indians by the invaders, was used. Stake Torture was therefore based upon purely retaliatory motives and was a recently acquired reflection of the culture of Europe. It has been pointed out that the Pole Torture of the Chickasaw was probably of the same basic type as Stake Torture with possibly some Iroquoian influence.

3. Platform Torture

The Platform Torture of the Iroquois appears to have been based upon human sacrifice to the Sun or War God and to have had certain resemblances to Aztec sacrifice. The rest of the Iroquois war trophy pattern was very comparable to that of the surrounding Algonkians, who were apparently receiving the idea of torturing from the Iroquois and using it as a retaliatory measure. That the intensive practice of torture by the Northern Iroquois was of rather late growth is suggested by the probability that the Southern Iroquois did not use it until after comparatively late contact with the north and therefore it was not a part of the earlier combined culture. Furthermore, the penetration of torture into the Algonkian cultures was seemingly not deep and was recognized as being of Iroquois origin. Perhaps due to an upsurge of war interest the importance and numerical amount of human sacrifice, a very old complex, increased, thereby

leading to a spread of terror of the Iroquois which was utilized and emphasized to obtain submission of their enemies with the consequent progressive brutalization of the human sacrifice motive.

It must be emphatically repeated that these conclusions can pretend to nothing more than a reasonable degree of probability based on the available material. Only through detailed studies of many other manifestations of Southeastern cultures will a clear understanding of the functional integrations emerge and the historical factors be properly evaluated. Such studies will obviously modify conclusions based on a limited segment of the cultures.

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SOIL-MOISTURE FLUCTUATION UNDER A LAWN, AS INDICATED BY ABSORPTION FROM POROUS-PORCELAIN IRRIGATOR CONES WITH CONTINUOUS OPERATION *

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Abstract

A new device for the observational study of soil-moisture fluctuation in the open ground is described. It consists essentially of a water-filled porous-porcelain irrigator cone permanently buried in the soil at the depth horizon to be considered and joined by a tube to a water reservoir above ground, with a suitable mercury barostat interposed between reservoir and cone to prevent excessive hydrostatic pressure in the latter, thus giving assurance that all movement of water from reservoir through cone to soil must be due to soil suction. Whenever the soil adjacent to the cone is so wet that the suction developed at the cone surface is less than the standard back pressure of the barostat, then no water is lost from the reservoir; at other times loss is continuous, being more rapid as the surrounding soil becomes drier, at least till a maximal rate is attained. Rates of water loss are measured from time to time without disturbing the soil.—Cones were operated at several depths in the clay-loam soil of a Baltimore lawn, and average hourly rates for each day from Aug. 20 to Sept. 27, 1938, are presented graphically, along with corresponding records of precipitation and evaporation. When considerable rain fell the rates for the uppermost cones soon dropped to zero, somewhat later the cones at the intermediate depth ceased to show losses, while the lowermost cones were still more tardy in showing this response to the downward penetration of water. Then the cones at each depth began to lose water again, but absorption from the uppermost ones might become relatively rapid before the descending wave of excessive soil moisture had reached the lowermost ones.

Introduction

In the study of soil-moisture condition with reference to the water supply of ordinary plants, attention has been generally directed to soil-moisture content per dry-weight unit or per volume unit, or even to amount and incidence of rainfall or irrigation (Livingston and Shreve, '21). Since plants generally absorb only a very small proportion of their required water through aerial parts, rain and artificial irrigation com-

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monly exert but little influence on plant growth excepting in an indirect manner, as they add water to the soil surface from time to time, thus increasing soil-moisture content. When water is thus applied to an area or region of previously unflooded soil surface more rapidly than it can penetrate, temporary flooding occurs and a portion of the applied water escapes to other areas or to streamways through superficial run-off, which may result in puddling and serious erosion. The rate of penetration for the area considered is generally limited by physical characteristics of the soil, including its surface slope, its porosity and its temperature, and by the hydrostatic pressure that results from superficial flooding. Thus, the volume of water actually taken into an ordinary soil on account of a surface application is determined not simply by the volume of water applied per unit of soil surface (depth of précipitation or acre-inches of irrigation) but by the physical characteristics of the soil itself, by the depth of flood water and by the length of time during which the soil surface is flooded. If all of the applied water is to penetrate, superficial run-off is to be prevented. But, when the added water is derived from rain or artificial overhead sprinkling, prevention of surface run-off may be bad for the health of many plant forms, since prolonged flooding of the soil surface may be injurious, as through its adverse influence on soil aeration. More or less prolonged flooding of the soil surface is commonly a necessary accompaniment of artificial irrigation, but overhead sprinkling may be applied so slowly that flooding and superficial run-off are reduced to a minimum and the soil absorbs water about as rapidly as it is applied. When heavy rains occur, provision for local surface run-off without injurious flooding, or erosion, may be had through contour furrowing.

These considerations naturally lead to the thought that the influence of precipitation on soil-moisture content and upon plants should be considered with reference to frequency of rains and to their respective durations, rather than with reference to their depths as measured by means of a simple rain-gage. Time distribution and durations of rains may both be shown by automatic records, such as those furnished by the tipping-bucket gage, but they have not yet been generally considered in studies on rainfall as related to plant growth or

vegetation. Instead of mean total precipitation for the year or for a part of the year, at various stations, more useful precipitation indices might be based on seasonal distribution and lengths of rain periods (when the surface soil is very wet or somewhat flooded) and of periods without rain, as was tried by Livingston and Shreve.

For more detailed study, in comparing regions, areas and different seasons, it should be desirable to base precipitation indices on depths of penetration of rain water into the soil. If data on that feature were available one might employ them in place of rain-gage values, asking, for example, at what times and for what lengths of time the soil of the areas considered approaches or surpasses field saturation at some specific depth; or one might characterize each rain or irrigation in terms of depth of penetration into the soil. Data of that sort may be inexpensively secured by means of the technique brought forward in this paper and by other more or less similar procedures. For any specified amount of water passing down into the soil across an areal unit of ground surface, the depth of penetration is mainly determined by soil structure, which naturally varies widely.

Students of soil-moisture fluctuation, as that is related to plant performance, have generally recorded soil-moisture contents (per dry-weight unit or, more preferably, per volume unit of soil) for several different horizons or depths at various representative locations for each field or topographic area considered. Such recordings are repeated at intervals so as to furnish time graphs of water content for one or more soil horizons at each station, and each graph is extended throughout some suitable period such as the growing season. At best, this method of approach involves a great amount of labor, including the collection of numerous representative soil samples and their subsequent weighing, drying and reweighing. Also, the resulting water-content values must each be specially weighted or adjusted with reference to the particular physical characteristics of the soil in question, at the horizon considered. As every one knows, a specified low volumetric water content in the region of absorbing roots may represent an adequate water supply for a long time if the soil in question is sandy, while the same water-content value may represent a quite inadequate

supply if the soil is loam or clay. The requisite weightings usually involve great uncertainties and additional labor—such as determinations of moisture equivalents, water-holding capacities, etc.—and the results so secured can be little better than more or less makeshift approximations of the values they are taken to represent. Nevertheless, most of our knowledge concerning subterranean plant water relations in the open has been secured by procedures of that kind.

After all, however, the effective soil variable with which students of plant water relations should be concerned is water-supplying power (Pulling and Livingston, '15; Livingston and Koketsu, '20; Mason, '22; Hardy, '23, '24; Livingston and Ohga, '26; Wilson, '27; Marshall, '31; Wilson and Livingston, '32; Livingston and Norem, '37) rather than water content or any measure of rainfall or of irrigation. The complex manner in which the water-supplying power of the soil around a plant root system is dependent on weather and soil characteristics may be set forth, in general outline, by the following scheme.

Water-supplying power (dynamic capacity to deliver water to unit area of a water-absorbing surface such as that of a root) is dependent on: (*A*) the relatively permanent physical characteristics of the soil—summarized in part as its volumetric water-holding capacity or its moisture equivalent (the latter based commonly on a centrifugal force of 1000 gravity, etc.); (*B*) soil temperature; (*C*) volumetric water content; (*D*) capillary potential and (*E*) capillary resistance to water movement. *A* depends mainly on arrangement and sizes of particles, pore sizes and internal surface (whether the soil is clay, loam, sand, etc., whether its particles are mineral or organic, whether its packing is loose or firm, etc.). *B* depends on air temperature, wind, absorbed solar radiation, fermentation, capacity of soil to absorb and conduct heat, etc. *C* depends on (1) previous supply of absorbed water (from rain, artificial irrigation, surface flow or subterranean water table) and (2) on previous loss of water through evaporation and root absorption. *C*(*1*) frequently depends, in any instance, on the length of the time when the soil surface was saturated by rain or sprinkling, but it is only roughly related to rain-gage readings. *C*(*2*) depends on evaporativity, absorbed solar and sky radiation and on kind and condition of plants, as the absorbing power

of root systems varies and fluctuates. *D* depends on *A, B* and *C,* and *E* depends on *A, B, C* and *D.*

Of all these environmental features, water-supplying power is the only one that acts directly on plant roots as far as water supply is concerned, and the others would be of only secondary interest in physiology, ecology and their applications if data concerning supplying power were available. When a plant is unhealthy because of inadequate soil-moisture condition, it is naturally the water-supplying power of the adjacent soil that is at fault, as far as soil-moisture condition is concerned. But of course recovery for a time may frequently be attained if the water needs of the plant are reduced, as by shading, etc.; supply must always be considered with reference to demand.

Although the initial water-supplying power of the soil at any place and depth may be directly estimated from time to time by means of Livingston soil-point cones (Pulling and Livingston, '15; Livingston and Koketsu, '20; Livingston, Hemmi and Wilson, '26; Livingston and Ohga, '26; Wilson, '27; Marshall, '31; Wilson and Livingston, '32; Livingston and Norem, '37), yet that technique—although clearly more satisfactory than the best procedures for securing weighted water contents—seems to be still somewhat too laborious or too novel to be highly attractive, and many competent students still regard water contents as capable of furnishing satisfactory indices of soil-moisture condition. The method of soil-point cones furnishes supplying-power indices to indicate the approach of drought injury to plants as the soil about their roots becomes gradually drier. Not least of the theoretical and practical advantages of that method is the consideration that values secured thereby have thus far shown no need for any special weightings to care for solid-phase and moisture-content differences among kinds of soil, etc.

Permanently buried osmotic cells have been experimentally employed by some students (Livingston, '06) for the estimation of the static pressure resistance offered by the adjacent soil to water absorption by plant roots, also for the estimation of dynamic water-supplying power (Pulling and Livingston, '15; Pulling, '17), but techniques for this method of approach are thus far not sufficiently developed for general application.

Permanently buried porous-porcelain cylinders or cones,

kept filled with water and equipped with suitable barostats, have long been employed for the automatic irrigation of plants growing in pots (Livingston, '08, '18; Livingston and Hawkins, '15; Livingston, Hemmi and Wilson, '26; Deatrich, '27), and the principles of that technique are applied in the tensiometers of Rogers ('35 (1, 2), '36) and of Richards and Gardner ('36), in which the barostat is replaced by a manometer. When permanently installed in the soil at desired depths, such a tensiometer appears to indicate at any time the value of the capillary potential (or suction) of the soil adjacent to the porous surface *as that potential is transmitted to the manometer,* and these instruments may be arranged to furnish automatically traced time graphs of manometer suction. This type of instrument has been found to give useful results so long as manometer suction does not too closely approach the current barometer reading. As thus far developed, it fails when that suction rises above about 60 or 65 cm. of mercury column (Livingston and Norem, '37), and this limitation is highly important when clayey soils or soils of considerable organic content are to be studied; for ordinary plants generally continue to thrive in soils of relatively high content of clay or organic matter even when the manometer suction has attained its limit. This tensiometer is therefore not satisfactory for use in heavy soils that are likely to experience drought. When used in sufficiently sandy soils there appears to be no such limitation, however, for permanent wilting of plants rooted in such soils supervenes long before drying can proceed far enough to raise the indicated manometer suction to the instrument's limit.

It should be emphasized, however, that, even with sufficiently sandy soils, the indicated tensiometer suction corresponding to the onset of drought injury to the plants, or to any stage or degree of such injury, differs from one kind of soil to another. Thus tensiometer data require special weighting when the soils dealt with are not very nearly alike in their physical characteristics, much as is true for water-content data. Plants in a very sandy soil wilt or wither when the indicated manometer suction is low, while plants in a somewhat less sandy soil fail to attain a similar wilting stage until the indicated suction is much higher.

It should also be remembered that capillary potential of the

adjacent soil, even if it might be satisfactorily indicated by some instrument, is not wholly satisfactory as a criterion by means of which to study the soil-moisture feature that influences plant health directly; the water-supplying power of the soil about the roots of a plant—or its reciprocal, the resistance offered by the soil to water absorption by roots—is of course dependent on the adjacent soil's dynamic water-conducting power (Gradmann, '32) as well as on its static capillary potential.

Related to the concepts of water-supplying power, water-conducting power and capillary potential is the concept of the *water-absorbing* power of the soil, which may be estimated from time to time as the initial rate of water loss, for a short time period, from a standard water-supplying surface such as that of a porous-porcelain irrigator cone permanently buried in the field at any desired depth and provided with a constant water supply and a standard barostat resistance to soil absorption. An instrument in which such a cone is allowed to operate only intermittently—its cavity being empty of water excepting while readings are in progress for short observation periods at not too frequent intervals—was described by Livingston and Norem, who reported on some tests of its performance in pot cultures. That instrument and the technique of its use are to receive further attention in another paper.

A New Type of Soil-Moisture Instrument

The present contribution presents some observations on rates of water loss from continuously water-saturated porous-porcelain auto-irrigator cones operating continuously, like automatic pot irrigators, at several depths in the clay-loam soil of a lawn near the Johns Hopkins Laboratory of Plant Physiology, at Baltimore, in the summer of 1938. In our set-up, which is essentially like that employed for continuous automatic pot irrigation with cones or cylinders, the cones were permanently buried in the soil at desired depths and were kept filled with water, water being continuously supplied from a reservoir above through a suitable U-tube mercury barostat. With such an arrangement water is absorbed by the adjacent soil from the surface of water-saturated porous procelain, the rate of absorption being somewhat retarded by the fixed hydrostatic resist-

ance (or suction) of the barostat. Absorption from a cone in moist soil generally proceeds at a fluctuating rate, which is controlled partly by the fluctuating capillary potential of the adjacent soil (the "pF" of Schofield, '35), partly by the fluctuating resistance offered by the neighboring soil to capillary movement of water outward from the adjacent soil layer into more distant regions, partly by the magnitude of the constant barostat resistance used, and partly by the viscosity of the moving water—which naturally depends upon soil temperature. When, because of addition of water from some other source—through rain, irrigation, etc.—the capillary pull of the adjacent soil, as transmitted to the cone, falls below the critical value of the barostat resistance, absorption ceases and a very little water moves *from soil to cone* for a short time, till a shift of the mercury column of the barostat brings such backward movement to a standstill. As the volumetric water content of the neighboring soil subsequently decreases—through subterranean run-off, evaporation and root absorption—the mercury column soon returns to its regular position in the U-tube and capillary flow from cone to soil is resumed, at a rate that increases as further drying proceeds, at least till a limiting rate is reached.

About thirty years ago (while porous-porcelain cylinders were still in use for the automatic irrigation of potted plants, the cones now employed being not yet available at that time) one of the present writers experimented a bit with an auto-irrigator cylinder installed in open soil, near the Desert Laboratory of the Carnegie Institution of Washington, at Tucson, Arizona. The soil dealt with was a heavy clay and the barostat resistance used was equivalent to about 3 cm. of mercury column. The vertical porous cylinder was at a depth of about 25 cm. below the sloping soil surface. Because that preliminary test with a cylinder was the direct forerunner of our tests with cones, and because the results secured thereby have never received adequate attention in the literature (Livingston and Norem, '37), a possibly important observation made at that time may be considered here.

The instrument was installed in the midst of a rainless period that extended from May till midsummer and the daily rate of water loss from the reservoir was essentially constant for many weeks before the beginning of the summer rains, al-

though the general soil of the neighborhood, whose surface layers were almost oven-dry at the time of installation, continued to dry out to progressively greater depths. When the first heavy rain of July wetted the general soil to depths greater than the depth of the instrument, absorption ceased for a time, to begin again later. What interests us most at present is that the rate of water loss from the cylinder was maintained essentially constant throughout a long rainless period with intense sunshine and evaporativity, for our cone tests at Baltimore have now given some evidence of a similar performance.

If it is true that absorption of water from such a continuously-operated porous-porcelain auto-irrigator (cylinder or cone) may be expected to go on at a nearly constant rate for a long time while the general soil of the neighborhood is becoming progressively drier, this may be at least tentatively explained somewhat as follows. Immediately around the instrument is, of course, a region of moist soil, which absorbs water from the porous porcelain and transmits it—mainly through capillary movement, but to some extent through distillation—to the periphery of that region, where it is absorbed by the drier soil of the general surroundings. Thus the moist region tends to enlarge. Radial movement of water naturally becomes progressively slower as the moist region enlarges, however, while at the same time the peripheral surface of that region tends always to increase in extent. Thus, if water were not removed from the moist region excepting at its periphery, a dynamic capillary equilibrium should be approached, when the rate of water loss from the moist region to the surrounding soil would equal the rate of movement out of the wet porcelain. Under such conditions, if water were removed from the moist region somewhat more rapidly that region should contract (thus reducing the extent of its periphery), and if the rate of water removal were retarded the moist region should expand and increase its periphery; and the rate of water loss from the porous-porcelain piece should remain essentially constant. This postulated state of affairs might be expected to continue indefinitely, regardless of fluctuations in the rate of evaporational water loss from the general soil surface, but equilibrium would surely be approached only slowly after each considerable alteration in the rate of water removal from the moist region.

If, however, absorbing roots of plants should be present within the moist soil region, relatively close to the surface of water supply, fluctuations in their rate of absorption might produce corresponding fluctuations in the rate of water loss from the instrument. Absorbing roots might extend to the immediate vicinity of the porous-porcelain piece, thus securing the benefit of auto-irrigation. Such water larceny by plants might be effectually prevented if all neighboring plants were kept cut back to below the soil surface or if the soil around an instrument of this sort were adequately poisoned—as by means of a slight addition of a copper salt, etc. It may be noted that the soil-moisture tensiometer and the intermittently operated irrigator cone, mentioned above, supply water to the surrounding soil in such small amounts or at such slow rates that this difficulty is not likely to be serious when they are used.

When the general soil, including the moist region about a continuously operating auto-irrigator, receives sufficient additional water from some outside source (as from precipitation, irrigation, river flood or subterranean supply from the water table below) to reduce soil-moisture potential in the vicinity of the porcelain piece to a magnitude less than that required for absorption from the instrument, then soil absorption naturally ceases and the suction resistance of the instrument may cause the latter to suck a little water out of the soil, as has been noted, until that movement is stopped by the reversed mercury column in the barostat.

We now return to the account of our own tests at Baltimore. Although our outdoor arrangement of cone, barostat and water reservoir was essentially like that employed by Livingston and Norem in their greenhouse study of the initial water-absorbing power of the soil as related to plant wilting in pot cultures, our procedure was fundamentally different from theirs in one important respect. In their tests, the cone cavity was filled with water only while a reading was in progress (for an hour and ten minutes at each time of observation), the cavity of the cone being emptied of water after each reading and remaining so till the beginning of the next following reading. Thus, their cones were operated only intermittently, at intervals of days or weeks, while ours supplied water to the neighboring soil continuously excepting when the latter was so wet that water movement from cone to soil could not occur.

The Instrumental Set-up

Our standard cones have a shape resembling that of an inverted funnel (Fig. 1) but the porcelain wall is continuous across the large end, which is about 7.5 cm. in diameter. The externally smooth-ground conical portion is about 6 cm. high and is continued upward as a cylindrical neck about 4 cm. high and 2.5 cm. in outside diameter. The wall—composed of ma-

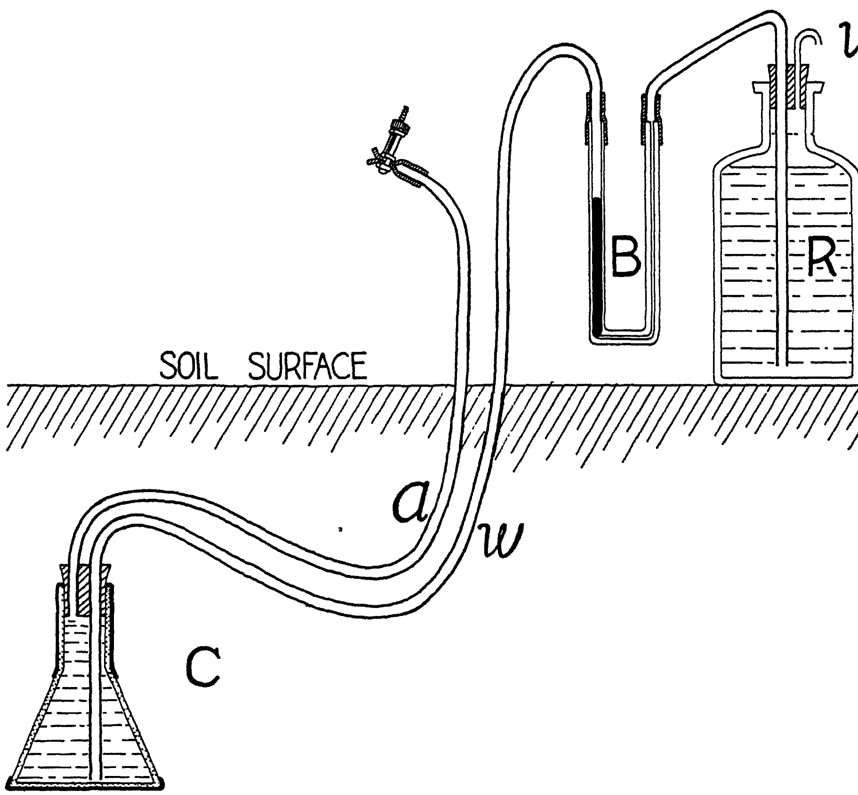

Fig. 1.

terial like that of the Livingston soil-point cone and the white atmometer sphere—is about 2.5–3 mm. thick throughout. Several coats of waterproof aluminum varnish are externally applied to the plane base, to an adjoining narrow zone of the conical part, to a corresponding zone of that same part just below the neck, and to the neck itself, including its upper free edge. The smooth-ground porous surface, from which alone

water movement into the adjacent soil is to take place, is thus restricted to a vertical frustum 5 cm. high, with slant height about 5.5 cm., whose outside diameter below is about 7.4 cm. while its external diameter above is about 3.1 cm.; its outside area is consequently about 90 sq. cm. For use at the 4–9-cm. depth in the soil, each irrigator was shortened by removing 3 cm. of the neck, so that the instrument would not extend upward to the soil surface.

Each cone is equipped with a 2-hole rubber stopper bearing two block-tin tubes (outside diameter, 6 mm.; bore, 3 mm.; preferably varnished outside), one of which (*w*, Fig. 1) reaches to the bottom of the cone cavity and serves as water inlet and outlet, while the other (*a*) terminates below at the lower surface of the stopper and serves as air inlet and outlet. Both tubes extend laterally and upward to terminate above the soil surface at the place where the reservoir is to be located, being bent so as not to act as leads for downward flow of rain water to the neighborhood of the cone. The upper end of the air tube is always closed, by means of a bit of rubber tubing and a clamp, excepting when suction is temporarily applied through it to remove air from the cone cavity. The water tube is extended to the barostat (*B*), which in turn is connected directly to the reservoir (*R*), both of these members being conveniently placed above ground. The barostat is a vertical glass U-tube about 11 cm. high, the bore of the entrance arm being about 1.5 mm. while the bore of the exit arm is about 5 mm.; it contains mercury in amount just sufficient to counterbalance the hydrostatic pressure of the water column in the system when water is moving from reservoir to cone and to introduce, in addition, a standard hydrostatic resistance. The height of the water column in the system is measured vertically from the standard level of the water meniscus in the reservoir to the level of the base (or of the middle) of the porous surface of the cone. In the tests here considered the barostat resistance was equivalent to 4 cm. of mercury column in all cases. For reservoirs we used narrow-neck half-liter bottles, each with a horizontal file-mark on the neck, to which zero mark the water meniscus was brought at each reading, water being added from a burette. Each bottle was provided with a 1-hole cylindrical cork stopper (slotted at one side, to provide an air vent) and

the water tube led through the stopper to the bottom of the bottle. In our instruments a sheet-rubber apron ligatured to the tube above the cork prevented entrance of rain water into the bottle through the vent slot. A little inverted J of copper tubing (v) set into the cork serves well as air vent with unslotted cork, but with that arrangement the cork must always be tightly set into the bottle neck; other arrangements might be employed to insure free air entrance without entrance of rain water. Slotted cork and apron were first used in this manner in simple mountings for Livingston's atmometers ('08) but the bent vent tube is now generally employed for those instruments ('35). For short-period readings the bottle is conveniently replaced by a burette, or both bottle and graduated pipette may be used, as in Johnston and Livingston's ('16) arrangement for short-period atmometer readings. In any case, the reservoir should be replenished frequently enough so that its water level is never lowered more than a few centimeters below the zero mark; for the hydrostatic pressure within the cone is an important characteristic of this type of instrument (as well as of the Livingston-Norem type while the cone is in operation) and that pressure should be approximately maintained.

In our cones the hydrostatic pressure was always less than one atmosphere by about 4 cm. of mercury column, as has been indicated. As long as the soil is absorbing water (with suction greater than 4 cm. of mercury column) the mercury stands high in the exit arm of the U-tube (as shown in Fig. 1) and water slowly slips past it and proceeds on its way to the cone. Thus the hydrostatic pressure within the cone is maintained and flow from reservoir to cone occurs only against a suction of 4 cm. of mercury column. If and when the suction of the soil close to the cone falls below the value just mentioned, the mercury shifts in the U-tube, descending in the exit arm (at left of the letter B) and ascending in the entrance arm (at right), while a very little water moves backward into the reservoir.

With this arrangement it is easy to tell at any time, by a glance at the barostat, whether the soil is absorbing water or not. This barostat, or a supplementary device, may be readily arranged so that an electric circuit becomes closed when soil suction falls below the standard barostat resistance, and an elec-

trically actuated pen may register the time of this occurrence on a clock-moved record sheet. When irrigation is applied by means of a sprinkler, flow of water to it may be cut off by an electrically operated valve as soon as water loss from cone to soil ceases; *i.e.,* as soon as added water has penetrated to the vicinity of the cone. Another type of barostat—which does not serve, however, to show when absorption ceases—resembles the Livingston-Thone ('20) atmometer valve; a vertical glass tube with bore of about 6 mm. and suitable length is plugged below with tightly packed wool yarn, glass wool, glass cloth, coarse-pored porcelain or sintered glass, and sufficient mercury is placed above the plug to furnish the requisite back pressure. Water from the reservoir passes through the plug, which itself interposes no considerable resistance to this movement, and slips upward past the mercury column on its way to the buried cone.

It is to be expected that water movement through the cone wall and then through the surrounding soil should be influenced by the viscosity of the water in wall and soil, which varies considerably with temperature. We have not studied the interesting temperature relation thus suggested, but our study gave some evidence that, in a rainless period, rates of water loss were likely to be somewhat more rapid by day than by night. To what extent such a day-night difference may be due to fluctuation in rate of root absorption or to temperature difference cannot be surmised.

The soil about a cone tends to settle against the conical surface, so as to maintain capillary contact between cone and soil more satisfactorily than is likely with a vertically placed porous-porcelain cylinder—such as was first employed for pot irrigation by Livingston and Hawkins and such as is used in the tensiometers of Rogers and of Richards and Gardner.

Installation and Operation

For the little study here reported, cones were installed on Aug. 1, a set of three being placed in each of two excavations 40 cm. deep and a meter apart. In each set the cone bottoms were 10, 20 and 40 cm., respectively, below the ground level and the porous surfaces were consequently at depths of 4–9 cm., 14–19 cm. and 34–39 cm., respectively. The three cones of each

set were distributed in a vertical spiral whose radius was about 15 cm.; thus no cone was directly above or below another and their parallel vertical axes were about 25 cm. apart. The excavated soil, which was relatively moist when the cones were installed, was returned without much mixing of layers, being firmly packed around the cones, much as a gardener packs the soil about the roots of a newly transplanted plant; but no water was artificially added. The lifted sod was returned to place. For protection, reservoirs and barostats were located within a nearby wire-mesh instrument enclosure, horizontally about a meter away from the excavations.

After being installed, these six cones were filled with water from their respective reservoirs, by means of suction applied through their temporarily opened air tubes, and they were allowed to operate, with frequent readings, for an adjustment period of 19 days. For a few days after installation, suction was occasionally applied briefly to the air tube of each cone, to remove any gas that might be present, but—as when pots of soil are irrigated in this manner—repetition of that operation soon became unnecessary and the cones were found to be free of contained gas at the end of the adjustment period.

On Aug. 20 the barostat resistances, which had hitherto been somewhat less than 4 cm. of mercury column, were all adjusted to that standard value. The actual mercury heights, when water was moving from reservoir to cone, were then respectively 6 cm., 6.9 cm. and 8.4 cm., for the three cones of each set. All were allowed to operate till Sept. 27, readings being taken at convenient intervals, usually several times each day. The volume of water removed from a reservoir (*i.e.*, the volume absorbed by the soil from the corresponding cone) in any observation interval was ascertained and recorded as the volume needed to replenish the reservoir to the zero mark on its neck. From the records thus secured was computed for each cone the average hourly rate of water absorption by the soil for each observation interval and for each day from Aug. 21 to Sept. 27. The resulting day averages are set forth graphically in Fig. 2. In the same figure are shown corresponding daily totals of corrected water loss from a standardized white porous-porcelain atmometer sphere (Livingston, '35) located 1.2 meters above the ground level at the Livingston atmometer station in

the open, near Riderwood—about eight miles north of this laboratory. Daily precipitation records (of more than 0.1 inch) for the instrument enclosure at this laboratory are also shown, plotted as vertical lines descending from the upper margin of the figure.

The Graphs of Fig. 2

Several rains occurred in the adjustment period, sufficient to penetrate to the deepest cones, as was indicated by the readings, and it is consequently safe to suppose that satisfactory capillary adjustment between cone and adjacent soil had been attained in that period. It is generally desirable to water the soil above and around newly installed cones several times, at intervals of a day or two, as a gardener waters newly transplanted plants—unless rains render artificial watering surely unnecessary. It is also generally desirable that the preliminary adjustment period be at least several weeks long in every instance.

A rain of 0.4 inch had fallen on Aug. 18, three days before the beginning of these records, penetrating first to the 4–9-cm. depth, then to the 14–19-cm. depth and at length to the 34–39-cm. depth. As the resulting wave of excessive water content descended in the soil, presumably bringing the moisture condition at the several horizons successively to field capacity or above, water absorption from the uppermost cones ceased for a time and then began again; a little later the intermediate cones showed a similar performance, but it was apparently not till Aug. 21 that the soil adjacent to the lowermost cones ceased to absorb water. For each depth, absorption began to increase soon after the minimal rate had been attained.

For the uppermost cones (4–9-cm. depth), the absorption rates increased until about Aug. 24, but they were approximately maintained thereafter, until quickly brought to zero by the 0.9-inch rain of Aug. 31. Although the grasses of our lawn, whose absorbing roots were largely at the 4–9-cm. horizon, did not generally show effects of severe drought, advanced wilting in scattered spots was to be observed on Aug. 31 and it is safe to suppose that the onset of severe drought condition was being approached at that time. The essential maintenance of these rates throughout the last week of that first rainless period,

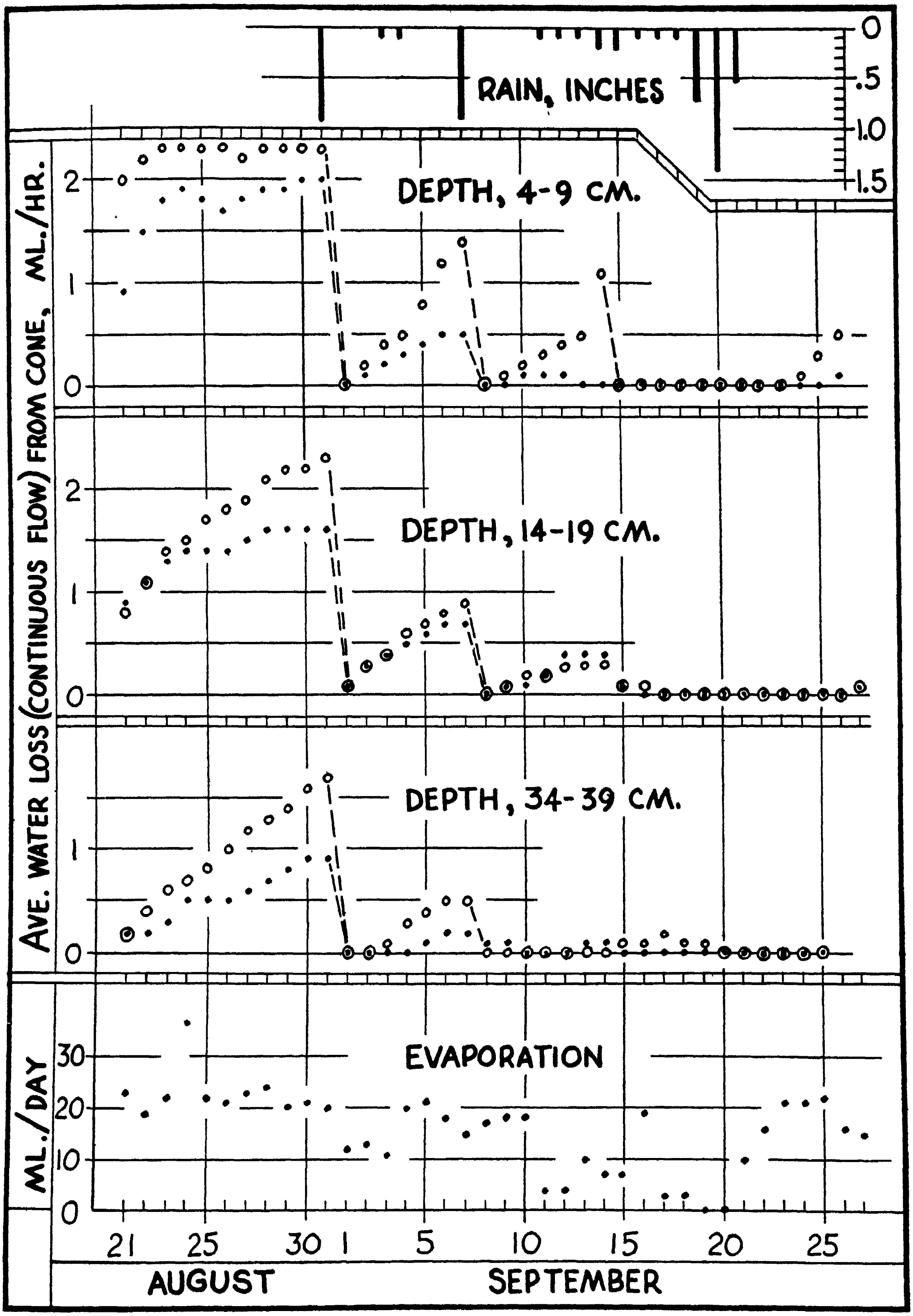
RAIN, INCHES
0
.5
1.0
1.5
AVE. WATER LOSS (CONTINUOUS FLOW) FROM CONE, ML./HR.
DEPTH, 4-9 CM.
DEPTH, 14-19 CM.
DEPTH, 34-39 CM.
EVAPORATION
ML./DAY
30
20
10
0
21
25
30
1
5
10
15
20
25
AUGUST
SEPTEMBER

Fig. 2.

when the surface soil was surely becoming progressively drier, is clearly reminiscent of the similar performance of the Tucson cylinder, referred to above.

If additional tests of instruments of this type, in a season of more prolonged rainless periods, should furnish further evidence of this sort, it should follow that their rates of water loss cannot themselves be taken as showing progressive drying of the general soil in the later stages of that process. In that case the later progress of drying might perhaps be indicated, day by day, by the increasing length of time elapsed since the preceding occurrence of zero readings. But that criterion might prove to be unsatisfactory because solar radiation, evaporativity and root absorption are all surely influential in determining the rate at which the soil dries, and their intensities may be expected to fluctuate from day to day throughout the time period in question. Perhaps the most promising suggestion in the present connection is that the approach and progress of soil drought might be followed in terms of cumulative water loss from the irrigator reservoir, measured from the time of the last preceding occurrence of a minimal or zero rate of soil absorption. It may be recalled that cumulative water loss from an aerially exposed atmometer, measured from the time of the last preceding heavy rain or irrigation, has furnished a useful index of the drying of the upper layers of a lawn soil and of the approach of drought injury to lawn grasses (Wilson ,'27; Wilson and Welton, '35).

One of the intermediate cones (at the 14–19-cm. depth) appears to have attained a constant rate of water loss (1.6 ml./hr.) by Aug. 28 and to have maintained that rate till the end of the current rainless period; but water loss from the other apparently increased throughout that period. As to the lowermost cones (at the 34–39-cm. depth), their performance was much like that of the intermediate ones. It is to be noted that the maximal rate of water loss, at the end of this rainless period, was greatest for the uppermost cones, less for the intermediate ones and still less for the lowermost ones. On Aug. 31 a rapidly penetrating rain of 0.9 inch brought absorption to a standstill at all three depths, and the average hourly rate for all cones is shown as about zero for Sept. 1. This means that in every case the capillary suction of the adjacent soil as trans-

mitted to the instrument was then about equal to or less than 4 cm. of mercury column (the standard hydrostatic resistance interposed by the barostat in these tests).

For the abscissal regions representing Aug. 31–Sept. 1, Sept. 7–8 and Sept. 14–15, the steeply descending broken lines of the graphs of Fig. 2 are of course not correctly curved, since daily indices of soil absorption are used for these graphs and we are not at present dealing with the rate of penetration of rain water into the soil. Although the zero rate of absorption was actually attained somewhat later for the intermediate cones than for the uppermost ones and still later for the lowermost ones, that minimal rate was attained for all three depths within less than 24 hours after the effective rain began to fall. For each cone, whenever the absorption rate fell to zero its fall was apparently almost instantaneous, as might be expected; that naturally occurred as soon as the soil adjacent to the cone received sufficient gravitational water from above to increase its water content nearly to the field capacity for the horizon in question.

Absorption at the uppermost and intermediate horizons had been renewed by Sept 2 but at the lowermost horizon it was not renewed till Sept. 3–4. Subsequently absorption rates increased at all three depths till absorption was again stopped by the 0.9-in. rain of Sept. 7. By Sept. 10–11 absorption had begun again from the uppermost and intermediate cones, but its rates remained low excepting for one of the uppermost cones, for which it increased to 1.1 ml./hr. on Sept. 14. Several light rains occurred between Sept. 11 and 18, and heavier rains fell Sept. 19, 20 and 21. (The storm of Sept. 19–20 represented the first Baltimore effect of the great hurricane that afterwards wreaked great havoc in New England.) Absorption from the uppermost cones began once more by Sept. 25, and absorption from the intermediate cones is shown to have just begun on Sept. 27.

As has been said, no effective drought occurred at our station in the period of these tests and our shallow-rooted lawn grasses generally remained green all summer, although some wilting was observed here and there on Aug. 31 (with daily atmometric evaporation of 20 ml. or more). It appears that soil drought was incipient or imminent on Aug. 31 at the 4–9-cm. depth,

when average rates of water absorption ranging between 1.7 and 2.3 ml./hr. had been continued for about 8 days, with evaporativity probably always as high as 20 ml./da. At that time about 12 or 13 days had elapsed since the last preceding record of a zero rate, and the cumulative losses from the two cones at the 4–9-cm. depth for the period since the occurrence of that zero record were about 650 ml. and 450 ml.

Effective drought was not approached, even at the 4–9-cm. depth, in the two subsequent nearly rainless but short periods shown by our records (Sept. 1–7 and Sept. 8–14). At all three depths the period from Sept. 1 to 7 showed much less drying than was shown by the period from Aug. 21 to 31, and the period from Sept. 8 to 14 showed still less drying, the latter interval being essentially without any drying effect at the 34–39-cm. depth. Another drying period is shown as beginning Sept. 25, for the 4–9-cm. depth.

It is specially important to note that two presumably like cones at the same depth and only a meter apart frequently showed different rates of water loss at times when loss was relatively rapid, but no information is available for studying these differences with regard to their probable causes. Such differences in rates may perhaps have been due to differences in the soil itself from location to location, to differences in root absorption near the similarly placed cones or to possible slight differences in the cones used—or to more than one of these things.

The Three-Dimension Diagram of Fig. 3

Figure 3 presents a somewhat clearer picture of the march of soil absorption for the period from Aug. 21 to Sept. 18 than can be shown by simple time graphs such as those of Fig. 2. Each absorption value used in Fig. 3 is the average of the two corresponding values from the two cones at the same depth but horizontally a meter apart. Thus, for example, on Aug. 21 the two cones at the 4–9-cm. depth lost water at hourly rates of 0.9 and 2.0 ml./hr., respectively, and their average rate is consequently 1.5 ml./hr. for that day. All the 2-cone averages are inscribed on the diagram of Fig. 3, where the two plane coordinates are dates (abscissas) and depths (ordinates) and the contours represent time-depth relations of the absorption rates

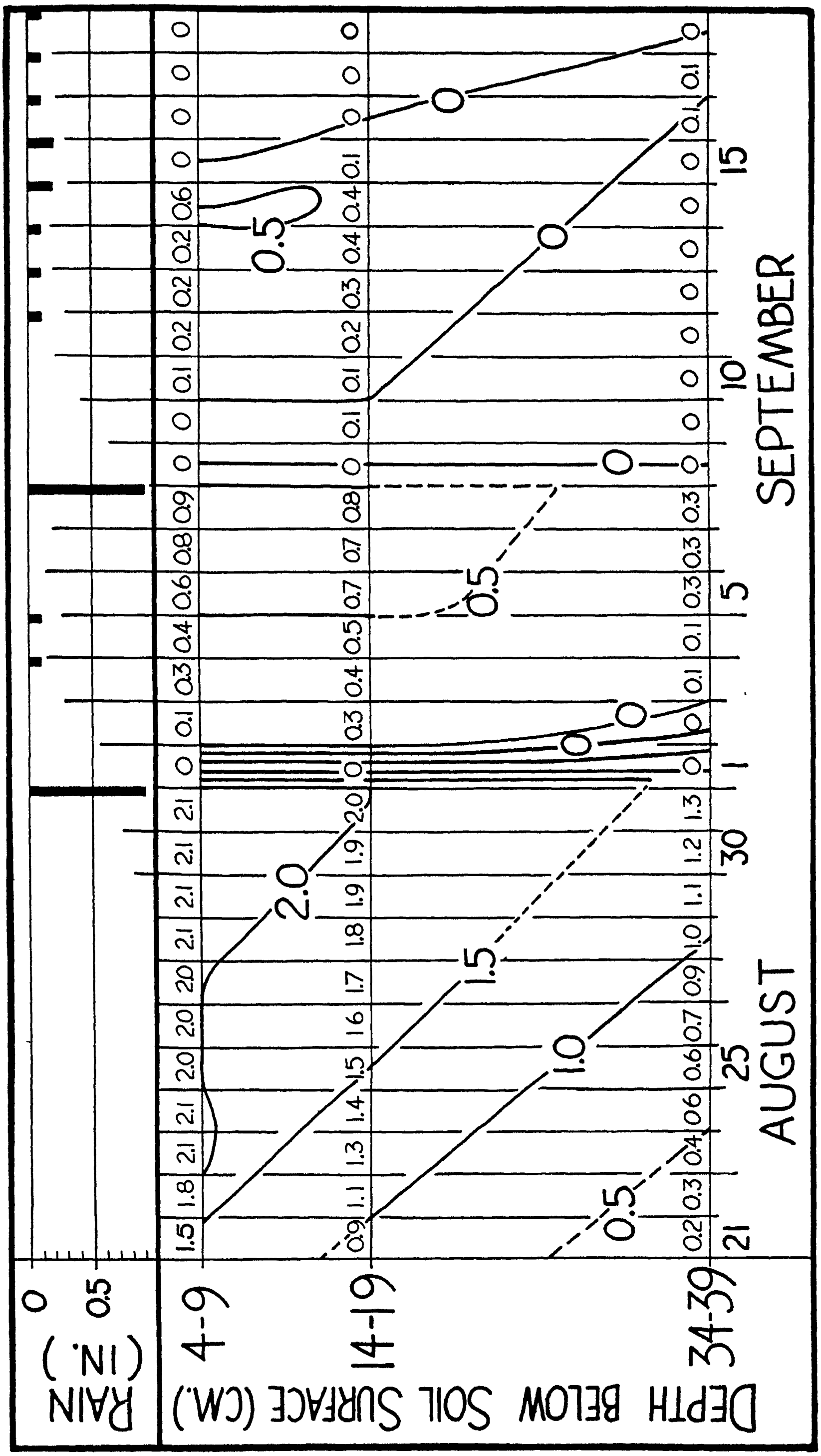
RAIN (IN.)
0
0.5
DEPTH BELOW SOIL SURFACE (CM.)
4-9
14-19
34-39
AUGUST
SEPTEMBER

Fig. 3.

0, 0.5, 1.0, 1.5, 2.0 ml./hr. Time of occurrence and amount of each rain are shown above, as in Fig. 2. The progressive downward advance of drying of the soil after each considerable rain is clearly indicated by the descending contours, and the approximate hourly absorption rate for any horizon down to the 34–39-cm. depth may be estimated for any day in the test period. Average absorption rates of 2.0 ml./hr. or more (dry soil) were confined to the surface layer from Aug. 22–23 to Aug. 27, after which the incidence of those high rates advanced downward, reaching the 14–19-cm. depth on Aug. 31, when such rates are shown as representing all depths above 14–19 cm. About Aug. 21, the rate of 1.5 ml./hr. is shown only for the 4–9-cm. depth and the same is true about Aug. 25 for the 14–19-cm. depth; but about Aug. 31 that rate may be supposed to have prevailed only for a depth of about 30 cm. As the degree of soil dryness corresponding to the 1.5-ml. rate advanced downward between Aug. 21 and Sept. 1, the soil at horizons already traversed is seen to have become progressively drier, with the result that by the time the 1.5-ml. rate was shown at its deepest horizon (presumably about the 30-cm. depth, about Aug. 31) the rate for the 14–19-cm. depth had increased to 2.0 ml./hr. Similarly, zero rates (very wet soil) are shown as occurring at all three depths only for Sept. 1, for Sept. 8 and for Sept. 18. For the first of these occurrences (due to the rain of Aug. 31) the zero rate is shown to have been maintained no longer than for a single day at the 4–9-cm. and the 14–19-cm. depths, but at the 34–39-cm. depth it is shown to have been maintained for two or three days before considerable absorption was resumed. For the second of these occurrences (due to the rain of Sept. 7) the zero rate was apparently maintained two days for the uppermost instruments, about one day for the intermediate ones and until Sept. 15 for the lowermost ones. The third occurrence of zero rates is shown to have begun on Sept. 15 at the 4–9-cm. depth, on Sept. 16 at the 14–19-cm. depth and on Sept. 18 at the 34–39-cm. depth.

To Estimate Depth of Penetration of Water Added by Rain or Irrigation

In the study of plant water relations and of soil drainage, it is often desirable to know how far into the soil the water

added by rain or irrigation finally penetrates, as well as to have information concerning the rates of its penetration, and these features may be easily estimated by means of instruments similar to the ones here described. For that purpose, the U-tube barostats of continuously operating cones at several depth horizons are simply examined at frequent intervals after the beginning of an application of surface water to the soil, and the time elapsed after that beginning and before any cone ceases to deliver water to the adjacent soil (as shown by shifting of the mercury column) is of course the time required for the front of the advancing gravitation water to penetrate to the cone in question. With sandy soils that front may move downward very rapidly, soon reaching its final depth, but with heavier soils the downward advance may continue many days after all free surface water has disappeared and the surface soil has dried considerably. It is necessary to consider, however, that the soil immediately about a continuously operating irrigator cone is always relatively wet and that the barostat should show cessation of water loss from the cone somewhat sooner than the penetrating water would reach the same horizon if the instrument were not present.

This device should lend itself also to the study of the upward and downward movement of the upper margin of the region of wet soil that lies above a natural or artificial water table when that is near enough to the ground surface for the effects of its rise and fall to require special attention. The new instrument should be equally applicable to the study of the outward movement of water from subterranean irrigator pipes, from irrigation furrows, etc. For these purposes, when records of rates of water loss are not required and observations are made by simply noting the position of the barostat column, the porous surface of the cone might well be much smaller than it was in our tests.

Conclusion

It appears that procedures of the type described here—employing permanently placed and continuously operating porous-porcelain irrigator cones and their observed rates of water loss to the adjacent soil—give promise of furnishing a valuable new means for the study of soil-moisture fluctuations at different

depth horizons in the open ground, as those fluctuations are related to plant growth and to additions of water to the soil through rain or irrigation. These new procedures seem worthy of further study.

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APPARATUS FOR VIEWING AND MEASURING STEREOSCOPIC CLOUD CHAMBER PHOTOGRAPHS

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ABSTRACT

Projecting apparatus is described which makes it possible to obtain quick and accurate measurements of helical electron tracks formed in a deep cloud chamber, and to get undistorted full-sized stereoscopic views of the tracks. All measurements are made with the aid of a ground-glass screen mounted on sliding ways. The observer sits in a comfortable position so that muscular strain is reduced, and optical strain is also reduced, for the eyes work at normal convergence and accommodation. Accessories useful in measurement are described. A mechanical method is given for eliminating important troubles due to shrinkage of the film and displacement of the film during reprojection. Since images on the screen are conical projections of real aerial track images, formulas are derived for correcting the apparent radii and axial dimensions of helical tracks; tests on wire models confirm their validity within 1 per cent. A conservative statement as to the performance of the instrument is that for tracks of about 5 cm. radius, unaffected by multiple scattering, radii can be obtained within 2 per cent, and scattering angles to better than 1 degree.

I. INTRODUCTION

SINCE the time required for the study of cloud chamber photographs is usually much longer than that required to obtain them, it is important to have good facilities for viewing and measuring them. The apparatus needed depends on the way the cameras are arranged, but there are only two cases of general interest. In the first, photographs are taken from points of view so widely separated that stereoscopic examination is impossible. In the second, the lines of sight are close together and stereoscopic views can be obtained.

Blackett [1] developed the method of taking photographs with two cameras whose lines of sight are at right angles. He gave algebraic and graphical methods for reconstructing a track and for measuring the angle between the branches of a forked track. The computations are rather complicated, so Curtiss [2] and others worked out a mechanical solution of the problem. Curtiss re-

[1] Blackett, *Proc. Royal Society*, **102**, 294 (1923); **103**, 62 (1923); and **123**, 613 (1929). See also Eggleston and Martin, *ibid.*, **162**, 95 (1937).

[2] Curtiss, L. F., *Bur. Standards J. Research*, **4**, 663 (1930).

placed the original photographs in the cameras and illuminated them from behind, so that the images of a straight track formed by the two cameras could be made to lie on a ground glass screen. By adjusting the screen until it lay in the plane of the original track, both images could be made to coincide. Distortion in the optical system is cancelled in reprojection if the films or plates can be put in exactly the positions they occupied when the pictures were taken.

When a magnetic field is applied to the chamber, the tracks become helical and if the illuminated region of the chamber is deep the above method loses much of its utility because the track images are not plane. In this case it is convenient to decrease the angle between the axes of the two cameras, or to use a mirror so that two stereoscopic views can be obtained with a single camera with its axis directed along the magnetic lines of force. Here two procedures have been developed. A large number of workers illuminate only a small "slice" of the chamber and restrict their attention to tracks which lie almost in a plane perpendicular to the lines of force. Others illuminate the whole chamber and determine the forms of all suitable tracks regardless of their position. We employ the latter plan, and strongly favor it for several reasons: Many measurable tracks are missed when the illuminated region is shallow, and this is serious when one is working with collisions of small cross-section. A deep illuminated region often helps one to avoid false interpretations, and the longer the visible track, the more accurately its curvature can be measured. Much has been said in the literature about the limited depth of field of large aperture lenses working at small distances from a cloud chamber. There are problems such as droplet counting, or the study of slow secondary electrons, in which extremely sharp focus is necessary, but there are many others in which out-of-focus effects are not important. Radii which are not too large can be measured on images which are slightly out of focus nearly as well as on those of the best photographic quality. To be specific, with a camera lens set at f:2.5 at a distance of about 35 inches, we have no difficulty in measuring radii throughout the volume of a chamber 2.25 inches deep.

Many devices for viewing and measuring helical tracks have been described. Loughridge and Skobelzyn,[3] for example, have

[3] Loughridge, *Phys. Rev.*, **30**, 488 (1930); Skobelzyn, *Z. Physik*, **43**, 354 (1927) and **65**, 773 (1930).

used stereocomparators to view the original negatives. The operation of these instruments is described in Skobelzyn's 1930 paper. They yield the three rectangular coördinates of any point on a track. Nuttall and Williams [4] and their collaborators place the developed films in the cameras with which they were taken and illuminate from behind to obtain full-sized real aerial images of the tracks. These images are viewed through a lens, and pins mounted on universal joints are made to coincide with any desired features of the track. Thus a sort of model of the track is made, but since it is inconvenient to apply scales directly to the pins, images of the pin-system are projected on a screen, where the actual measurements are made. Grosev, Dobrotin, and Frank [5] have devised a simpler plan. They employ a stereoscopic camera and replace the negatives in the camera, so that they can be examined with an ordinary stereoscope through holes in the back. Then one sees an image exactly like the original track and a pointer placed in front of the camera at the original position of the track can be brought into coincidence with any point on it. The pointer is mounted on slides so that its rectangular coördinates can be read off directly.

Two more specialized methods deserve mention here. Simons and Zuber [6] constructed an instrument for careful measurements of the radius of curvature in a magnetic field. With a goniometer mounted on a compound lathe slide they get the direction of the track (or, more accurately, of its projection on a plane normal to the field) as a function of distance along its length. This facilitates the detection of small angle scattering and is an accurate but quite laborious method for measuring radii. Blackett [7] has developed a method for dealing with tracks having a very large radius.

Many investigators content themselves with simply projecting the tracks on a screen perpendicular to the field. With a very shallow illuminated region this method can yield fair results but in the case of a deeper region it introduces large errors which have been either overlooked or neglected up to the present. In this paper we describe apparatus which has been developed with the purpose of making this method both accurate and rapid. It has been very satisfactory, and the principal features are these:

[4] *Proc. Phys. Soc.*, 42, 212 (1929); also Williams and Terroux, *Proc. Roy. Soc.*, 126, 294 (1929).

[5] *C. R. de l'Académie de Science de l'URSS*, 3, 289 (1936).

[6] *Proc. Roy. Soc.*, 159, 384 (1937).

[7] *Proc. Roy. Soc.*, 159, 1 (1937).

(1) Full-sized undistorted stereoscopic views are available on a flat translucent screen. (2) The camera used for photography is also used for reprojection. (3) Track radii are measured with transparent sheets of circles in the usual way. (4) Components of momentum parallel to the field are obtained by measurements on the above-mentioned screen. (5) Angle-measurements in the plane normal to the field are carried out on the screen with a convenient device. (6) Change from any one of the above modes of operation to another is practically instantaneous. One can obtain all the information desired from a photograph before passing to another, and passage to the next photograph is accomplished by touching a switch which activates a solenoid and advances the film through the camera. The observer sits comfortably on an adjustable chair, looking straight ahead, with notepaper at his elbow and all mechanical and electrical controls conveniently placed. Thus eye and arm strain are reduced.

II. General Description

The apparatus has two main parts. The first is a strong box open at one end, which contains a mirror and carries a Sept camera operated by a solenoid. In taking photographs this box rests on Helmholtz coils surrounding the cloud chamber as in Fig. 1, the chamber axis being vertical. The second part, made from an old photostat machine, is shown in Fig. 2. This machine has a horizontal platform, on which the camera box can be laid for reprojection, the direction of the rays being horizontal. At the front there is a vertical translucent screen with means for moving it, and a mirror for stereoscopic observation.

1. *The Camera, Box, and Procedure for Photography.* Figure 3a illustrates the arrangement for photography. One view is produced by rays proceeding directly from the cloud chamber to the camera and the other by rays reflected from a front-surfaced mirror 28″ × 5″, coated with a chromium-aluminum alloy. We shall find it convenient to refer to the two images on the negative as the direct and indirect photographs, respectively. The center of the camera lens is about 1.25″ from the mirror plane. This is about half the interpupillary distance of the human eyes, as it should be for correct stereoscopic viewing. The distance from the lens to the central plane of the chamber is 35″ and the chamber is 6.25″ in diameter and 2.25″ deep. These dimensions cause

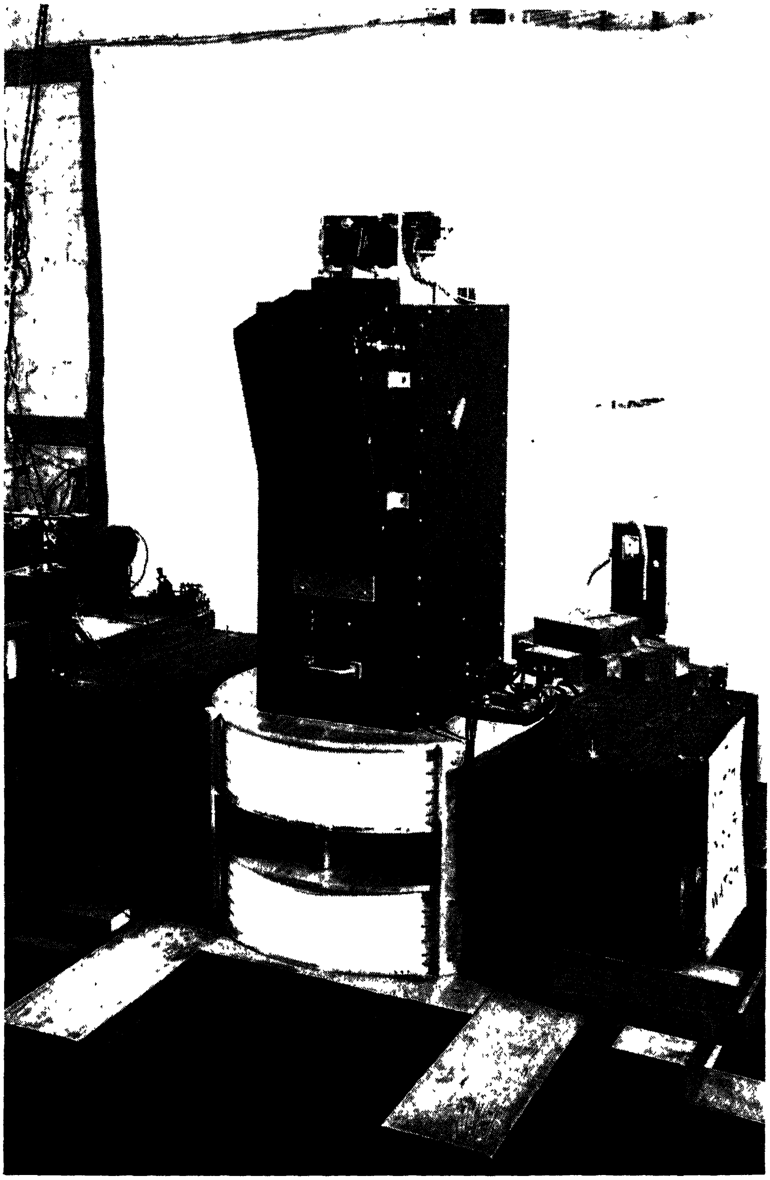

FIG. 1. Camera and box in position for photography.

both images to fall comfortably within the limits of a single frame of 35 mm. film when the camera is so inclined that the optical axis of the lens passes approximately through the right edge of the chamber. It is desirable to make the camera-chamber distance as small as possible consistent with good focus, in order to keep the

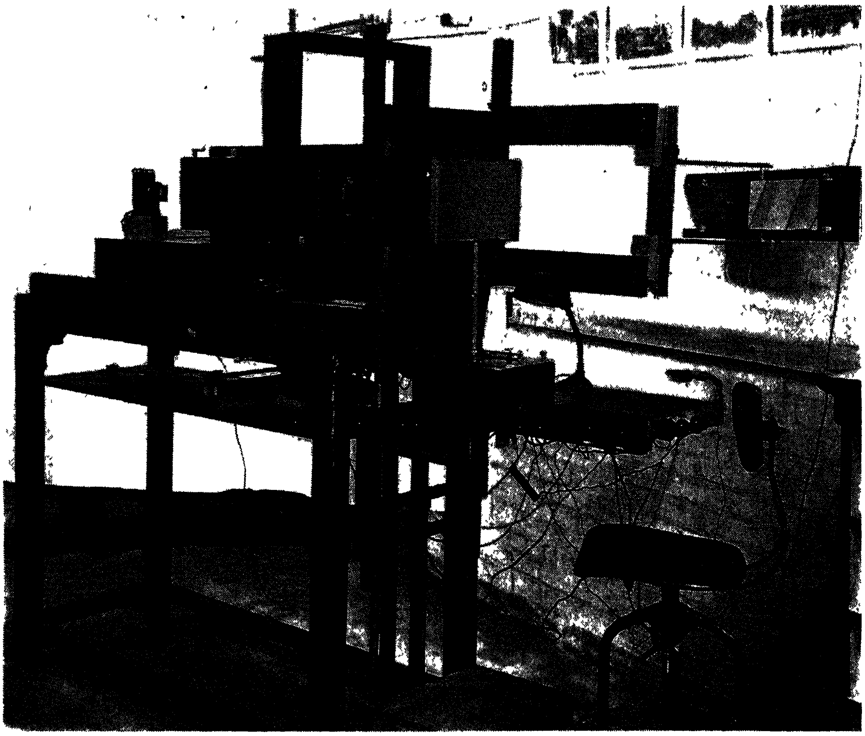

FIG. 2. Viewing and measuring machine.

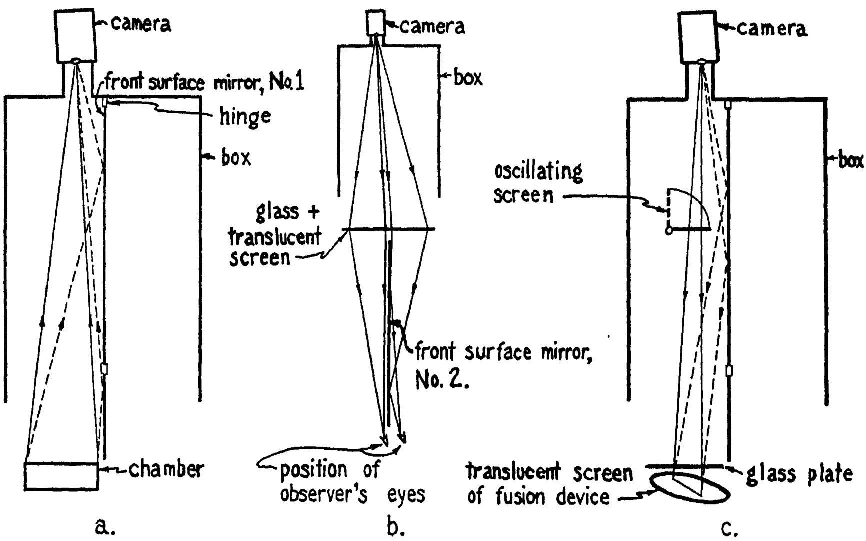

FIG. 3. Arrangements for (*a*) photography; (*b*) stereoscopic viewing and radius measurements; (*c*) determination of angle which track makes with plane normal to field.

box and mirror small. For any given maximum camera-aperture a practical compromise is required and the dimensions we have employed work well for apertures up to f:2, which is larger than we customarily use. The choice of dimensions was not affected by considerations of detail in the pictures.

The direct picture shows the entire interior of the chamber, and little is missed in the indirect view, for the mirror plane is tangent to the cylindrical edge of the chamber and the bottom of the mirror is only 1.75″ from the interior of the chamber.

2. The Viewing Machine. In Fig. 2, one sees a horizontal platform supporting the camera box which lies on its side with mirror No. 1 facing toward the left of the machine. At the extreme left of the figure is a projector lamp which illuminates the negatives through a hole in the rear of the camera. At the front there is a ground glass screen large enough to receive two full-scale images of the chamber side by side. The glass is of the same thickness as the front glass of the cloud chamber (3/8″), so that refraction errors are compensated in reprojection. A crank on the left drives a screw which raises and lowers the front platform, supporting the ground glass screen. A telescoping tube also permits adjustment of the distance between the screen and the platform. These adjustments are seldom moved, but were retained in case it became necessary to replace the screen with other viewing devices of different heights. The screen is supported on ways which permit sliding it from left to right and from front to back. To the right of the screen there is a wooden frame supporting hinges which make it possible to rotate mirror No. 2 of Fig. 3 into position for stereoscopic viewing.

3. Stereoscopic Viewing. Figure 3*b* is a plan view in which the camera box lies horizontally on the viewing machine platform. Mirror No. 1 has been dispensed with by rotating it on hinges which are fastened to one of its long edges. This is accomplished very quickly by releasing a positive catch which holds the mirror, and then pulling a cord. With mirror No. 1 out of the way, rays which would have struck the mirror are allowed to strike the right-hand part of the screen, forming an image of the chamber reversed from right to left. Rays coming from the direct photograph retrace their paths to the left-hand portion of the screen. Now, mirror No. 2 is swung into the position shown, so that one can view the left image of the chamber with the left eye while the right eye is viewing the right image after reflection in the mirror.

The mirror reverses it again and brings it into a position where the two images can be visually fused. In the stereoscopic view of the chamber the piston appears to be farther away from the observer than the top glass.

The view is undistorted or orthostereoscopic [8] and of natural size provided certain conditions are satisfied: the front nodal point of the camera should be half the interocular distance from mirror No. 1; the plane of mirror No. 2 during viewing should coincide with that of No. 1 during photography; the distance from camera to screen should equal the distance from observer to screen; each eye should be half the interocular distance from mirror No. 2; there should be no shrinkage or deformation of the film and it should be replaced in the camera in exactly the position occupied during photography. These conditions are easy to satisfy approximately, except the last two (Section III). In practice excellent vision is obtained and the head can be moved freely as long as one keeps within such limits that the two eyes see the proper views. There is little fatigue because the convergence and accommodation of the eyes are correlated in the same way as in normal seeing.

4. Outline of the Measurements. A fast electron track formed in the presence of a magnetic field H is a uniform helix in the ideal case in which scattering and energy loss are neglected. With the charge in e.m.u., the momentum p is given by

$$p = Hre/\cos\beta, \tag{1}$$

where r is the radius of the orthogonal projection on a plane normal to H, and β is the angle the track makes with this plane. We shall use a right-handed axis system with the origin at the lens center. (By using the properties of the nodal points, we can replace it by a thin lens in the following considerations.) OX is from left to right, OY is directed away from the observer, and OZ is from bottom to top (Fig. 7). To measure β, we project aerial images of the track in the fashion shown in Fig. 3*c*, and receive the rays on the ground glass screen, which is kept permanently parallel to the XZ-plane. As shown in Fig. 4*a*, this screen has a graduated slide S permitting motion along OY, so that it can be made to coincide with the image of any point on the track. Before coincidence is reached, two images of the point will be seen close together.

[8] Kurtz, *J. O. S. A.*, 27, 323 (1937); see also a paper on orthostereoscopic microscopy of alpha-ray tracks in photographic emulsions, Martin and Wilkins, *ibid.*, 27, 340 (1937).

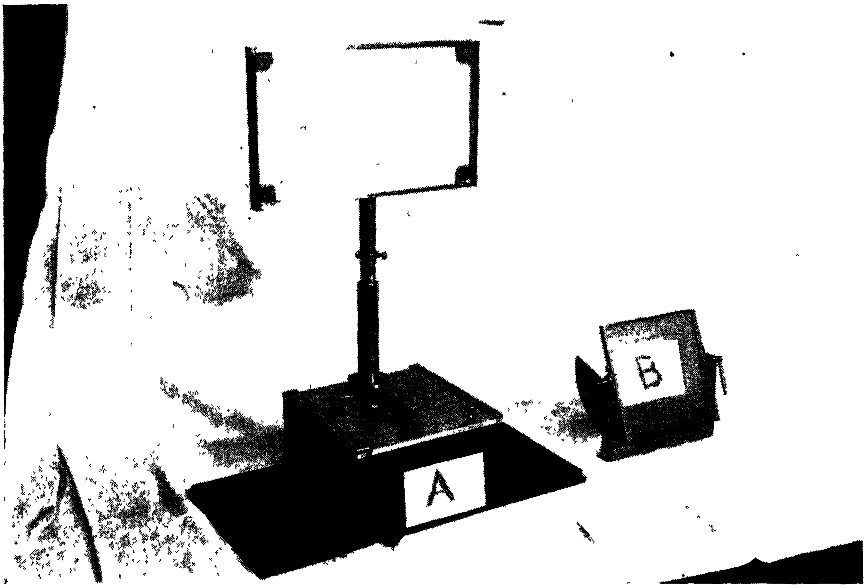

FIG. 4. Fusion apparatus. (*a*) Screen for use with curved tracks; (*b*) screen for study of straight tracks. It can be placed in any orientation when mounted on the stand in place of (*a*).

After they are "fused," other parts of the track give double images since the track is in general a helix. To increase the accuracy of setting the ground glass screen, a sheet metal screen is built into the camera box so that the rays forming the direct image can be cut off by rotating it into the path of the rays from the direct photographic image. The sheet metal screen is oscillated at the rate of one or two cycles per second by pulling a string. If the two images of a droplet are almost but not quite coincident there will be an apparent motion. The value of this opaque screen as an aid in determining the position of coincidence is great, for judgments as to the coincidence of the two images made with the unaided eye are sometimes uncertain to the extent of 5 mm. or more. Before beginning measurements, it is possible to see stereoscopically whether the track drops smoothly into the chamber. In case it does, the y-coördinates of two points 1 and 3 near the ends are determined with the graduated slide. If h is the difference of these y-coördinates, then on the assumption that the track is a uniform helix,

$$\tan \beta = h/s, \tag{2}$$

where s is the length of the orthogonal projection of the arc 1–3

on the XZ plane. The projection of this arc on the screen can be measured by sets of circles whose circumferences are graduated in millimeters, but we obtain it by measuring the chord c of the projected arc and its radius r. A table has been constructed, giving the quantity $(s - c)/c$ as a function of c/r.

To measure r, the screen is set in coincidence with a point near the middle of the aerial track image. Then mirror No. 1 may be rotated out of the way so that radius measurements can be made on the direct image without any interference from the closely neighboring points of the indirect one.

The radius is determined in the usual way, by superposing a set of circles of known radius until one is found which corresponds with the track as closely as possible throughout its entire length. Several sets of transparent circle-sheets are used. A typical sheet has groups of three circles with radii as follows: 0.8, 1.0, 1.2, 1.8, 2.0, 2.2, $\cdots$ 10.8, 11.0, 11.2 cm. Another sheet fills in the gaps and still others extend the range. This arrangement has been adopted because the clear spaces between the groups give a better view of the chamber and enable us to distinguish easily between points on the track being measured and those of adjoining tracks. In general sheets are used on which the radii are marked but in the case of a disagreement between two observers, we employ sheets on which the markings are covered. To obtain cheap but fairly accurate transparent sheets of circles, a drawing was made on tracing cloth; negatives were prepared on film by contact printing and positives were contact-printed on $8'' \times 10''$ film. After a year of use, these films are one per cent under size, and a correction for this is necessary. It would be better to engrave circles directly on glass or well-aged celluloid.

If an electron is deflected through an angle θ by a nucleus one desires to measure the angle between the tangents to the track at the point of collision. Let β_1 and β_2 be the values of β for the portions of the track on either side of the collision-point, and let δ be the angle between their orthogonal projections on the XZ plane. Then

$$\cos \theta = \cos \beta_1 \cos \beta_2 (\cos \delta + \tan \beta_1 \tan \beta_2). \qquad (3)$$

To obtain δ we use a device proposed by Dr. Forrest Western, which is shown in Fig. 5. A transparent sheet carrying a set of concentric circles possesses a celluloid arm which may rotate freely around the center of the circles on a well-made bearing. A radial

line A is ruled through this common center, and a protractor scale is laid off on one of the circles, so the angular position of the arm can be determined. The device is used in a vertical position,

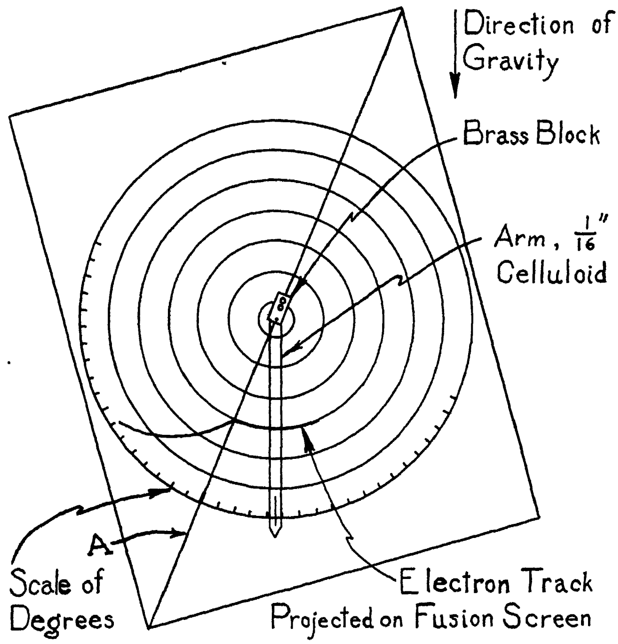

FIG. 5. Western's device for measuring angles in the plane of the screen. The brass block is fastened to the circle sheet with flat-head rivets and carries a steel wire bearing which supports the arm in a position close to the sheet. The clearance is small, and parallax is easily avoided.

pressed against the translucent screen, so that the arm hangs down freely. The intersection of the line A and the circle which is appropriate to the portion of the track under consideration, is placed at the point of collision. With that circle lying along the track, the position of the celluloid arm is read on the protractor scale. A similar procedure is carried out for the portion of the track on the other side of the collision. The difference of the readings gives a good approximation to the angle between the conical projections of the tangents.

It will be noted in Fig. 4*a* that the screen can be rotated about a vertical axis having a horizontal protractor. This motion was provided for use with the screen shown in Fig. 4*b*, which was designed for work on straight tracks and is of little use for helical ones. This screen has a horizontal axis and vertical protractor so that it can be placed in any orientation.

III. Errors and Corrections

When we have measured the apparent values of h, s, δ and r on the screen, the problem is to correct systematic errors arising in photography and measurement. The original form of the track may differ from that of the track we photograph. As pointed out by Blackett [9] a track formed before the expansion suffers a pure strain when the chamber expands. Our present work is done on post-expansion tracks, but in counter-controlled chambers the trail of ions is formed before expansion and in such cases the strain must be taken into account. The effect of turbulence must also be considered.

Next, there are troubles in reprojection. The aerial image of a track is not identical with the track itself, because of film shrinkage and inability to get the film into the exact position it occupied when the picture was taken. Finally, the angles and distances occurring in the aerial image are not the same as the corresponding quantities measured on the screen because the screen image is a conical projection of the aerial image. We shall calculate the shape of the aerial image, shall present methods for making it as nearly like the track as possible, and shall discuss the matter of conical projection on the fusion screen. The results of these calculations have been checked by special experiments, which will be presented. We shall estimate the remaining errors, both personal and instrumental, in β, r, and H. Multiple scattering is very important at low energies, and has been thoroughly treated by Zuber. Finally the errors in momentum and energy will be discussed.

1. Errors in Reprojection. Consider Fig. 6, a top view of the reprojection apparatus, considerably exaggerated. The camera axis is drawn parallel to the mirror, although in practice it is slightly inclined. This simplification affects the results very little. The camera is focussed approximately on the median plane of the chamber and v is the lens-film distance. Any point $Q(x, y, z)$ on a track produces the direct and indirect images Q_1 and Q_2 whose coördinates are

$$(xv/y,\ v,\ zv/y),\ [(2P - x)v/y,\ v,\ zv/y],$$

where P is the distance from the camera axis to the mirror.

As a result of shrinkage or displacement of the film, let Q_1 and Q_2

[9] *Proceedings Royal Society A*, **146**, 281 (1934).

shift to Q_1' and Q_2' respectively. On reprojection the rays from these points will not always cross after leaving the lens, because of the reflection in the mirror; but they will cross at some point Q' with coördinates x', y', z', if the film shrinkage is uniform and if the displacement is merely a translation. In practice, the film

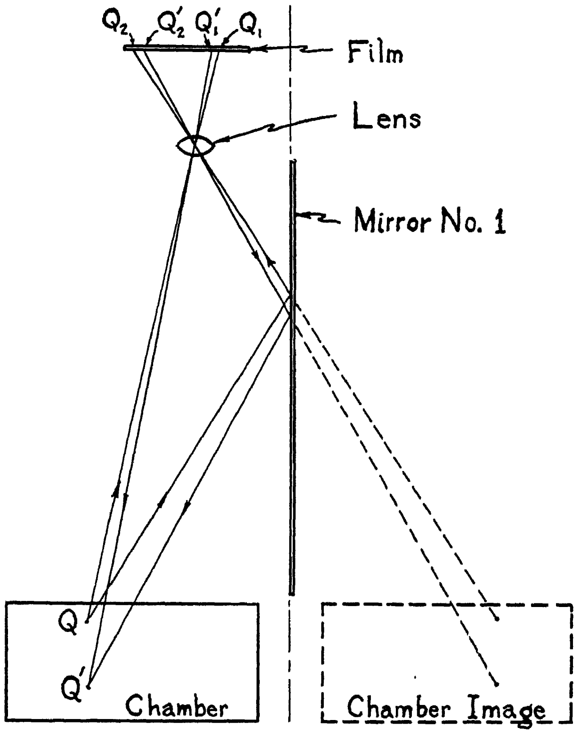

FIG. 6. Positions of a track point and of its reprojected aerial image.

shrinks to a fraction F of its original dimensions, and the fractional shrinkage $1 - F$ will be called f. Symmetrical shrinkage without displacement would change y to y/F, and the effect of this would be quite negligible; but in both photography and reprojection the film is pressed toward the left of the film gate by a light spring, and so the shrinkage determines the displacement to the left, the effect of which is important. If the action of the spring were perfect a photographic image formed at abscissa x_1 would go to $x_1 - f(w + x_1)$ where w is half the film width; but ideal conditions are not attained so we must consider the effect of a superimposed translation, a, in the positive x-direction, and of a translation, b, in the y-direction. The effect of shrinkage, spring pressure, and

displacement in the positive x-direction is given by the equations

$$x = (S/F)(x' + \alpha y'); \qquad y = Sy'; \qquad z = Sz', \tag{4}$$

where

$$S = \frac{F}{1 + (\alpha y'/P)} = F - (\alpha y/P); \qquad \alpha = \frac{fw - a}{v}.$$

A translation b in the y direction, acting alone, gives the result $x' = x$, $y' = y(1 + b/v)$, $z' = z$. In practice the maximum possible value of b is so small that we shall not consider it further; moving the film as far as possible in the y-direction changes y' by only 2 mm. The film gate of the camera was modified by adding a plate of transparent celluloid, and guides on the gate have been tightened; to hold the film more nearly in the same position during photography and reprojection.

In processing the 35 mm. film the shrinkage measured soon after drying is about 0.3 per cent, but further shrinkage occurs over a long period. The final value often lies in the range 0.004 to 0.006, but it may reach values higher than 0.01 when the films are kept at low humidity. For some weeks or months the size can be increased almost to the original value by keeping the film in an atmosphere of high humidity, but after a year, overnight exposure to high humidity produces no easily detectable expansion. These statements refer to Eastman Super X stock processed with Champlin's No. 15 developer and a hardening bath of sodium bisulphite and chrome alum. With shrinkage of the magnitude described, equations (4) predict large effects and these are found. For example, putting $f = .003$, $w = 1.75$ cm., $y = -86$ cm., $v = 5$ cm., and $P = 3.2$ cm., we get $\Delta y = +2.25$ cm. Putting $f = 0$ and $a = 0.01$ cm., we have -4.6 cm. Moving the film by hand in the positive x-direction, Δy could be made as large as -10 cm., corresponding to $a = 0.02$ cm. This represents an extreme condition never realized in ordinary work. In practice, the shift is always inward (*i.e.*, Δy is positive) and may be as much as 5 to 6 cm. The relatively large shifts caused by motion along OX are associated with the fact that the reflected rays strike the mirror at a large angle and with the relatively short focal length of the camera, but it is not practical to reduce the shift by changing the dimensions of the apparatus, which are convenient as they stand. It is desirable to reduce the shift because of the fact that the point Q' may recede to a position outside the range of travel of the viewing screen, whose motion is normally limited

by the end of mirror No. 1. Reduction is also desirable to make corrections smaller. Curtiss and others have used pins to improve relocation; the Sept camera has a special set of teeth which engage the sprocket holes in the film, but the action is such that accurate relocation does not occur and improvements would be difficult. Accordingly other plans were tried. We humidified the film, with unsatisfactory results; a displacing device was then built into the film gate, but did not prove satisfactory. The plan finally adopted consists in rotating the camera around an axis parallel to OZ before reprojection, by means of adjusting screws. The rotation required to make the y-coördinate of the image point equal to that of the point P on the track is of the order of 0.15 degree. The effect has been analyzed in detail; neglecting the square of the angle of rotation G, and such terms as αG, the coördinates of Q' become

$$\begin{aligned} x' &= xF + Pf + [\alpha + (Pf/y)]x^2/y, \qquad (5)\\ y' &= y,\\ z' &= zF. \end{aligned}$$

In x', the term Pf is merely a constant displacement and the following term is only 0.5 mm. in the worst case, so we neglect them and write $x' = xF$.

The effect of conical projection on the rays from the direct photographic image is shown in Fig. 7. We use capital letters X, Z, R, etc., for the points and distances on the screen which correspond to x, z, r, etc., on the track. Let the screen be set so that fusion is achieved at distance y_M for the middle point M of the arc to be measured; this choice of position simplifies the formulas and reduces the corrections. Let η be the y coördinate of any point on the aerial image, measured from screen toward camera. Then

$$x' = X\left(1 + \frac{\eta}{y_M}\right), \quad y' = y_M + \eta, \quad z' = Z\left(1 + \frac{\eta}{y_M}\right), \qquad (6)$$

and the transformation relating the original track to the screen-image is

$$x = \frac{X}{F}\left(1 + \frac{\eta}{y_M}\right), \quad y = y_M + \eta, \quad z = \frac{Z}{F}\left(1 + \frac{\eta}{y_M}\right) \cdot \qquad (7)$$

2. Correction of the Radius. We assign the sense 1, 3 to the track and to its chord in Fig. 7 and make h positive if point 3 is closer to the camera than point 1. Let φ be the angle between

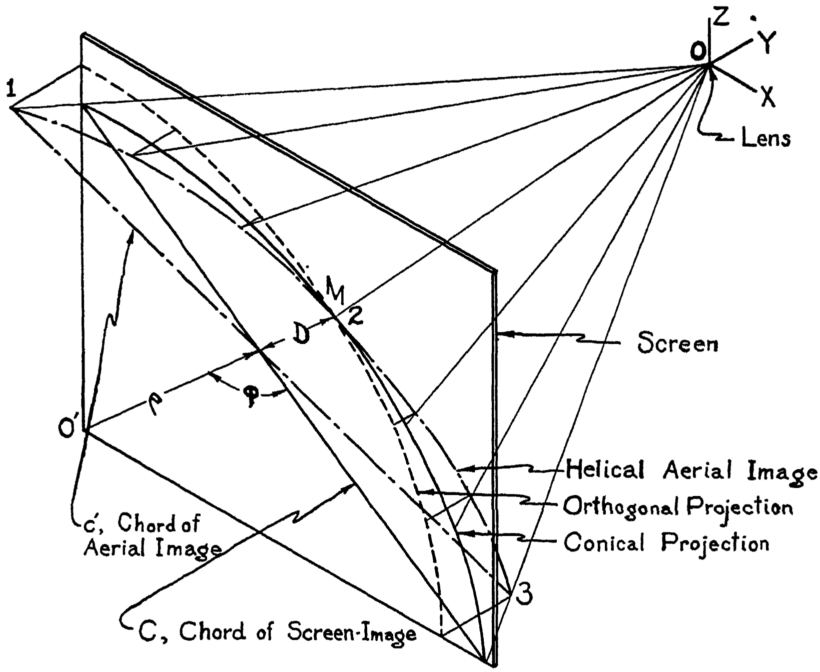

FIG. 7. Conical projection of the aerial image on the screen.

the chord C on the screen and the radius vector ρ drawn from point O' on the Y axis to the midpoint of this chord. The midpoint lies on the screen, at a distance D from M. The corresponding length in the space of the track is D/F. Then the chord c of the orthogonal projection of the track is related to C as follows:

$$c^2 = (C^2/F^2)\left(1 + \frac{2h}{y_M}\frac{\rho\cos\varphi}{C} + \frac{h^2\rho^2}{y_M{}^2C^2}\right). \tag{8}$$

By the rule that the product of two intersecting chords of a circle is constant, we get

$r = (c^2/8d) + (d/2)$, so that

$$r \cong (1/F)\left[R\left(1 + \frac{2h}{y_M}\frac{\rho\cos\varphi}{C} + \frac{h^2\rho^2}{y_M{}^2C^2}\right) - \frac{D}{2}\left(\frac{2h}{y_M}\frac{\rho\cos\varphi}{C} + \frac{h^2\rho^2}{y_M{}^2C^2}\right)\right], \tag{9}$$

where $R = (C^2/8D) + D/2$. The transparent ruled sheet in Fig. 8 yields $\rho \cos \varphi$, C, and D in one operation and by adding a graduated arm ρ can be obtained. We incur negligible error if we replace y_M in the correction terms by y_0, the value of y for the midplane

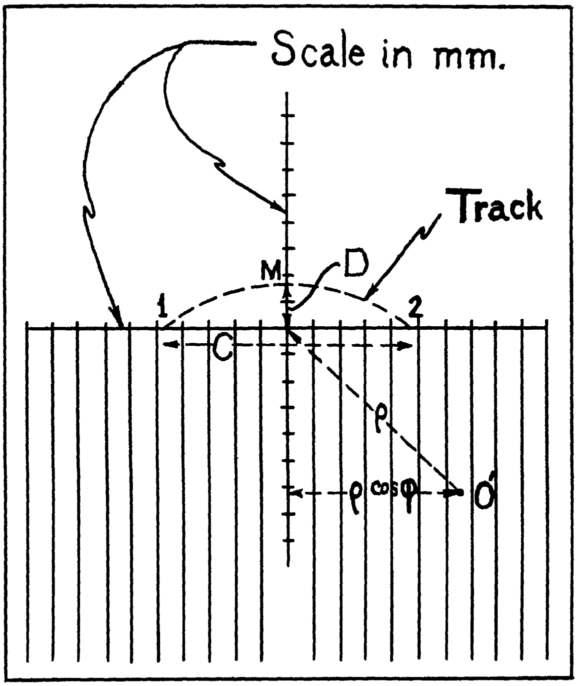

FIG. 8. Device for getting chord, D, and $\rho \cos \varphi$.

of the chamber; to avoid calculations we have plotted a family of lines so that $2h\rho \cos \varphi/y_0$ can be read off when h/C and $\rho \cos \varphi$ are given. The quadratic correction is treated similarly; it is often negligible.

The question arises whether the radius measured with circle-sheets agrees with the quantity R defined in terms of C and D. The screen-image of a track is not a circle, but if the screen is set so that the middle point of the arc is fused, the portions near this middle point agree closely with the orthogonal projection of the aerial image, which is very nearly a circle according to equations (5). Further the largest deviations of the conical projection from the orthogonal one occur when the track is located well toward the left of the screen (large negative x). In such cases the curvature increases from one end to the other, so a circle chosen to

conform to the average curvature of a large arc has a radius agreeing well with that of the aerial image. To test these conclusions we made helices of stiff wire soldered to brass rods, with $h = 5$ cm. for one complete revolution, and with radii of about 4.2 cm. These were photographed in positions for which the correction terms in eq. (9) are large. In reprojecting, the camera was rotated until a known reference point was brought out to the correct position. The shift required was 3.0 cm. The film shrinkage was determined from this value, using (4) and (5), and it was measured directly with a comparator. The two values, $f = 0.44$ and 0.43 per cent, are in good agreement. In three representative cases the corrected radii were 4.18_2, 4.15_5, and 4.18_4 cm. respectively while the actual value is 4.161 cm. On these excellent "artificial tracks" two observers agree in their estimates of the uncorrected radii (R) to about 1 per cent, or 0.05 cm. The values above are based on very painstaking work by one observer. They are within 0.5 per cent of the true radius. In dealing with actual tracks such care would not ordinarily be taken and it seems fair to say that without undue pains the method can yield results correct to 0.1 cm. when applied to helical objects of about 5 cm. radius for which β is about 20°.

Even in cases where a track has obviously suffered some multiple scattering we find that different observers agree in their estimates of *average* radii to about 0.2 cm., when R is less than 8 cm. For larger radii, up to 20 cm., a single observer will often repeat radius readings to 0.4 or 0.5 cm. However, such agreement does not indicate the magnitude of the error. It is easy to see from the approximate formula $R = C^2/8D$ that if D can be measured within 0.025 cm. and C is 6 cm. the error in R runs from 0.55 cm. at $R = 10$ cm. to 2.5 cm. at $R = 20$ cm. It is difficult to make exact statements about the errors in large radii. Each investigator has his own technique in matching circles to tracks and the act is not as simple as it appears. The moral is that large radii should be avoided, as far as possible, by proper choice of the magnetic field, or a technique like that of Blackett (reference 7) should be employed.

The correction terms in r are large for tracks unfavorably placed, so the formulas developed here are essential for avoiding systematic errors in radii. While multiple scattering can falsify energy measurements on electrons of 1 or 2 MEV by many per cent, it must be realized that such errors partly average out in

statistical results on large-angle scattering or energy-loss. This implies that even in the "radioactive" range of energies care should be taken to avoid both systematic and random errors in measurement. In the cosmic ray range, where multiple scattering gives little trouble, the full accuracy provided by the formulas can be utilized to get precise results on any individual event which merits careful measurements.

It may be of interest to indicate how the radius of an electron track is affected by the energy loss due to ionization. It can be shown from the data of Corson and Brode and of Brode and Bagley [10] that the fractional decrease in radius in going 10 cm. is as follows:

Energy in MEV	$-\delta r/r$
0.3	.043
0.5	.025
1.0	.012
2.0	.0065

3. Correction of β. Equation (7) shows that h is given directly by fusion measurements. By using considerable care the uncertainty in Y-values can be reduced to 1 mm., but in ordinary work on well placed tracks with β not more than 30°, one is often content to obtain these values to about 2 mm. The routine determinations of one skilled observer showed an average deviation of 1.4 mm.

To get s, one uses the table mentioned in connection with equation (2), after working out c and r, and β is calculated from equation (2). Experiments with helical models showed that chords of about 8 cm. length could be measured with an accuracy better than 1 per cent. In general the error in β is mostly due to that in h.

4. Correction of δ. It is not useful to give an explicit formula for correcting δ. Such a formula would be too complicated for general use. If it is obvious from the nature and location of the scattering event that the value of δ, measured with the device of Fig. 5, is afflicted with considerable error, one can get $\tan\delta$ from the slopes, dz/dx, of the two branches of the track. From (7), considering η as a parameter on which X, Z, and x, z depend,

$$\frac{dz}{dx} = \frac{dZ}{dX}\frac{1 + \dfrac{Z}{y}\dfrac{d\eta}{dZ}}{1 + \dfrac{X}{y}\dfrac{d\eta}{dX}}. \tag{10}$$

[10] *Phys. Rev.*, **53**, 773 (1938), and correction by Brode and Bagley, *Bull. Amer. Phys. Soc.*, **14**, No. 3, 10 (1939).

Algebraic complexities are avoided by using the approximate numerical values of the quantities on the right. Equation (10) can also be applied when track radii are determined by measuring arcs and slopes (Simons and Zuber, reference 6). The device shown in Fig. 5 is mechanically capable of giving readings to 0.2 degree and it could easily be improved. However, this would serve no useful purpose, for the following reasons. (1) It is sometimes difficult to measure a scattering angle closer than one degree because of the finite size of the droplet-images and the irregular distribution of the droplets. There may be no droplet at the point of collision, and even when there is, the size of the image may introduce uncertainties. The difficulties are of course most acute when the scattering angle is small and the direction of deflection is toward the axis of the helix on which the particle moves. (2) The instrument cannot yield accurate values of δ when the pitch of the helical track is large, for it takes account of the position of the whole arc, rather than the value of dZ/dX at the scattering event. For accurate work on scattering angles one may substitute the well-known mirror device for determining tangents, with a hanging arm and protractor attached to it.

5. Errors in the Field. Many observers use Helmholtz coils which fit closely around the chamber and also place the coils closer together than called for in the classical design, to increase the field. To minimize inhomogeneity, we used the Helmholtz proportions, designed our coils on a more ample scale (22″ O.D., 11″ I.D., cross-section 4.48″ × 4.76″) and wound them with square asbestos-insulated wire. Using 220-volt power, the maximum field available is about 2500 gauss, which is well within the limits of thermal and mechanical safety for intermittent operation. (N.B., forces between such coils amount to several tons.) For electrons between 0.5 and 2.5 MEV we can use 900 gauss without cooling but at 1250 gauss an electric fan is employed. Keeping the current on for 5 seconds in every minute, the temperature rise in 4 hours is about 10° C. in the latter case. This has no detectable deleterious effects on tracks formed with 95 per cent ethyl alcohol, but on several occasions, with the coil operating about 8 seconds in every minute, the temperature rose to 39° C., and this invariably caused poor tracks. This is in agreement with the observations of others on the effect of temperature.

The maximum deviation from the field at the center is 0.7 per cent, occurring at the outside edge of the midplane, and the average

deviation over the whole chamber is less than 0.1 per cent on the basis of the formulas for ideal coils.[11]

The current is read for each expansion of the chamber on a meter calibrated by potentiometer. It is nominally a "half per cent" meter, but this figure means the accuracy obtained when the meter is used without calibration. We find that successive readings on the same current check to 0.25 per cent. Considering all sources of error we believe the field is known within 1 per cent.

6. *Observational Errors in Momentum and Energy.* From equations (1) and (2)

$$\frac{\Delta p}{p} = \frac{\Delta H}{H} + \frac{\Delta r}{r} + \sin^2 \beta \left(\frac{\Delta h}{h} - \frac{\Delta s}{s} \right). \tag{11}$$

Also, if the kinetic energy T and momentum p are expressed in units of mc^2 and mc respectively, we have

$$\Delta T/T = [p^2/T(T+1)](\Delta p/p). \tag{12}$$

The following correlated values of T and of the bracket suffice for drawing a curve: 0.25, 1.80; 1, 1.5; 2, 1.33; 3, 1.25; 5, 1.17; 10, 1.09.

No statements of general utility to other workers on electron tracks can be made concerning the values of $\Delta T/T$, for the errors in r, h, and s depend on the energy of the particles, the age of the tracks, and other factors. However, in an investigation of electrons with energy from 0.3 to 2.2 MEV in air, we concluded we were dealing with the following *average* errors in momentum, the estimates being conservative ones:

Coil Construction	<1 per cent
Current	0.4 per cent
Radius	2.6 per cent
β	0.8 per cent
Multiple Scattering	12.3 to 9.7 per cent

At present we could do better with the observational errors, due to improvements in apparatus. Scattering errors were worked out with the aid of Bothe's semi-empirical formula, the application of which to gases has been clearly discussed by Zuber.[12] Compounding these independent errors according to the usual formulas, the error in the energy of an individual electron ran from 21 per

[11] Ruark and Peters, *J. O. S. A.* and *R. S. I.*, **13**, 205 (1926).
[12] Zuber, *Helv. Phys. Acta*, **11**, 207 (1938).

cent at 0.3 MEV to 14 per cent at 2 MEV, most of this being due to multiple scattering.

We wish to thank Dr. Forrest Western of Lincoln Memorial University who participated actively in the study of errors and the establishment of sound working procedures. We are grateful to the American Philosophical Society for a grant which made it possible for one of us (C. C. J.) to undertake work on the nuclear scattering of electrons. The techniques described in this paper were developed as a necessary part of this work.

PHOTOGRAPHIC METEOR STUDIES, II

NON-LINEAR TRAILS

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(Communicated by Harlow Shapley)

ABSTRACT

Several meteor trails on plates taken with the larger Harvard patrol cameras have been found to be sinuous, with total amplitudes up to 30 microns. The sinuosity of these trails is shown to have been caused by the vibration of the cameras, and not to have originated in the meteors themselves. In one case the vibration was maintained by the one-impulse-per-second motion of the camera drive. The frequency of 20 vibrations per second has been determined from a long meteor trail and represents the natural vibration of the camera. In a second case a forced vibration of 16 per second was maintained by the continuously operating motor drive.

The oscillations in the trails can be used in exactly the same fashion as interruptions from a rotating shutter for the determination of angular velocities, radiants, heights and linear velocities. A Geminid trail provided a satisfactory check on the method, as did deceleration measures near the end of a long trail. The spatial orbits of two sporadic meteors have been determined and found to be elliptical.

Numerical data concerning the physical process of splitting have been deduced for a long trail which divided near the end. Splitting is probably caused by the thermal expansion of the outer layers of the meteoroid when it enters the earth's atmosphere.

1. *Observations.*—In his unpublished notes on meteor trails found on Harvard plates, the late Dr. Willard J. Fisher listed ten trails that showed obvious deviations from linearity. The most conspicuous case was a meteor photographed with the Ross-Fecker lens (focal length 20.8 inches, aperture 3 inches) on plate RH 2075. This trail was studied by Miss Olmsted [1] for sinuosity and for light variations. The double amplitude was observed to average 0.04 mm over a distance of 2.7 mm on the plate and the sinuosity was not correlated with the light variations. Miss Olmsted suggested that the non-linear trail might have been produced by rotation of the meteoroid or by vibration of the camera but she carried the investigation no further. The author, on inspecting Harvard photographs of meteors, has found several other cases of sinuosity. Occasionally, where the lengths of the waves were great, detection was expedited by observation at a grazing angle to the surface of the plate, the eye being directed nearly along the

[1] M. Olmsted, *H. B.* 888, 1932.

length of the meteor trail and aided by a low-power magnifying glass.

If sinuosity in the trails were produced by the actual motion of the meteoroid, accelerations normal to the path far greater than the decelerations produced by air resistance [2] would be required. If sinuosity were an effect on the luminous train caused by rotation of the meteoroid the phenomenon would be of great importance to the theory of the physical processes involved. If vibration of the cameras were the cause, a detailed study would be necessary from the point of view of technique, and the vibration might be of practical value in determining velocities.

With these possibilities in mind, the author selected for detailed study five meteor trails that were conspicuously sinuous. Three were trails of shower meteors for which linear velocities are approximately known and one was an unusual trail illustrating the phenomenon of splitting. Six to ten comparison stars were chosen for each plate, and accurate position determinations of the trails made in the manner described in the previous paper.[2] Careful measures were made of the y-coordinate perpendicular to the trails at frequent intervals in the x-coordinate along the trails, so that short-period deviations from linearity might be detected. The trails were oriented as well as possible parallel to one of the screws of the Coast and Geodetic Survey measuring engine; so also was one of the crosswires of the microscope. This procedure has an advantage in accuracy over the use of diagonal crosswires because a finite length of the trail is averaged in y at each measure, but it has a disadvantage because the true amplitude of the waves may be underestimated when sharp maxima or minima are measured.

Graphs of the measures along the five trails are reproduced in Fig. 1. The y-scale (ordinate) is greatly magnified with respect to the x-scale. The unit of the scales is 1/24 inch, the value of one revolution of the screw. The arrows in the figure indicate the directions of motion. Meteors are identified by the number of the plate on which they were photographed, and by the Harvard file number. The RH camera is described above; the RL camera is equipped with a Ross-Lundin lens of 4-inch aperture and a focal length of 27.2 inches. The coordinates in Fig. 1 have been corrected for distortion of the lens and are referred to a precise plane by the method previously employed.[2] Residual errors may exist,

[2] F. L. Whipple, *These Proceedings*, 79, 499, 1938.

however, because of lens aberrations that produce systematic effects in the relative measures of stellar images and meteor trails. Such effects would be most serious near the edges of the field.

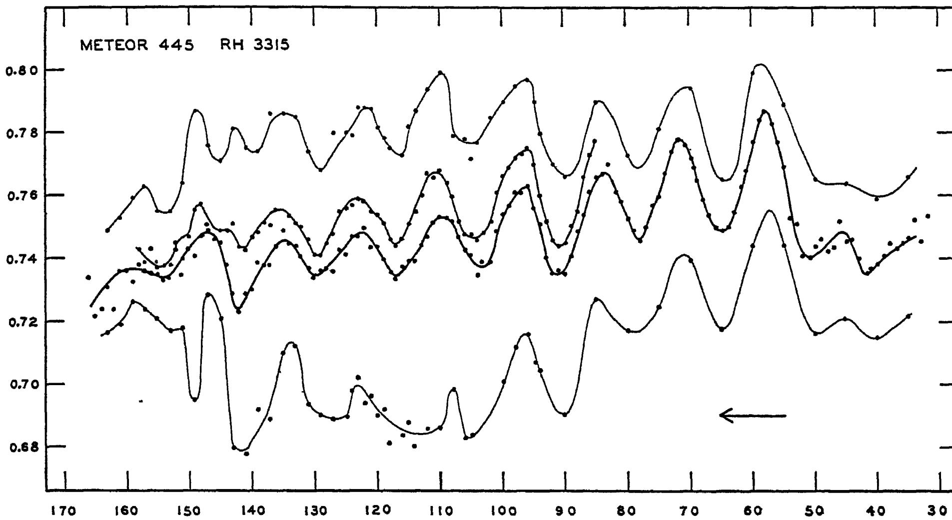

FIG. 1*a*.

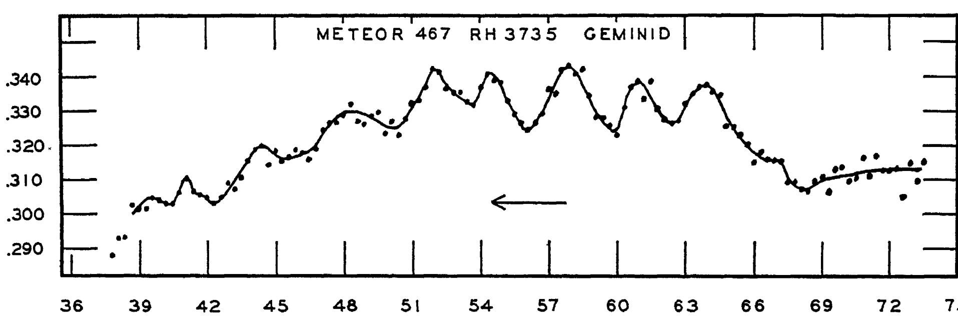

FIG. 1*b*.

For the Perseid on RH 3315, measures were also made of the edges of the trail as indicated by the upper and lower curves of Fig. 1. The splitting of the trail is indicated by the second central curve beginning at $x = 85$. The main central curve represents the apparent center of gravity of the trail. An important characteristic of the first large oscillation in this trail is that the center of gravity moves outside the trail as measured by the edges half a period before and after. Such a motion would be unlikely were the deviations from linearity produced in an oscillating train; the

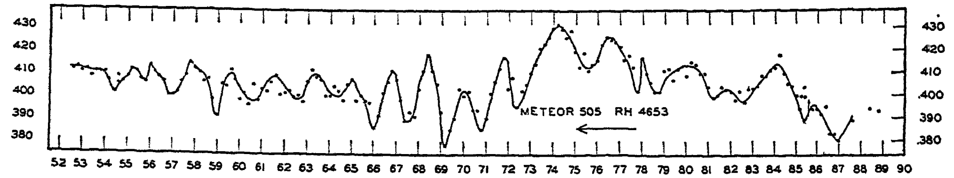

Fig. 1c.

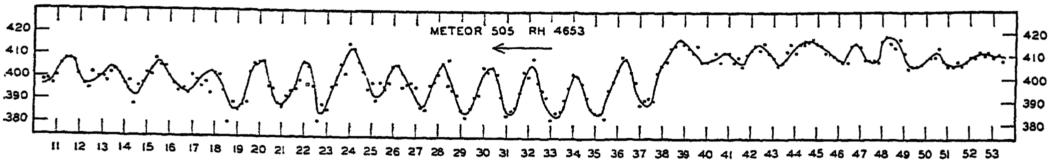

Fig. 1c′.

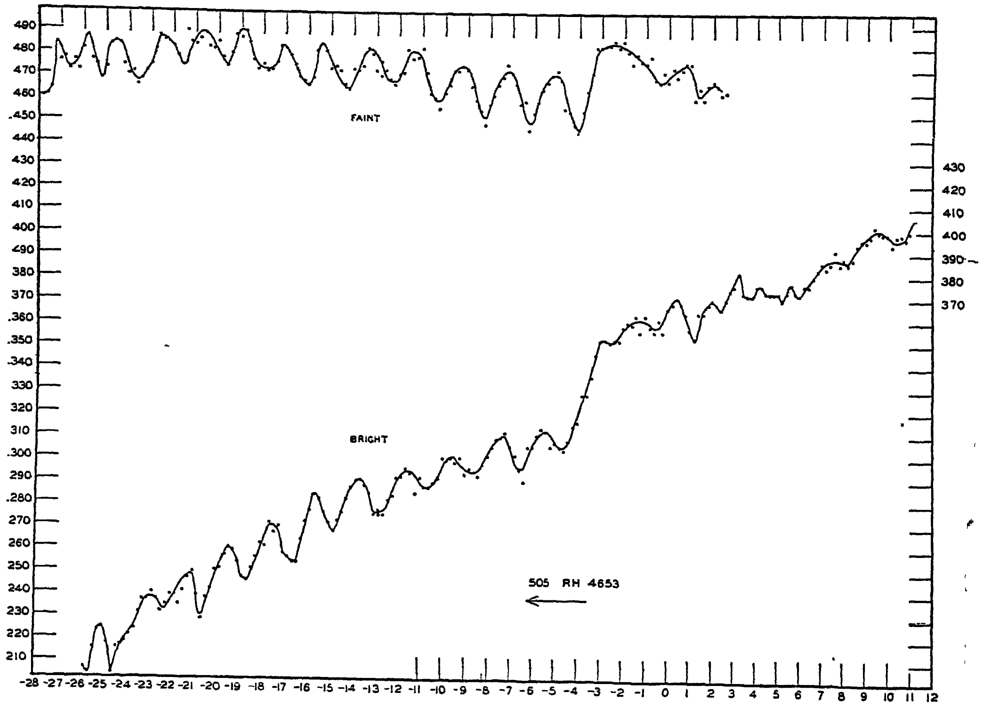

Fig. 1c″.

principal luminosity should then be produced in the immediate neighborhood of the meteoroid. (Miss Hoffleit's study [3] of a sinuous trail photographed with the 36-inch reflector at the Steward Observatory showed that the amplitude of the vibration was less than the width of the trail.) Inspection of Fig. 1 shows too that

[3] D. Hoffleit, *H. B.* 903, 1936.

both edges of the trail on RH 3315 follow roughly the same curve as the center of gravity, except for the effect of flares (particularly near $x = 142$). Most important of all, the second component of the trail, when measurable, follows both the center of gravity and the edges. Likewise the two components of RH 4653, Fig. 1c'', oscillate synchronously.

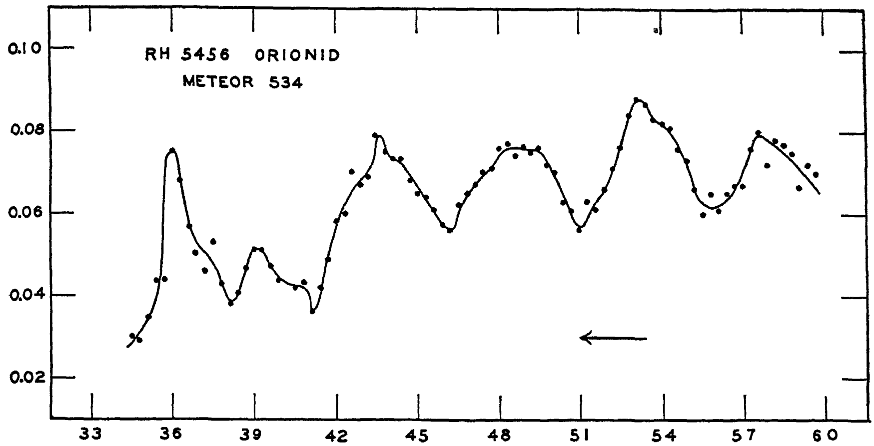

FIG. 1d.

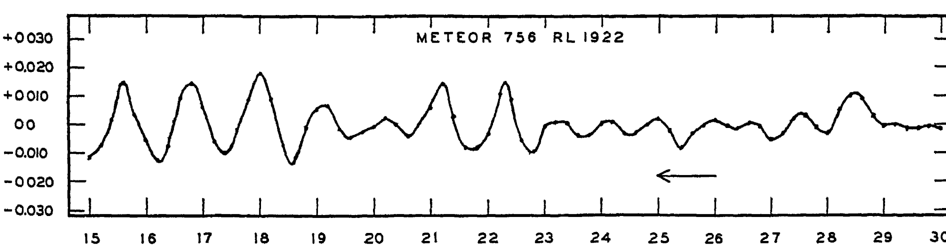

FIG. 1e.

These facts are strong evidence that the deviations from linearity occur in the optical path or camera and not in the meteor. As additional evidence several trails on plates taken with the 8-inch Bache, the 16-inch Metcalf, and the 24-inch Bruce telescopes were inspected. If the oscillations were in general due to the meteors or to the atmosphere, they should show as gross waves on some of these plates of from two to six times the scales of the RH and RL cameras. No conspicuous examples of sinuosity were observed, though there were a few instances in which deviations from linearity were detectable.

The assumption that the non-linearity is due to camera vibration provides a ready explanation for a certain peculiarity of three of the RH meteor trails. At $x = 50$ on RH 3315 and $x = 66$ on RH 3735 the amplitudes increase suddenly and the mean y-coordinates shift appreciably. The phenomenon occurs three times on RH 4653, at $x = 73$, 38 and -3, in the last case for both components of the trail. The camera was, at the times of these exposures, operated by the discontinuous Gerrish drive, motion being imparted each second by an electromagnetic escapement. It is clear that the shifts in y were produced by the drive impulse and that the amplitude of the vibration was increased each second by the shock of the electromagnet at the base of the mounting. Both the directions and the amounts of the shifts would seem to confirm this explanation. For RH 3315 it appears that a very large amplitude occurred because the natural period of the camera was nearly commensurable with the period between signals. Thus the impulse came in phase with the damped vibration.

The free period of vibration for the camera can be obtained from RH 4653 where three clock signals are registered. The pendulum of the central clock has a complete period of two seconds and therefore the interval between the first and third signals should be exactly two seconds; but, as the signal is sent by a magnet supported at the bob, the interval between successive signals need not be exactly one second. Unless the centering is perfect the time between alternate signals may vary appreciably. A study of the maxima and minima for the RH 4653 meteor indicates that there are 20.0 vibrations per second, with a probable error of about 2 per cent. The period is not more precisely determined because the measures are indefinite near the split of the trail.

In spite of the overwhelming evidence that the RH camera may vibrate, it seemed essential to observe such vibration by direct means and to investigate the RL camera which is driven continuously by a motorized Gerrish drive. Mechanical-optical methods for amplifying small motions or vibrations were difficult to apply because the measures were necessarily made while the cameras were in motion. Electrical "pick-ups" for the detection of vibration proved to be unsatisfactory because of the low frequencies and amplitudes of the vibrations. A stroboscopic method was finally found to be satisfactory. A Spencer Lens Co. binocular microscope with a magnification of twenty times was mounted on a steel tripod and focused on the back of the camera to be tested.

A viewing surface of waxed tinfoil provided an extended area of pointlike images. Observations were made at night with light from a General Radio Co. Strobotac. The Strobotac emits a series of exceedingly short flashes the period of which can be controlled and measured to a precision of about one part in a thousand.

A vibrating point observed under these conditions can be made apparently to stand still when the Strobotac frequency is adjusted to the frequency of vibration. Multiple points are seen at multiple frequencies. The continuous motion of the camera has no appreciable effect on the accuracy of measurement. It was impossible to investigate the RH camera as it was at the time the sinuous trails were photographed, because the camera has since been completely rebuilt and remounted. Tests were made, however, on a similar camera operated as the RH was operated originally. The tests revealed a vibration of the same order of magnitude in period and in amplitude as was observed in the meteor trails of the RH plates.

The RL camera is mounted on a concrete pier that is one of four piers supporting four small cameras at the Oak Ridge Station. The four piers support in common a concrete platform some seven feet above ground. The cameras are operated (RL now has a separate drive) by a continuously running motor controlled by a pendulum clock. When the microscope tripod was set up on the platform it was found to vibrate with the frequency of the motor (960 R.P.M.). An independent support for the tripod was temporarily arranged and tests of the RL camera were made. In some positions of the camera the motor frequency could be detected but the double amplitude was small (15± microns). With the motor shut off no conspicuous natural frequencies of the camera could be measured when it was disturbed. Although the measurements were difficult because of the unstable nature of the tripod support, it is reasonably certain that the camera vibration is induced by the motor and that it becomes appreciable only in certain positions of the camera.

We shall assume, therefore, that the frequency of vibration of the RL camera was 16 per sec at the time the meteor was photographed on plate RL 1922, and that the RH camera vibrated at a frequency of 20 per sec to produce the sinuosities in the trails of the present study.

2. Height, Velocity, and Orbit Determinations.—If the sinuous trails are produced by camera vibrations of known frequency,

meteor radiants, heights, and linear velocities can be determined by the method used by Millman and Miss Hoffleit [4] and discussed by the author.[2] The radiant is determined by a geometrical consideration of the apparent change in separation of points along the trail equally spaced in time. The height and velocity are determined from the angular velocity and from the relation between the height of maximum light and the linear velocity.

As can be seen by inspection of the figures, the method can be used only when a number of vibrations are measurable. The determination of the radiant is complicated by the fact that the period of the RH camera sometimes appears to decrease with time immediately after an impulse.

TABLE I

Meteor number	445	467	505	534	756
Plate number	RH 3315	RH 3735	RH 4653	RH 5456	RL 1922
Date	1931 Aug. 16	1931 Dec. 15	1932 Nov. 4	1933 Oct. 19	1938 July 30
Class	Perseid?	Geminid	Sporadic	Orionid	Sporadic
Length of trail	15°.6	4°.1	13°.5	3°.0	1°.4
Number of vibrations	11	8	41	5	14
Length of time	$0^s.5$	$0^s.35$	$2^s.00$	$0^s.25$	$0^s.81$
B (1/24 inches sec^{-1})	2 532	0.5492	0.3275	0.935	0.1221
p.e.	±0.008	±0.0190	±0.0032	±0.006	±0.0016
C (1/24 inches sec^{-2})	—	0.3085	0.0257	—	0.0424
p.e.	—	±0.0523	±0.0014	—	±0.0019
App pg. mag. (max. light)	−8 65	−3.77	−5.02	−3.51	−1.72
App. vis. mag. (max. light)	−6.85	−1.97	−3.22	−1.71	+0.08
v (sec^{-1})	—	0.4517	0 1341	0 376	0 3237
log i (ergs sec^{-1} cm^{-2})	—	−3 37	−2.87	−3.48	−4.19
Pole of trail (1900.0) λ	−0.43461	−0.44562	−0 09492	−0 66321	+0.99224
μ	−0 73780	−0 62840	+0.67348	−0.22114	+0 05260
ν	+0 51650	+0.63760	+0.73308	+0.71502	+0.11267

NOTES TO TABLE I

Meteor 445. Although classed as a Perseid by Miss Hoffleit [5] in the Second Catalogue of Harvard Observatory Meteor Photographs, the great-circle path of this meteor misses the radiant ($\alpha = 45°$, $\delta = +57°$) by 11°. Possibly the zenith correction would remove some of the discrepancy but the author prefers not to analyze the material further. If the meteor actually belongs to a radiant near that of the Perseids, then the apparition occurred after the body had passed the point of nearest approach to the camera—an infrequent occurrence. The exposure time of RH 3315 was 139^m, but the non-appearance of the meteor on other plates limits the Cambridge sidereal time to the interval from $20^h 45^m$ to $21^h 24^m$.

Meteor 467 Occurred also on plate AC 29731. Cambridge sidereal time of apparition is in interval from $6^h 55^m$ to $7^h 55^m$. Miss Hoffleit's [6] identification of this meteor as a Geminid is verified by present analysis.

Meteor 505. Cambridge sidereal time of apparition lies in the interval from $22^h 49^m$ to $0^h 41^m$ by absence of meteor from plate AI 30508.

Meteor 534 Cambridge sidereal time of apparition is limited only by the exposure limits of the plate, from $4^h 47^m$ to $6^h 07^m$. Great-circle path of meteor passes within 1°.8 of Orionid radiant, if this is assumed to be at $\alpha = 94°.2$, $\delta = +15°.0$ The convergence quantity C is completely indeterminate. The quantities v and i were calculated from an adopted radiant on the great-circle path nearest to the radiant assumed above.

Meteor 756. Also photographed on plate AI 34070 The trail was too faint for measurement of the breaks produced by the rotating shutter, and the meteor too faint to be photographed by the FA camera in Cambridge. The total range in possible exposure time is only 7^m.

Pertinent data concerning the five meteors are given in Table I. The length of the trail is from beginning to end, as seen on the plate, while the number of vibrations and the length of time refer to that part of the trail used in the calculation of B and C, which

[4] P. M. Millman and Miss Hoffleit, *H. A.*, **105**, 601, 1938.

[5] D. Hoffleit, *H. A.*, **82**, 179, 1937.

were determined by a least squares solution of the equation:

$$x = A + Bt + Ct^2,$$

where t is the time in seconds and x is the running coordinate along the trail measured at successive crests and troughs. Since C measures the apparent variation with time of the length of the oscillations, the relative accuracy of C is a direct measure of the accuracy with which the radiant can be determined. If C is uncertain, a solution for radiant, height, and velocity is not profitable; if the radiant is known, however, a solution for height and velocity can be made.

The apparent magnitudes were determined in the manner described before.[2] The linear velocity of the meteor is given by the product $R_m v$ where R_m is the meteor's distance at maximum light. The "visual intensity" is given by $R^2{}_m i$. The direction cosines of the poles of the apparent trails are given in the last three lines of Table I.

No further reductions were made for meteors 445 and 534 because of the indeterminacy of C and because 445 did not appear to be a regular member of the shower (see notes to Table I). For meteor 467 the radiant was poorly determined from C; consequently a solution was made by assuming Maltzev's [6] radiant for the Geminids on the date in question. The equinox of his coordinates was taken to be 1928.0. As the trail passed 0°.7 from the predicted position, a perpendicular was dropped to the trail and the point of intersection adopted as the radiant. The radiant obtained by means of C differed from the adopted radiant by about 1°.

For the three meteors 467, 505, and 756 the results of more extended reductions are given in Table II. The solution for the Geminid (No. 467) was practically independent of the time of apparition so that a mean solution is given; solutions for two sidereal times are given for meteor 505. The local sidereal times in Table II refer to Cambridge for meteors 467 and 505, and to Oak Ridge for meteor 756. The distance at maximum light is referred to the camera. The density of the atmosphere, ρ_m, the distance, height above sea level, and velocity at maximum light were determined by the methods described above. The elongation of the radiant is measured from the apex of the earth's motion,

[6] Maltzev, *Russ. A. J.*, 8, 67, 1931.

TABLE II

	RH 3735	RH 4653	RH 4653	RL 1922
Meteor number	467	505	505	756
Local sidereal time	$7^h 25^m$	$23^h 0^m$	$0^h 30^m$	$17h 43^m.5$
U. T.	1932 Dec. 15.28	1932 Nov. 4.0363	1932 Nov. 4 0987	1938 July 30.1663
Distance at max. light (km)	84.9	137.5	97.0	86.2
$\log \rho_m$	−7.46	−6.37	−6.03	−7.23
Height at beginning (km)	96.5	86.5	71.9	98.4
Height at end (km)	75.8	42.5	41.6	74.0
Height at max. light (km)	83.3	61.2	54.5	86.2
Apparent radiant α (1900.0)	115°.0*	351°.1	351°.1	267°.1
δ (1900.0)	+32.1*	+15.1	+15.1	+42.5
Elongation	64°.3	131°.4	131°.4	95°.0
$\cos Z_R$	0.984	0.885	0.860	1.000
V_0 apparent rel. vel. (km/sec)	38.4	18.43	13.00	27.92
Obs. vis. mag. (max. light)	−1.62	−3.91	−3.16	+0.40

* Assumed 1928.0.

and Z_R is the zenith distance of the radiant. The apparent relative velocity is uncorrected for the effect of the earth's rotation.

The calculated apparent velocity of 38.4 km/sec for the Geminid meteor is in excellent agreement with the value of 36.6 km/sec previously determined.[2] The present method of reduction is shown, therefore, to be as reliable as expected (p.e. = 12 per cent approximately).

Orbital elements have been calculated for the two sporadic meteors 505 and 756 (see Table III; notation standard). Although

TABLE III

Meteor number	505	505	756
Local sidereal time	$23^h 0^m$	$0^h 30^m$	$17^h 43^m.5$
V_G Geocentric vel. (km/sec)	14.7	6.9	25.6
V_H Heliocentric vel. (km/sec)	41.5	35.7	40.8
1/a (a.u.)$^{-1}$	0.069	0.579	0.090
q (a.u.)	0.927	0.963	0.951
e	0.936	0.442	0.914
P (years)	55	2.3	37
Mean anomaly	359°.6	351°.4	353°.5
ω (1900.0)	210.3	204.4	210.2
Ω (1900.0)	220.9	220.9	97.2
i (1900.0)	5.3	2.2	36.3

the velocity and period for meteor 505 are uncertain, the perihelion distance, argument of perihelion, longitude of the ascending node, and inclination are all well determined. This meteor orbit

represents clearly another case of direct motion with a small inclination to the ecliptic. By analogy with previous results [2] one might expect the short-period orbit to be more nearly correct. The orbit of meteor 756 is cometary in nature, perhaps more typical of the orbits of the short-period comets.

3. Physical Results.—The above calculations for meteor 505 (RH 4653) were made for the early part of the trail before the division into two parts. On the basis of the velocity and height before the division it is possible from the data of Fig. 1 to calculate the velocities, decelerations, and heights of the two components after their separation. The results are given in Table IV

TABLE IV

METEOR 505 (RH 4653)

Sidereal time:	$23^h\ 0^m$		$0^h\ 30^m$	
Component:	Bright	Faint	Bright	Faint
Apparent rel. velocity (km sec^{-1})	13.6	13.4	9.6	9.5
p.e.	±4.1	±2.1	±2.9	±1.5
Deceleration (km sec^{-2})	2.4	9.4	1.7	6.6
p.e.	±1.1	±0.6	±0.8	±0.4
Abs. vis. mag.	−2.36	−1.94	−1.61	−1.19
log ρ (calculated)	−6.05	−5.68	−5.75	−5.38
Height (km)	47.0	47.0	44.8	44.8
log ρ (assumed)	−5.72	−5.72	−5.61	−5.61
Δ log ρ	−0.33	+0.04	−0.14	+0.23
Mass (gm)	43	8	122	22

for the two sidereal times for which the calculations were made. The results apply to the middle of the component trails, over the distance in which the camera oscillations are distinct in Fig. 3c. The density of the atmosphere has been calculated at the corresponding points from the velocities, decelerations, and absolute magnitudes (log ρ calculated). The assumed values of log ρ were obtained from the "adopted" density curve [2] after corrections were made for systematic and seasonal effects, as has likewise been done in Table II. The quantity $\Delta \log \rho$ in Table IV is $\log \rho$ (calculated) minus log ρ (assumed).

The satisfactory agreement between the calculated and assumed values of log ρ demonstrates the internal consistency of the theory and of the assumptions involved. The height of the early part of the trail was established from the maximum-light considerations involving the density-velocity-magnitude relations,

while the density was calculated at a much lower level from the velocity-deceleration-magnitude relations. We note also that the fainter component of the meteor acquired a smaller velocity and a much larger deceleration than the brighter component, in qualitative agreement with the theory.

The apparent angle between the two components of the trail is 20′.1, corresponding to a minimum relative velocity of the two masses approximating 80 meters per sec (mean for the two sidereal times). No estimate could be made of the components of the velocity along the length of the trail or along the line of sight. By inspection of Fig. 1, the maximum time in which the relative velocity may have been acquired is approximately the interval corresponding to the distance between the intersection of the two component trails and the point where the trails could be separated on the plate, about $0^s.15$. Thus the minimum relative acceleration of the two masses is approximately 0.5 km sec^{-2}. The total deceleration of the whole meteor to be expected at this time would be slightly larger, of the order of 1 km sec^{-2}.

The mass of the meteoroid at the time of maximum light was about 250 gm. We have the problem of how this mass separated or broke into two main fragments whose combined mass was of the order of 100 gm slightly more than a second later. The general loss of mass, of course, came about by evaporation and probably somewhat by fragmentation. Our problem is to account for the main separation or breakage.

Before discussing the process of splitting that appears to be the most probable—breakage from internal stress produced by the thermal expansion of the heated exterior—it is advisable to discuss certain of the other possible processes. The most obvious explanation of apparent splitting might be that the meteoroid was originally double, the two components rotating very slowly if at all. Separation would then occur by the differential pressure of the atmosphere on their irregular surfaces. The main objection to this possibility is that since the various parts of the trail appear to be nearly straight (after correction for camera vibration) except in the interval of confusion near the break, the relative acceleration appears to have occurred in a short period of time, perhaps very much less than the $0^s.15$ assumed above. It would be very surprising if a component of acceleration perpendicular to the direction of motion acted for only a small fraction of the total lifetime of the meteor and was inactive during the remainder.

Also the *minimum* value of the acceleration perpendicular to the trail is of the same order as the deceleration of the entire meteor by the atmosphere. It is unlikely, but just possible, that this might occur; it is improbable that the double effect, a very great acceleration perpendicular to the trail acting in opposite directions for the two components for a very small fraction of the trail, should occur. If this were the case, however, a similar effect should be observed occasionally for single meteors whose trails do not split. We should expect to find irregularly crooked meteor trails.

The observations of the straightness of meteor trails are hardly sufficient to decide this point finally. H. A. Newton's [7] measures of seven points on the trail of a fireball indicated deviations from great-circle motion of about 15″. Curvature of the trail was indicated but the deviations were of the same order as the errors of measurement. Fisher and Dimitroff [8] found deviations of the order of one minute of arc from great-circle motion for a bright meteor on a Harvard plate. Relatively great curvature (2′ in 3°) was observed in one part of the trail. For two other photographic trails Fisher, Miss Wurl, and Miss Desmond [9] found similar deviations from great-circle motion but the apparent splitting of one trail, that on plate AM 6090, is probably only an optical effect caused by the astigmatisms of the lens near the edge of the plate. The surprising observation that both components of the divided trail varied in light synchronously is thus explained.

For the split meteor of Dec. 14, 1933, Miss Hoffleit [10] has compiled the measures made by the late Dr. W. J. Fisher. The path was straight for sixteen or seventeen degrees and the trail then diverged before it was apparent that the meteor had split. The divergence of the principal trail from the original direction was about 159″, of the fainter component, 282″. Dr. Fisher had no success in identifying the spindles in the trail as photographed from the Cambridge and Oak Ridge stations. By inspection of plate RL 117 (Oak Ridge, not used by Fisher) the author has been able to identify positively the spindles with those on plate AC 31639 (Cambridge).

The author has found that deviations from linearity are usually small, not much greater than the measuring error (5″ to 15″ for

[7] H. A. Newton, *Am. J. of Sci.*, **146**, 161, 1893.
[8] W. J. Fisher and G. Z. Dimitroff, *H. B.* 877, 1930.
[9] W. J. Fisher, E. L. Wurl and M. S. Desmond, *P. N. A. S.*, **13**, 540, 1927.
[10] D. Hoffleit, *H. B.* 901, 1935.

the small scale cameras), except for trails that split. A careful study of this problem will be made when the reductions now under way are completed. The evidence thus far, although meager, indicates that atmospheric pressure may cause measurable deviations from linearity in ordinary meteor trails but that the deviations observed among the split trails are generally greater. The effects of random motions of the cameras are difficult to predict and might occur for almost any camera, with the result that a meteor trail would be slightly curved or distorted. It is unlikely that atmospheric pressure alone can be responsible for the separation of the two components of a meteoroid.

The argument above, if accepted, rules out all processes of splitting among slowly rotating meteoroids where the energy for the eventual separation of the component masses is not inherently provided. Three such possibilities are: first, double or multiple meteoroids that are attached together by light bonds that break under the stresses of atmospheric pressure; second, very irregularly shaped meteoroids that become multiple because of surface vaporization in the atmosphere (as an irregular cake of ice may melt into two or more fragments); third, previously cracked or fractured meteoroids that simply come apart under stress.

In the preceding paragraphs it has been assumed that meteoroids have no appreciable energy of rotation. Öpik,[11] however, on the assumption of collisions from small hyperbolic meteoroids in space, has calculated that solar system meteoroids should have surface velocities of rotation amounting to several meters per second. Such a rapidly rotating meteoroid, broken by any of the several possible processes, might fly apart to produce a split trail of the type under consideration. The order-of-magnitude agreement between the observed and calculated velocities of separation is not unfavorable to the hypothesis of rotation.

Rapid rotation of a meteoroid might produce observable effects in its trail other than the short-period fluctuations of brightness discussed by Öpik. The maintenance of an axis of rotation in an irregular meteoroid might cause considerable deviations from linearity in the trail by producing a systematic component of atmospheric pressure perpendicular to the direction of motion. This effect might account for the interesting fact that the bright component of meteor 505 deviates more from the original direction of motion than does the fainter and presumably less massive com-

[11] E. Öpik, *Publ. Obs Tartu*, 28, No. 6, 1936.

ponent. Meteoroids with their axes of rotation nearly in the direction of motion might tend to exhibit deviations from linearity in their trails while those with the axes perpendicular to the direction of motion might tend to exhibit short-period fluctuations in brightness. The light curve of meteor 505 is quite smooth, but the trail is generally hazy and broad.

If flares (spindles) in a meteor trail arise from the spilling of accumulated molten material from the meteoroid, then flares should occur rarely for rapidly rotating meteoroids. From her study of meteor trails in the Harvard plate collection, Miss Hoffleit [12] found that about 40 per cent of the shower-meteor trails have spindles. Either the assumption concerning the cause of flares is wrong or else rapid rotation is not a general property of solar-system meteoroids.

The above argument applies whether the rotation exists before the meteoroid enters the earth's atmosphere or whether rotation is produced by atmospheric resistance, as suggested by Lindemann and Dobson.[13] According to their theory double meteors are only apparently double, the optical result of a cone of fine spray or droplets of meteoric liquid being whirled away from a rapidly rotating meteoroid. It is clear that such a phenomenon cannot account for the photographic double and multiple trails, but it is difficult to be certain concerning the effect of atmospheric resistance in producing or retarding the rotation of a meteoroid.

If flares are the result of rapid melting and spraying of meteoric material the argument of the preceding two paragraphs becomes invalid. A careful study of all the meteor trails on Harvard plates is being undertaken by the author and should help to clear up various of these uncertainties.

Whether rapid rotation is present or not, the rapid thermal expansion of the surface of a meteoroid in the earth's atmosphere will probably produce breakage, at least for a stone meteoroid. Mr. David Griggs and Dr. E. B. Dane, Jr., have conducted experiments in which quartzite pebbles were dropped into molten lead. When the temperature of the lead was above the inversion temperature (573°) the 2 per cent volume-increase of the outer layers of the pebbles produced breakage, often into two nearly equal pieces. Although the physical properties of stony meteoroids are not so especially sensitive to temperature as are those of the

[12] D. Hoffleit, *P. N. A. S.*, **19**, 212, 1933.
[13] F. A. Lindemann and G. M. B. Dobson, *Proc. Roy. Soc.*, A, **102**, 411, 1923.

quartzite pebbles, nevertheless the temperatures in meteoric phenomena are much higher, and meteoric material is far from homogeneous. Breakage of stony meteoroids should, therefore, occur occasionally. (It is important to note that in the quartzite experiment the surface of the pebbles did not tend to peel but that the pebbles broke into relatively large pieces.)

Although breakage of the type here discussed occurs in a somewhat explosive manner, it is difficult to estimate the velocities with which the pieces should separate. In the quartzite experiment the breakage was not enough to splash the molten lead violently. Mr. Griggs and Dr. Dane, however, have demonstrated that when ordinary stones are crushed by external pressure, pieces are ejected with velocities of the order of 15 meters per second, roughly comparable with the relative velocity attained by the two components of meteor 505. More experiments of the types mentioned above are planned for the near future. Experimentation on meteoric material should demonstrate finally whether the breakage of meteoroids actually occurs as the result of thermal expansion of the surface.

The author wishes to thank Messrs. Griggs and Dane for their kindness in allowing him to discuss their experiments in advance of publication.

MASSES, RADII, AND OTHER ABSOLUTE DIMENSIONS FOR 224 ECLIPSING VARIABLES

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(Communicated by Harlow Shapley)

ABSTRACT

The eclipsing variables are of extreme importance for an understanding of stars in general. During the last twenty-five years the material relating to these variables has grown to such an extent that we may study their properties from a limited point of view, and also may view them broadly as a whole. Both of these aspects are treated in the present paper.

Two hundred and twenty-four systems are discussed. The principal results may be summarized as follows:

1. Absolute dimensions for 448 single components are derived by the method described in Section 3, and are given in Tables 3 and 4.

2. An expression is derived for the relation between mass and radius, which is given numerically in Table 6.

3. Other relations between the absolute dimensions are illustrated and discussed.

1. Introductory. The paramount importance of eclipsing binaries for our understanding of the stars in general, and the valuable results to be obtained from comparisons of the former with the latter, have long been recognized, but not fully utilized. The reason for this apparent neglect is that the accumulation of sporadic material *[1] during the past twenty-five years has changed very little, if at all, our conception of the properties of eclipsing stars, and consequently of stars in general, though the results for some exceptional systems like AO Cassiopeiae (established as an eclipsing binary by Pearce in Victoria) and ζ Aurigae began to foreshadow a new epoch. For example, the computation of absolute dimensions from relative elements was (and, regrettably, is still) based on the assumption that the mass of an eclipsing system is twice as large as that of the sun.

Let us, for the sake of clarity, assume that there are two observational groups of information about an eclipsing system. The first group may be called the *primary* characteristics: the mass, size, density, temperature (on the surface), and absolute luminosity (either per unit of surface or total, both expressed in magnitudes); in other words, the absolute dimensions.

*[1] Systematic attempts have been made at Washburn Observatory and at Berlin-Babelsberg to study spectroscopic binaries photoelectrically, in order to discover important eclipsing binaries.

The second group may be called the *secondary* effects: the ellipticity, apsidal motion, limb darkening, and reflection, besides some other smaller effects upon which it is too early to lay emphasis. It is of interest to note that in the last six or seven years the attention of astronomers has been confined almost wholly to the study of these secondary effects. Indeed after it was realized that the ellipticity and apsidal motion could give us information on the density distribution inside a star, very intensive work (judging from the excitement it has provoked) has been carried out at many observatories. But though without doubt the eclipsing binaries are suitable for such studies, the observed material is too incomplete at present to give us definite results upon the problem.

On the other hand, the first-group characteristics seem to have received little attention. Accumulation of results on the masses and sizes of the different systems has been very slow because determinations both of the orbital velocities of the components and of a light curve are necessary. Formerly such rare stars as AO Cassiopeiae and ϵ Aurigae have simply been considered abnormal, and systems like Y Cygni have been regarded as the standard for spectral Class O.

Owing to the large collection of photographic plates at Harvard, the author has had an opportunity to substantiate, almost without being aware of it, the importance of such systems as AO Cassiopeiae (its twin is 29 (UW) Canis Majoris [1]), [*2] and ϵ Aurigae (its twin is VV Cephei [2]). This material led the writer first to the establishment of a correlation between mass, period, and spectrum for the heavier component, in a purely graphical manner; later, the correlation was developed in analytical form. We may anticipate one by-product of the results, namely, a realization of the importance of the elements derived from the light curve. On them, according to this method, depend the mass and the radius, not only on the basis of the inclination, as heretofore, but also independently of it. It is of interest to note that the same idea concerning the derivation of the absolute dimensions of eclipsing binaries, without determination of the orbital velocities of both components, has occurred independently to a number of

[1] S. Gaposchkin, *H. B.* 902, 1936.

[*2] A new photoelectric light curve, determined at the McDonald Observatory, confirms our results.[3]

[2] S. Gaposchkin, *H. C.* 421, 1937.

[3] G. Kuiper, *Ap. J.*, 88, 472, 1938.

workers, though with different emphasis, in several countries: in Czechoslovakia by Kopal,[4] in Italy by Colacevich,[5] in Java by Kreiken,[6] in Russia by Krat,[7] [*3] and by the writer in the United States.[8] Among the four authors mentioned above, Colacevich's work has the closest similarity to ours, though the details of the method are different. Without giving any analytical expression of the method, Kuiper, Struve, and Strömgren [9] have followed a similar procedure in deriving absolute dimensions for ϵ Aurigae.

2. The Purpose of the Study. Our purpose has been to derive the probable absolute dimensions for all eclipsing binaries for which the relative elements are known and the spectra observed. How far this goal can be attained on the basis of the observational material will be seen from later discussion. One point however should be discussed here. It concerns the degree to which the absolute dimensions, derived by this method, are "observational." If the effective temperature and the mass-luminosity relation are observational results, then the absolute dimensions based upon these functions are observational also, and the uncertainty of the results is only a matter of the observational accuracy. Usually one understands by the "observational absolute dimensions" of an eclipsing system those that are obtained on the basis of the orbital velocities of both components and the relative elements of the light curve. But how far is this statement true? In very close binaries—say, when the distance between the surfaces of the components is less than one-fifth of the distance between the centers—the reflection effect could produce a change in the computed mass of over ten per cent. For example, as Kuiper has shown for an extreme case,[10] the change in the values of the masses through reflection is about 20 per cent for the system of AO Cassiopeiae. In other words, what we usually call observational masses are a rough approximation. This fact was particularly realized by Pike and in more recent times by Krat. On the other hand, the absolute dimensions of ϵ Aurigae, derived on the same basis as is shown here, have been used by Chandrasekhar [11] as observational results.

[4] Z. Kopal, *Zs. f. Ap.*, 9, 235, 1935.
[5] Colacevich, *Mem. d. Soc. Astr. Ital.*, 11, 3, 1938.
[6] Kreiken, *A. N.*, 259, 349, 1936.
[7] W. Krat, *Publ. de l'Obs. Astr. Engelhard*, Kasan, No. 19, p. 81, 1937.
[*3] Krat and Kopal knew of each other's ideas from private communication.
[8] S. Gaposchkin, *Publ. A. A. S.*, 9, 121, 1938.
[9] Kuiper, Struve, and Strömgren, *Ap. J.*, 86, 570, 1937.
[10] G. Kuiper, *Ap. J.*, 88, 502, 1938.
[11] Chandrasekhar, *Introduction to the Study of Stellar Structure*, p. 317, Univ. of Chicago Press, 1938.

3. The Method. The idea underlying the procedure for deriving the absolute dimensions has already been discussed by the writer.[12] For the sake of completeness we summarize it here. The following four fundamental relations are used:

$$\mu_1 + \mu_2 = c_1 A^3 P^{-2} = \mu_1(1 + \alpha), \tag{1}$$

$$L = c_2 R^2 T^4, \tag{2}$$

$$L = c_3 \mu^{3.2}, \tag{3}$$

$$R = rA, \tag{4}$$

where μ_1 and μ_2 are the masses of the components; A is the distance between the centers; P, the period of revolution; L, the total luminosity of the star; R, its radius; T, its effective temperature, defined in the sense of the relation (2); r, the relative radius in units of the distance between the centers, and c_1, c_2, and c_3 are constants. The combination of the four above equations gives us the fundamental equation for our investigation:

$$\log \mu_1 = 0.263[1.871 + \log(1 + \alpha) + 2 \log P + 6 \log T + 3 \log r]. \tag{5}$$

We have chosen the more convenient solar units in our computations of μ, L, R, and T, and for P we have used days.

An examination of Equation (5) reveals our method. It should be mentioned first that of the four equations given above, (3) is not an unambiguous relation as are the others; it furnishes, however, the key to our method. We have adopted the exponent 3.2 for the mass-luminosity relation, in accordance with our earlier discussion.[13] In a private communication, Professor H. N. Russell [*4] points out that the theoretically appropriate expression for (3) is:

$$L = c_3 \mu^x R^y, \tag{3a}$$

[12] S. Gaposchkin, these PROCEEDINGS, 79, 327, 1938.

[13] S. Gaposchkin, these PROCEEDINGS, 79, 334, 1938.

[*4] Professor Russell permits us to quote the following statement· "Eddington's original theory gives $x = 5.5$; but this takes no account of the 'guillotine factor' and its variations. Bethe has recently shown that over the range of conditions covered by the lower part of the Main Sequence (below A0 or perhaps B5) a good approximation may be made by setting $x = +5.25$, $y = -1.25$. This gives:

$$3.85 \log \mu_1 = -\log c_1 + \log(1 + \alpha) + 2 \log P + 3 \log r + 3.69 \log T.$$

The main coefficient is very nearly equal to Gaposchkin's (0.260 instead of 0.263); but the coefficient of log T is 3.7 instead of 6.

"In our study of the masses of double stars, we have just finally derived the empirical relation for Main Sequence stars (assuming $y = -\frac{1}{2}$). . . . This corresponds to $x = 4.07$."

where c_3 depends largely on the hydrogen abundance. We have adopted $x = 3.2$, $y = 0$. The possible resulting changes in our final expression (Equation 5) are considered on page 300.

Professor Russell also writes that the results of an extensive study of the masses of visual binaries lead to a value of this coefficient (0.263), differing by only two per cent from the one which had been previously adopted here.

We give below some critical remarks, step by step, on the terms used in formula (5):

$(1 + \alpha)$. This factor has the least effect upon the computed mass of the heavier component; α cannot be greater than 1 and it must be larger than 0. The statistical mean value [14] of α is about 0.75; so that for general purposes, if we limited the investigation to the heavier component, that value of α could be safely used. In order, however, to be able to take the second component (where the difficulties always begin) into consideration also, we have derived an empirical relation [15] between the ratio of the masses and the ratio of the relative total luminosities of the components of thirty-five eclipsing systems:

$$\alpha = 0.20 + 1.6\ (1 - L_1). \tag{6}$$

The probable error of the factor 1.6 is 0.14.

A logarithmic relationship of the form $\alpha^{-1} = (L_b/L_f)^{2/5}$ has been used by Russell. For the sake of uniformity of treatment, however, we shall use our own relation. Russell has pointed out to us that this relation is not consistent with our adopted mass-luminosity relation, but the difference produced in the ratio between α and L is not greater than 25 per cent in any actual case.

To illustrate the size of the possible difference we tabulate the values of α for several values of L, derived from Equation (6) and from the mass-luminosity relation ($L = m^{3.2}$):

L	α (Equation 6)	α (Mass. Lum. Rel.) with $x = 3.2$
0.50	1.00	1.00
0.60	0.84	0 88
0.70	0.68	0.77
0.80	0.52	0.64
0.90	0.36	0.49
0.95	0.28	0.39
0.98	0.23	0.29
0.99	0.22	0.23
1.00	0.20	0.00

[14] Gaposchkin, *Variable Stars*, Harvard Observatory Monograph No. 5, p. 48, 1938.
[15] S. Gaposchkin, these PROCEEDINGS, 79, 333, 1938.

From the considerations mentioned above, it follows that the mass of the secondary component derived by this method is not of such accuracy as that derived for the first.

Because the relative brightness of the heavier component enters as argument for several functions, we give in Table 1 all the quan-

TABLE 1

L	α	log α	log $(1+\alpha)$	η	Δm	L	α	log α	log $(1+\alpha)$	η	Δm
0.50	1.00	−0.000	+0.301	1.00	0.00	0.90	0.36	−0.444	0.134	9.00	2.40
0.55	0.92	−0.036	0.283	1.22	0.22	0.95	0.28	−0.553	0.107	19.0	3.21
0.60	0.84	−0.076	0.265	1.50	0.44	0.96	0.26	−0.585	0.100	24.0	3.47
0.65	0.76	−0.119	0.246	1.86	0.68	0.97	0.25	−0.603	0.097	32.3	3.79
0.70	0.86	−0.168	0.225	2.33	0.92	0.98	0.23	−0.638	0.090	49.0	4.25
0.75	0.60	−0.222	0.204	3.00	1.20	0.99	0.22	−0.658	0.086	99.0	4.92
0.80	0.52	−0.284	0.182	4.00	1.50	0.995	0.21	−0.678	0.083	199.	7.51
0.85	0.44	−0.357	0.158	5.67	1.89						

tities that are necessary for our computations. Successive columns contain: the total luminosity, L; the ratio of masses deduced from Equation (6), α; the logarithm of the previous quantity; the logarithm of $(1 + \alpha)$; the ratio of the luminosities, η; and the difference in luminosity, Δm, expressed in magnitudes. As in all the tables given in this paper, square brackets denote logarithms to the base 10.

(*6 log T*). The effective temperature is of paramount importance for our method. It is subject to two sources of uncertainty, the first in importance being the spectral class itself. Different observatories use slightly different criteria for spectral classification, and hence have slightly different systems. Yet the notation for all spectral classifications (save for some terms in use by the Swedish school) remains the same as that adopted at Harvard. Hitherto, most classifications have been empirical estimates, based on not too well-coordinated measures, except for the type of work that is being carried on, for example, by Dr. W. W. Morgan at the Yerkes Observatory. The most serious difficulty is that of reducing one determination of spectral class to another system, in a manner analogous to the reduction of a determination of stellar magnitude to a known system. Although this result is in reality attainable, it has not been attained as yet.

In many problems of modern astrophysics, where the order of the results only is of importance, this deplorable state of spectral classification is not significant. But for the present investigation,

and for others similar to it, a difference of half a spectral class makes the drawing of conclusions hopeless. If at first for example a spectral class of A0 is assigned, and later (by a different investigator in a different observatory) the class B5 is assigned to an eclipsing star, or if at first M1, and then K6 is given as the spectrum, it becomes difficult to draw valid physical conclusions.

The second problem is that of finding the effective temperature that corresponds to a given spectral class. In this respect, however, the study of eclipsing stars is self-supporting: they are themselves the source of the scale of effective temperature that we shall use. And though we realize that the material on which the scale is based is neither complete nor thoroughly satisfactory, both disabilities will probably be removed in the future by the discovery of new eclipsing stars of known parallax. We believe, however, that the scale we have derived and used is not far from the true one.

While this work has been in progress a new discussion of the temperature scale has been published by Kuiper.[16] For most spectral classes his scale is close to ours, which is not surprising since he bases it on selected eclipsing stars; but for very early B and O stars, his temperature is systematically higher than ours, as in this portion he uses other sources than eclipsing stars. We shall use our own temperature scale in this investigation, as it is more consistent for our purpose.

The second auxiliary table contains several functions, with spectral class as argument. Successive columns give the spectral class; the temperature, T; the quantity ($6 \log T$); the bolometric correction; the relative visual surface brightness (with arbitrary zero), I_v; and the corresponding relative photographic surface brightness, I_p. The supergiant temperature scale is based on an unpublished discussion by C. Payne-Gaposchkin.

($3 \log r$). This function throws the importance of the light curve into glaring prominence. In Equation (5), r is the mean radius, so that for an elliptical system a correction of b/a should be added. For some eclipsing stars there are several solutions for the relative elements. Often these solutions differ by forty or fifty per cent, and to determine their real probable errors is a problem for the future. In grading the elements, it is not enough to know that one light curve is better than another. Success in deriving acceptable elements depends also on the ability of the computer

[16] Kuiper, *Ap. J.*, 88, 429, 1938.

to balance the light curve with the other information about the system.

After this discussion of the quantities in Equation (5), we summarize the course of our computations. We calculate:

(*a*) the mass of the heavier component (Equation 5),

(*b*) the bolometric magnitude *[5] of the heavier component, by the relation:

$$M_{b,1} = M\odot - 2.5 \times 3.2 \log \mu_1 = 8 \log \mu_1 \tag{7}$$

(*c*) the visual magnitude of the heavier component:

$$m_{v,1} = M_b + \text{bol. corr. (Table 2)} \tag{8}$$

TABLE 2

SPECTRUM, TEMPERATURE,* AND SURFACE BRIGHTNESS

Spectrum	Effective Temp.	6 log T	Bol Corr.	I_v	I_p	Spectrum	Effective Temp.	6 log T	Bol. Corr.	I_v	I_p
	°		m	m	m		°		m	m	m
O7	28,750	4.20	−2.10	0.0	0.0	A0	10,520	1.57	−0.35	2.2	2.8
O8	24,100	3.94	−1.92	0.1	0.1	A1	10,000	1.45	−0.30	2.3	2.9
O9	23,290	3.64	−1.77	0.2	0.3	A5	8,390	0.98	−0.12	2.9	3.6
B0	21,390	3.42	−1.60	0.5	0.6	F0	7,300	0.64	−0.01	3.4	4.2
B1	19,950	3.24	−1.47	0.6	0.8	F5	6,210	0.22	−0.00	4.2	5.1
B2	18,630	3.06	−1.34	0.7	1.0	G0	5,750	0.00	−0.02	4.6	5.6
B3	17,250	2.86	−1.21	1.0	1.3	G5	5,160	−0.28	−0.12	5.3	6.3
B4	16,040	2.68	−1.08	1.1	1.4	K0	4,650	−0.55	−0.30	5.9	7.0
B5	14,490	2.50	−0.95	1.3	1.6	K5	4,050	−0.91	−0.60	6.8	8.0
B6	13,970	2.31	−0.83	1.4	1.7	M0	3,890	−1.02	−1.05	7.2	8.5
B8	11,850	1.93	−0.58	1.7	2.2	M1	3,850	−1.06	−1.14	7.3	8.6
B9	11,210	1.75	−0.46	2.0	2.5	M2	3,735	−1.11	−1.23	7.4	8.7

* For the three supergiants: ζ Aurigae (K5); VV Cephei (M2); and W Crucis (G0), we have adopted respectively the temperatures 3,200°, 2,750°, and 4,850°, in accordance with unpublished calculations by C. Payne-Gaposchkin.

(*d*) the radius of the heavier component:

$$\log R_1 = \frac{1}{3}[\log \mu_1(1 + \alpha) + 3 \log r_1 + 2 \log P + 1.871] \tag{9}$$

(*e*) the density of the heavier component:

$$\log \rho_1 = \log \mu_1 - 3 \log R_1 \tag{10}$$

(*f*) the mass of the secondary component:

$$\log \mu_2 = \log \mu_1 + \log \alpha \tag{11}$$

*[5] The bolometric magnitude for the sun adopted here is 4.60, in accordance with the results of Kuiper. The old value, $4^m.85$, would make all our magnitudes $0^m.25$ fainter.

(*g*) its absolute magnitude in accordance with Equations (7) and (8)

(*h*) its radius:

$$\log R_2 = \log R_1 + \log K_{1,2}^{-1} \qquad (12)$$

(*i*) its density, in accordance with Equation (10).

It should be noted that in all computations we have used for r the mean radius, appropriately calculated for elliptical systems.

4. The Material. We have used for our purpose all eclipsing binaries for which relative elements have been computed and spectra are known—224 systems in all—*6 and in so large a number there is necessarily a great variety in the reliability of the fundamental data. The material has accordingly been divided into two grades.

In grading the elements, there are two distinct points to be considered. One concerns the accuracy of the observations themselves, and there is no need to reiterate at length that a photoelectric light curve is more reliable than a visual or photographic one, obtained under the same conditions. But there is another point: the certainty of the elements depends (provided that the magnitude scale is correct over the whole range) directly on the depth of the minima. For example, even though the light curve for ϵ Aurigae has been photoelectrically obtained, its relative elements may be indeterminate by more than 100 per cent. This uncertainty can only be reduced by considering other facts, wholly independent of the light curve. On the other hand, if there are two minima with depths not far from $0^m.75$, the elements are considerably reliable, even with a visual or photographic light curve. For our purpose, and probably for all purposes where absolute dimensions are in question, this second point is quite as important as the first.

The division of the material into several grades has proved to be very difficult and must always be to some extent a matter of opinion, in the absence, in most cases, of an objective numerical criterion.

First-grade elements have been determined for 76 per cent of the 224 systems; we have included all systems observed photo-

*6 With very few exceptions (see Remarks), all the observational data were taken from the book *Variable Stars*. In using the data, the values for r, γ, and k were made consistent, and some stars (for example, AO Cassiopeiae) have received a better grade after such adjustments. The computed spectra of secondaries are in some cases slightly altered.

electrically, without regard to the intrinsic determinacy of the relative elements.

Second-grade elements relate to 24 per cent of the material; the spectra are known, but the photometric elements are not of such good quality as those for the previous group. Elements of the second grade cannot be seriously wrong.

5. Results. The results are assembled in Tables 3 and 4, which are self-explanatory. Every computation has been carried to the third decimal of the logarithm, but two decimals only are given in the tables.

For each star, the first line in the tables contains the quantities calculated with the values $x = 3.2$, $y = 0$ (Equation 3a), which we regard as the observed values of these quantities. The second line contains the results calculated, at the suggestion of Professor Russell, by taking account of the temperature term: instead of $6 \log T$ in our Equation (5) we have used the value $4.8 \log T$, which is theoretically more appropriate.

The spectra of a few stars in Tables 3 and 4 have not previously been published. Most of these (which are tabulated below) were classified by Mrs. Mayall on Harvard plates. The colors of ZZ Cassiopeiae and SZ Herculis were given in Volume **89** of Harvard Annals, No. 10, by Miss Wright; the spectrum for AB Centauri is taken from the Potsdam Spektral Durchmusterung.

FK Aql	B9	DW Car	B5	DK Cyg	A5
SS Boo	G	AB Cen	G:	KR Cyg	A0
TU Cnc	A0	SS Cet	A0	TU Her	F5
CV Car	A3	RZ Com	F5	AQ Mon	A0
		SS Com	F5	W Vol	K-M

6. Discussion. Before conclusions can be drawn from our study, we must enquire first how accurate are the derived masses—the fundamental quantities in the present research—and, secondly, what will be the general nature of the conclusions possible from our data. The answer to the first question may be found in a previous section, where the method was discussed. It was there concluded that the reliability of the results is simply a matter of the reliability of the observational data. An additional test is provided by a numerical comparison between the masses derived from the orbital velocities and those obtained by the methods of the present paper. The results are given in Table 5, which is self-

TABLE 3

ABSOLUTE DIMENSIONS OF ECLIPSING STARS (FIRST CLASS)

Star	Period	First Component						Second Component					
		Abs. Bol. Mag.	Abs. Vis. Mag.	log Mass ☉	log Radius ☉	log Dens. ☉	Sp.	Abs. Bol. Mag.	Abs. Vis. Mag.	log Mass ☉	log Radius ☉	log Dens. ☉	Sp.
RT And	d 0.629	5.92 5.63	6.34 6 05	−0 16 −0.13	−0.03 −0.02	−0.07	K2	6.14 5.85	6.82 6.53	−0.19 −0.16	0.09 0.11	−0.47	(K7)
SY And	34.91	1.16 1.82	1.51 2.17	0.43 0.35	0.16 0.14	−0.06	A0	2.94 3.60	4.44 5.10	0.21 0.12	1.05 1.02	−2.94	(M5)
TT And	2.766	−0.18 0.48	0.17 0.83	0.60 0.52	0.43 0.40	−0.70	A	2.09 2.75	2.10 2.76	0.31 0.23	0.59 0.56	−1.45	(F7)
TW And	4.123	1.30 1.56	1.31 1.57	0.41 0.38	0.45 0.44	−0.93	F0	3.92 4.18	4.28 4.54	0.08 0.05	0.62 0.60	−1.76	(K1)
WZ And	0.696	1.92 2.33	2.04 2.45	0.34 0.28	0.21 0.19	−0.29	A5	4.06 4.47	4.08 4.49	0.07 0.02	0.32 0.30	−0.89	(G0)
AA And	0.935	1.10 1.51	1.22 1.63	0.44 0.39	0.37 0.36	−0.68	A5	2.34 2.75	2.35 2.76	0.28 0.23	0.37 0.36	−0.84	(F1)
AB And	0.332	5.33 5.21	5.45 5.33	−0.09 −0.08	−0.05 −0.05	+0.07	G5	5.43 5.31	5.55 5.43	−0.10 −0.09	−0.05 −0.05	+0.05	(G5)
AD And	0.987	1.62 1.88	1.63 1.89	0.37 0.34	0.38 0.37	−0.78	F	1.83 2.09	1.84 2.10	0.35 0.31	0.39 0.38	−0.83	(F1)
S Ant	0.648	1.67 2.01	1.72 2.06	0.37 0.32	0.32 0.30	−0.58	A8	2.77 3.11	2.82 3.16	0.23 0.19	0.20 0.19	−0.37	A8
RY Aqr	1.967	0.91 1.42	1.11 1.62	0.46 0.40	0.33 0.31	−0.53	A3	4.46 4.97	4.58 5.09	0.02 −0.05	0.40 0.38	−1.17	(G5)
FK Aql	2.651	−1.27 −0.54	−0.81 −0.08	0.73 0.64	0.59 0.56	−1.04	B9	2.59 3.32	2.71 3.44	0.25 0.16	0.71 0.68	−1.89	(G5)
OO Aql	0.507	4.45 4.33	4.57 4.45	0.02 0.03	0.35 0.35	−1.02	G5	5.54 5.42	5.73 5.61	−0.12 −0.10	0.23 0.24	−0.82	(G7)
σ Aql	1.950	−1.21 −0.42	−0.63 0.16	0.73 0.63	0.53 0.50	−0.87	B8	−0.80 −0.01	−0.22 0.57	0.68 0.58	0.53 0.50	−0.92	B8
RW Ara	4.368	0.37 1.03	0.72 1.38	0.53 0.44	0.32 0.30	−0.44	A0	2.99 3.65	3.25 3.91	0.20 0.12	0.85 0.82	−2.34	(G9)
UW Ara	3.297	−1.01 −0.35	−0.66 0.00	0.70 0.62	0.60 0.57	−1.10	A0	0.43 1.09	0.43 1.09	0.52 0.44	0.81 0.78	−1.90	(F3)
SX Aur	1.210	−3.54 −2.42	−2.46 −1.34	1.02 0.88	0.77 0.72	−1.30	B4	−2.10 −0.98	−1.27 −0.15	0.84 0.70	0.68 0.63	−1.19	(B6)
TT Aur	1.333	−3.39 −2.19	−2.18 −0.98	1.00 0.85	0.70 0.65	−1.09	B3	−2.20 −1.00	−0.99 0.21	0.85 0.70	0.65 0.60	−1.11	B3
AH Aur	0.494	4.38 4.41	4.39 4.42	0.03 0.02	0.06 0.06	−0.14	F8	4.59 4.62	4.65 4.68	0.00 −0.00	0.13 0.13	−0.38	(G2)
AM Aur	13.62	−2.80 −2.39	−2.68 −2.27	0.92 0.87	1.15 1.14	−2.54	A5	−1.02 −0.61	−0.94 −0.53	0.70 0.65	1.38 1.36	−3.42	(G3)
AR Aur	4.134	0.46 1.12	0.81 1.47	0.52 0.44	0.31 0.28	−0.40	A0	0.53 1.19	0.88 1.54	0.51 0.43	0.34 0.31	−0.52	A0
β Aur	3.960	−0.50 0.11	−0.20 0.41	0.64 0.56	0.54 0.51	−0.97	A1	−0.50 0.11	−0.20 0.41	0.64 0.56	0.54 0.51	−0.97	A1
ε Aur*	9883.	−7.58 −7.50	−7.58 −7.50	1.52 1.51	2.37 2.36	−5.59	F5	−7.47 −7.39	−5.97 −5.89	1.51: 1.50:	3.37 3.36	−8.60	>(M5)

* ε Aur.—From the temperature of the second component, computed by Kuiper, Struve, and Strömgren,[17] and also by the writer,[18] its spectral class must be much later.

[17] *Ap. J.*, 86, 575, 1937.

[18] S. Gaposchkin, these PROCEEDINGS, 79, 338, 1938.

TABLE 3—*Continued*

Star	Period	First Component						Second Component					
		Abs. Bol. Mag.	Abs. Vis. Mag.	log Mass ⊙	log Radius ⊙	log Dens. ⊙	Sp.	Abs. Bol. Mag.	Abs. Vis. Mag.	log Mass ⊙	log Radius ⊙	log Dens. ⊙	Sp.
ζ Aur	972.2	−4.66 −5.04	−3.06 −3.44	1.16 1.21	2.36 2.38	−5.93	K5	−2.25 −2.63	−1.79 −2.17	0.86 0.90	0.50 0.52	−0.64	B9
i Boo	0.268	5.61 5.56	5.67 5.62	−0.13 −0.12	−0.16 −0.16	+0.37	G2	5.61 5.56	5.67 5.62	−0.13 −0.12	−0.16 −0.16	+0.37	G2
Y Cam	3.305	−1.37 −0.71	−1.02 −0.36	0.75 0.66	0.67 0.64	−1.27	A	3.31 3.97	3.43 4.09	0.16 0.08	0.61 0.58	−1.67	(G5)
SS Cam	4.824	−0.26 −0.28	−0.24 −0.26	0.61 0.61	0.99 0.99	−2.37	G1	0.60 0.58	0.60 0.58	0.50 0.51	0.53 0.53	−1.09	F5
SV Cam	0.593	4.54 4.43	4.66 4.54	0.01 0.02	0.11 0.11	−0.31	G5	8.60 8.48	8.68 8.56	−0.50 −0.49	−0.10 −0.10	−0.19	G3
SZ Cam	2.698	−6.52 −4.99	−4.74 −3.21	1.39 1.20	1.01 0.95	−1.65	O9	−5.08 −2.55	−3.48 −1.95	1.21 1.02	0.80 0.74	−1.20	(B0)
S Cnc	9.485	−0.76 −0.10	−0.41 0.25	0.67 0.59	0.55 0.52	−0.98	A0	2.26 2.92	2.38 3.04	0.29 0.21	0.83 0.80	−2.21	G5
RU Cnc	10.17	3.21 3.23	3.22 3.24	0.17 0.17	0.27 0.26	−0.62	F9	5.10 5.12	6.42 6.44	0.06 0.06	0.64 0.64	−1.99	(M3)
RZ Cnc	21.64	−0.15 −0.38	+0.15 −0.08	0.59 0.62	1.13 1.14	−2.81	K0	1.29 1.06	1.83 1.60	0.41 0.44	1.45 1.46	−3.94	K4
RS CVn	4.798	3.30 3.42	3.30 3.42	0.16 0.15	0.16 0.16	−0.32	F4	4.54 4.66	4.77 4.89	0.01 −0.01	0.68 0.68	−2.05	G8
RV CVn	0.270	5.89 5.92	5.90 5.93	−0.16 −0.16	−0.28 −0.29	+0.69	F8	6.38 6.41	6.42 6.45	−0.22 −0.23	−0.28 −0.29	+0.63	(G1)
R CMa	1.136	2.52 2.82	2.55 2.85	0.26 0.22	0.18 0.16	−0.27	A9	6.59 6.89	6.95 7.25	−0.25 −0.29	0.17 0.16	−0.75	(K1)
UW CMa	4 393	−7.81 −6.28	−6.04 −4.51	1.55 1.36	1.27 1.20	−2.26	O9	−7.20 −5.67	−5.43 −3.90	1.47 1.28	1.19 1.13	−2.10	O9
RW Cap	3.392	−0.48 0.03	−0.28 0.23	0.64 0.57	0.61 0.59	−1.20	A3	1.66 2.19	1.82 2.33	0.37 0.30	0.61 0.59	−1.47	A4
SS Car	3.301	0.62 1.28	0.97 1.63	0.50 0.41	0.27 0.24	−0.32	A	0.84 1.50	1.19 1.85	0.47 0.39	0.31 0.28	−0.45	A
ST Car	0.902	0.63 1.29	0.98 1.64	0.50 0.41	0.27 0.24	−0.32	A0	3.73 4.39	3.73 4.39	0.11 0.03	0.37 0.34	−1.00	F6
CV Car	14.42	−0.57 −0.06	−0.37 0.14	0.65 0.58	0.63 0.61	−1.24	A3	3.02 3.53	3.50 4.01	0.38 0.31	1.15 1.13	−3.08	(K3)
DW Car	1.328	−2.40 −1.39	−1.45 −0.44	0.88 0.75	0.57 0.53	−0.83	B5	−2.22 −1.21	−1.27 −0.26	0.85 0.73	0.55 0.50	−0.79	(B5)
FP Car	176.0	−1.64 −1.23	−1.52 −1.11	0.78 0.73	0.92 0.90	−1.98	A5	0.44 0.85	0.92 1.33	0.52 0.47	1.16 1.14	−2.95	(K3)
RX Cas	32.32	−2.31 −2.36	−2.25 −2.30	0.86 0.87	1.42 1.42	−3.39	G0	−1.90 −1.95	−1.80 −1.85	0.81 0.82	1.42 1.42	−3.44	(G4)
RZ Cas	1.195	0.78 1.32	1.03 1.57	0.48 0.41	0.32 0.30	−0.49	A2	4.62 5.16	4 74 5.28	0.00 −0.07	0 34 0 32	−1.02	(G5)
SX Cas	36 57	−2.26 −1.88	−2.16 −1.78	0 86 0.81	1.08 1.06	−2.39	A6	−2.26 −1.88	−2 11 −1.73	0.86 0 81	1 41 1 39	−3 38	G6
TV Cas	1.813	−0.58 0.08	−0 23 0 43	0.65 0.56	0.51 0.49	−0 89	A0	2.11 2.77	2.46 3 12	0.31 0 23	0 56 0 53	−1.37	A0
TW Cas	2.857	−0.49 0.24	−0 03 0.70	0.64 0.54	0.43 0.40	−0.67	B9	0.00 0 73	0.25 0.98	0.58 0 48	0.43 0 40	−0 73	(A2)
TX Cas	2.927	−3.28 −2.62	−2 93 −2 27	0.98 0.90	1 08 1.05	−2.24	A0	−0 65 0 01	−0.45 0.21	0 66 0.57	0 79 0.76	−1 72	(F4)

TABLE 3—*Continued*

Star	Period	First Component						Second Component					
		Abs. Bol. Mag.	Abs. Vis. Mag.	log Mass ⊙	log Radius ⊙	log Dens. ⊙	Sp.	Abs. Bol. Mag.	Abs. Vis. Mag.	log Mass ⊙	log Radius ⊙	log Dens. ⊙	Sp.
XX Cas	3.067	−4.49 −3.37	−3.41 −2.29	1.14 1.00	0.93 0.88	−1.64	B4	−2.22 −1.10	−1.39 −0.27	0.85 0.71	0.77 0.72	−1.46	B6
YZ Cas	4.467	0.38 0.89	0.58 1.09	0.53 0.46	0.48 0.45	−0.90	A3	4.56 5.07	4.56 5.07	0.00 −0.06	0.16 0.14	−0.49	(F3)
AO Cas	3.523	−8.82 −7.25	−6.90 −5.33	1.68 1.48	1.39 1.32	−2.49	O8.5	−7.15 −5.58	−5.23 −3.66	1.47 1.27	1.21 1.14	−2.16	O8.5
SU Cen	5.354	0.74 0.82	0.74 0.82	0.48 0.47	0.71 0.70	−1.64	F5	4.81 4.89	5.27 5.35	−0.03 0.04	0.63 0.63	−1.92	(K2)
SV Cen	1.660	−2 01 −1.22	−1.43 −0.64	0.83 0.73	0.68 0.65	−1.21	B8	−0.06 0.73	0.19 0.99	0.58 0.48	0.60 0.57	−1.23	(A2)
SZ Cen	4.108	−1.98 −1.44	−1.73 −1.19	0.82 0.76	0.87 0.85	−1.80	A2	−1.46 −0.92	−1.21 −0.67	0.76 0.69	0.72 0.70	−1.41	(A2)
U Cep	2.493	−0.24 0.42	0.11 0.77	0.60 0.52	0.44 0.42	−0.73	A0	2.46 3.12	2.69 3.35	0.27 0.18	0.65 0.62	−1.69	G8
VV Cep	7430.	−9.16 −9.63	−6.76 −7.23	1.72 1.78	3.39 3.41	−8.46	M2	−6.86 −7.33	−6.40 −6.87	1.42 1.48	1.39 1.41	−2.76	B9
VW Cep	0.278	6.22 6.10	6.34 6.22	−0.20 −0.19	−0.23 −0.23	+0.49	G5	6.83 6.71	7.25 7.13	−0.28 −0.26	−0.12 −0.11	+0.08	(K2)
WY Cep	1.249	0.38 0.75	0.45 0.82	0.53 0.48	0.55 0.54	−1.13	A7	0.49 0.86	0.52 0.89	0.51 0.47	0.58 0.56	−1.21	(A9)
XZ Cep	5.096	−1.59 −0 58	−0.64 0.37	0.77 0.65	0.41 0.36	−0.44	B5	−1.49 −0.48	−1.24 −0.23	0.76 0.63	0.71 0.67	−1.36	(A2)
AH Cep	1.775	−5.26 −3.82	−3.66 −2.22	1.23 1.05	0.79 0.73	−1.15	B0	−4.66 −3.22	−3.06 −1.62	1.16 0.98	0.92 0.86	−1.60	B0
T Cir	3.298	−1.99 −1.26	−1.53 −0.80	0.82 0.73	0.76 0.73	−1.44	B9	0.28 1.01	0.29 1.02	0.54 0.45	0.85 0.82	−2.02	(F2)
RZ Com	0.339	4.70 4.78	4.70 4.78	−0.01 −0.02	−0.09 −0.09	+0.25	F5	4.70 4 78	4.70 4.78	−0.01 −0.02	−0.09 −0.09	+0.25	(F5)
SS Com	0.413	4.30 4.38	4.30 4.38	0.04 0.03	−0.01 −0.01	+0.05	F5	4.30 4.38	4.30 4 38	0.04 0.03	−0.01 −0.01	+0.05	(F5)
U CrB	3.452	−0.97 0.04	−0.02 0.99	0.70 0.57	0.28 0.24	−0.15	B5	0.93 1.94	1.39 2.40	0.46 0.33	0.87 0.82	−2.14	B9
α CrB	17.36	0.18 0.84	0.53 1.19	0.55 0.47	0.36 0.33	−0.53	A0	5.78 6.44	7.28 7.94	−0.15 −0.23	−0.19 −0.22	+0.43	(M5)
W Cru	198.5	−6.92 −6.92	−6.90 −6.90	1.44 1.44	2.47 2.47	−5.96	G0	−6.63 −6.63	−6.61 −6.61	1.40 1.40	2.09 2.09	−4.86	(G0)
Y Cyg	2.996	−5 35 −3.82	−3.58 −2.05	1.24 1.05	0.78 0.72	−1.09	O9	−5.35 −3.82	−3.58 −2.05	1.24 1.05	0.78 0.72	−1.09	O9
SW Cyg	4.573	−0.32 0.22	−0.07 0.47	0.62 0.55	0.54 0.52	−1.01	A2	3.43 3.97	3.79 4.33	0.15 0.08	0.73 0.71	−2.04	(K1)
WW Cyg	3.318	0.33 0.87	0.58 1.12	0.53 0.47	0.41 0.39	−0.70	A2	5.15 5.69	6.38 6.92	−0.07 −0.14	0.65 0.63	−2.01	(M2)
WZ Cyg	0.584	0 02 0.68	0.37 1.03	0.57 0.49	0.39 0.36	−0.61	A	2.16 2.82	2.23 2.89	0.31 0.22	0.29 0.26	−0.55	(A7)
CG Cyg	0.631	3.50 3.69	3.51 3.70	0.14 0.11	0.07 0.06	−0.07	F2	4.55 4.74	4.65 4.84	0.01 −0.02	0.17 0.17	−0.52	(G4)
DK Cyg	0.471	3.11 3.52	3.23 3.64	0.19 0.14	0.06 0.05	−0.01	A5	3.60 4.01	3.61 4.02	0.15 0.10	0.17 0.16	−0.37	(F0)
DL Cyg	4.830	−4.73 −3.53	−3.52 −2.32	1.17 1.02	0.91 0.86	−1.57	B3	−3.12 −1.92	−2.77 −1.57	0.96 0.81	0.96 0.91	−1.93	(A0)

TABLE 3—*Continued*

Star	Period	First Component						Second Component					
		Abs. Bol. Mag.	Abs. Vis. Mag.	log Mass ⊙	log Radius ⊙	log Dens. ⊙	Sp.	Abs. Bol. Mag.	Abs. Vis. Mag.	log Mass ⊙	log Radius ⊙	log Dens. ⊙	Sp.
GO Cyg†	0.718	−0.49	−0.03	0.64	0.44	−0.67	B9	3.50	3.85	0.11	0.20	−0.49	A0
		0.24	0.70	0.54	0.40			4.23	4.58	0.02	0.17		
KR Cyg	0.845	0.86	1.21	0.47	0.23	−0.21	A0	4.14	4.15	0.06	0.08	−0.19	(F1)
		1.52	1.87	0.38	0.20			4.80	4.81	0.03	0.06		
MR Cyg§	1.677	−0.20	0.15	0.60	0.60	−1.21	A0	2.52	2.87	0.32	0.70	−1.78	A0
		0.46	0.81	0.52	0.57			3.18	3.53	0.23	0.67		
W Del	4.806	−0.91	−0.56	0.69	0.58	−1.05	A	2.94	3.17	0.21	0.73	−1.98	(G8)
		−0.25	0.10	0.61	0.55			3.60	3.83	0.12	0.70		
RR Del	4.600	0.12	0.47	0.56	0.37	−0.56	A	1.22	1.26	0.42	0.72	−1.74	(G1)
		0.78	1.13	0.48	0.34			1.88	1.92	0.34	0.69		
RW Dor	0.285	8.58	9.63	−0.50	−0.41	+0.74	M0	9.68	11.18	−0.64	−0.41	+0.60	(M4)
		8.15	9.20	−0.44	−0.40			9.25	10.75	−0.58	−0.40		
Z Dra	1.357	1.50	1.62	0.39	0.29	−0.49	A5	5.58	5.84	−0.12	0.30	−1.03	(G9)
		1.91	2.03	0.34	0.28			5.99	6.25	−0.17	0.28		
SX Dra	5.169	1.82	1.89	0.35	0.27	−0.46	A7	4.67	5.21	−0.01	0.87	−2.62	(K4)
		2.19	2.26	0.30	0.25			5.04	5.58	−0.06	0.86		
TW Dra	2.807	0.88	0.98	0.46	0.44	−0.85	A6	4.24	4.66	0.04	0.62	−1.83	K2
		1.26	1.36	0.42	0.42			4.62	5.04	0.00	0.61		
S Equ	3.436	0.94	1.29	0.46	0.21	−0.17	A0	5.62	5.72	−0.13	0.25	−0.89	(G4)
		1.60	1.95	0.37	0.18			6.28	6.38	−0.21	0.23		
RW Gem	2.886	−0.65	−0.30	0.66	0.53	−0.92	A	2.05	2.09	0.32	0.66	−1.67	(G1)
		0.01	0.36	0.57	0.50			2.71	2.75	0.24	0.64		
RY Gem	9.301	−0.50	−0.25	0.64	0.58	−1.09	A2	2.19	2.55	0.30	0.92	−2.47	(K1)
		0.04	0.29	0.57	0.55			2.73	3.09	0.23	0.90		
YY Gem	0.814	6.81	7.50	−0.28	−0.13	+0.12	K6	7.10	7.79	−0.31	−0.18	+0.23	K6
		6.42	7.11	−0.23	−0.12			6.71	7.40	−0.26	−0.16		
AC Gem	9.961	−3.46	−3.11	1.01	1.09	−2.26	A0	−2.64	−2.63	0.90	1.28	−2.92	(F0)
		−2.80	−2.45	0.92	1.06			−1.98	−1.97	0.82	1.25		
AF Gem	1.244	−0.88	−0.53	0.68	0.57	−1.03	A0	0.90	1.00	0.46	0.52	−1.11	(A6)
		−0.22	0.13	0.60	0.54			1.56	1.66	0.38	0.50		
U Gru	1.880	1.01	1.13	0.45	0.39	−0.73	A5	5.19	5 42	−0.07	0.39	−1.25	(G8)
		1.42	1.54	0.40	0.38			5.60	5.82	−0.13	0.38		
Z Her	3.993	2.40	2.41	0.28	0.29	−0.59	F2	2.81	2.82	0.22	0.56	−1.44	F2
		2 59	2.60	0.25	0.28			3.00	3.01	0.20	0.55		
RX Her	1.779	0.09	0.44	0.56	0.38	−0.58	A0	0.09	0.44	0.56	0.38	−0.58	A0
		0.75	1.10	0.48	0.35			0.75	1.10	0.48	0.35		
SZ Her	0.818	0.60	0.95	0.50	0.30	−0.40	A0	2.87	2.88	0.22	0.32	−0.74	(F2)
		1.26	1.61	0.42	0.27			3.52	3.54	0.13	0.29		
TU Her	2.267	3.05	3.05	0.19	0.24	−0.54	F5	7.12	8.35	−0.32	0 39	−1.49	(M2)
		3.13	3.13	0.18	0.24			7.20	8.43	−0.33	0.39		
TX Her	2.060	1.22	1.47	0.42	0.23	−0.28	A2	2.08	2.33	0.32	0 23	−0.38	A2
		1.76	2.01	0.36	0.21			2.62	2.87	0.25	0.21		
AK Her	0.422	4.30	4 31	0.04	0.03	−0.06	F8	4.79	4.80	−0.02	−0.05	+0.13	(F8)
		4.33	4.34	0.03	0.03			4 82	4.83	−0.03	−0 05		
u Her	2.051	−4.76	−3.55	1.17	0.92	−1.59	B3	−4.54	−3.33	1 14	0 76	−1.15	B3
		−3.56	−2.35	1.02	0.88			−3 34	−2.13	0.99	0 72		

† GO Cyg.—The photometric relative elements used are unpublished values by Dr. N. Pierce, kindly placed at the disposal of the writer.

§ MR Cyg.—The elements are taken from the work of Nekrassova.[19]

[19] *N. N. V. S.*, **5**, 45, 1936.

TABLE 3—*Continued*

Star	Period	First Component						Second Component					
		Abs. Bol. Mag.	Abs. Vis. Mag.	log Mass $\odot$	log Radius $\odot$	log Dens. $\odot$	Sp.	Abs. Bol. Mag.	Abs. Vis. Mag.	log Mass $\odot$	log Radius $\odot$	log Dens. $\odot$	Sp.
RX Hya	2.282	0.53 1.07	0.78 1.32	0.51 0.45	0.37 0.35	−0.61	A2	4.60 5.14	4.90 5.44	0.00 −0.06	0.47 0.45	−1.42	(K0)
SX Hya	2.896	1.54 2.05	1.74 2.25	0.38 0.32	0.21 0.19	−0.24	A3	6.22 6.73	7.18 7.69	−0.20 −0.27	0.51 0.49	−1.73	(K9)
TT Hya	6.953	1.55 2.06	1.75 2.26	0.38 0.32	0.20 0.18	−0.23	A3	4.91 5.42	5.06 5.57	−0.04 −0.10	0.71 0.69	−2.18	G6
RT Lac	5.074	1.64 1.44	1.90 1.70	0.37 0.39	0.76 0.77	−1.91	G9	2.25 2.05	2.61 2.41	0.29 0.32	0.76 0.77	−1.98	K1
SS Lac	14.42	−1.08 −0.42	−0.73 −0.07	0.71 0.63	0.61 0.58	−1.13	A	0.11 0.77	0.46 1.12	0.56 0.48	0.45 0.42	−0.78	(A0)
SW Lac	0.321	5.25 5.18	5.33 5.26	−0.08 −0.07	−0.07 −0.07	+0.14	G3	5.46 5.39	5.54 5.47	−0.11 −0.10	−0.04 −0.04	+0.02	G3
VY Lac	1.036	0.86 1.40	1.11 1.65	0.47 0.40	0.31 0.28	−0.45	A2	4.13 4.67	4.14 4.68	0.06 0.01	0.31 0.28	−0.86	(F8)
AR Lac	1.983	4.81 4.58	5.11 4.88	−0.03 0.00	−0.14 0.15	−0.45	K0	6.25 6.02	6.37 6.14	−0.21 −0.18	0.35 0.36	−1.25	G5
CM Lac	1.605	1.66 2.20	1.91 2.45	0.37 0.30	0.15 0.12	−0.07	A2	2.39 2.93	2.44 2.98	0.28 0.21	0 24 0.22	−0.45	A8
CN Lac	0.637	4.42 4.35	4.50 4.43	0.02 0.03	0.09 0.10	−0.25	G3	8.69 8.62	10.01 9.94	−0 51 −0.50	0.04 0.04	−0.62	(M3)
Y Leo	1.686	−0.05 0.61	0.30 0.96	0.58 0.50	0.41 0.38	−0.64	A	4.02 4.68	4.14 4.80	0.07 −0.01	0.48 0.45	−1.36	(G5)
V Lep	1.070	0.17 0.58	0.29 0.70	0.55 0.50	0.56 0.54	−1.13	A5	2.44 2.85	2.45 2.86	0.27 0.22	0.42 0.40	−0.98	(F1)
δ Lib	2.327	−0.96 −0.30	−0.61 0.05	0.70 0.61	0.59 0.56	−1.07	A0	2.79 3.45	2.83 3.49	0.23 0.14	0.63 0.60	−1.65	(G1)
RV Lyr	3.599	0.12 0.78	0.47 1.13	0.56 0.48	0.37 0.35	−0.56	A	3.48 4.14	3.78 4.44	0.14 0.06	0.69 0.66	−1.94	(K1)
TT Lyr	5.244	−1.19 −0.53	−0.84 −0.18	0.72 0.64	0.64 0.61	−1.18	A	3.23 3.89	3.65 4.31	0.17 0.09	0.80 0.77	−2.24	(K2)
UZ Lyr	1.891	−0.02 0.52	0.23 0.77	0.58 0.51	0.48 0.46	−0.86	A2	4.66 5.20	5.02 5.56	−0.01 −0.08	0.48 0.46	−1.45	(K1)
β Lyr‡	12.91	−5.74 −4.95	−5.16 −4.37	1.29 1.19	1.43 1.39	−2.99	B8	−1.06 −0.27	−1.05 −0.26	0.71 0.61	1.16 1.13	−2.77	(F0)
RU Mon	3.585	0.05 0.71	0.40 1.06	0.57 0.49	0.39 0.36	−0.60	A	0.54 1.20	0.89 1.55	0.51 0.42	0.31 0.28	−0.43	(A0)

‡ β Lyr.—The photometric solution here adopted was obtained by the writer on the basis of Smart's photoelectric light curve.[20] It deviates considerably from that of Guthnick and Prager;[21] in our solution we have an almost annular eclipse during primary minimum, which removes practically all the difficulty of explaining the persistence of the B8 spectrum throughout the whole variation. New photometric results by Sandig[22] in 1934 brought to light no additional facts except the color observations. From his values one can easily deduce that the second component must be of spectral class later than A. Our previous determination,[23] combined with the present one, leads to a value of Class F.

[20] Smart, *M. N.*, 95, 648, 1935; Gaposchkin, *Publ., A. A. S.*, 9, 152, 1939.

[21] *Sitzungsber. Akademie Berlin*, p. 222, 1917.

[22] *Zs. f. Ap*, 8, 1, 1934.

[23] These PROCEEDINGS, 79, 340, 1938.

TABLE 3—*Continued*

Star	Period	First Component						Second Component					
		Abs. Bol. Mag.	Abs. Vis. Mag.	log Mass ⊙	log Radius ⊙	log Dens. ⊙	Sp.	Abs. Bol. Mag.	Abs. Vis. Mag.	log Mass ⊙	log Radius ⊙	log Dens. ⊙	Sp.
RW Mon	1.906	−0.06 0.60	0.29 0.95	0.58 0.50	0.41 0.38	−0.65	A	2.08 2.74	2.08 2.74	0.32 0.23	0.54 0.51	−1.31	(F6)
TU Mon	5.049	−2.62 −1.83	−2.04 −1.25	0.90 0.80	0.77 0.73	−1.40	B8	1.57 2.36	1.58 2.37	0.38 0.28	0.84 0.80	−2.13	(F9)
UX Mon	5.905	−0.75 −0.34	−0.63 −0.22	0.67 0.62	0.74 0.74	−1.56	A5	1.52 1.93	1.71 2.12	0.38 0.33	0.97 0.96	−2.53	G7
UY Mon	5.340	−0.32 −0.24	−0.32 −0.24	0.62 0.60	0.96 0.95	−2.26	F5	−0.32 −0.24	−0.32 −0.24	0.62 0.60	0.96 0.95	−2.26	(F5)
AQ Mon	2.546	2.32 2.98	2.67 3.33	0.28 0.20	−0.07 −0.09	0.49	A0	2.42 3.08	2.42 3.08	0.27 0.19	0.20 0.17	−0.32	(F3)
Z Nor	2.557	−2.42 −1.41	−1.47 −0.46	0.88 0.75	0.57 0.53	−0.83	B5	−1.90 −0.89	−1.64 −0.63	0.81 0.68	0.87 0.83	−1.80	(A4)
U Oph	1.667	−2.23 −1.22	−1.28 −0.27	0.85 0.73	0.53 0.49	−0.75	B5	−2.06 −1.05	−1.11 −0.10	0.83 0.70	0.53 0.49	−0.77	B5
RV Oph	3.687	0.24 0.90	0.59 1.25	0.54 0.46	0.35 0.32	−0.50	A0	2.72 3.38	2.76 3.42	0.24 0.15	0.56 0.53	−1.44	(G1)
RZ Oph	261.9	−1.91 −1.91	−1.89 −1.89	0.81 0.81	1.30 1.30	−3.09	G0	−1.74 −1.74	−1.13 −1.13	0.79 0.79	1.83 1.83	−4.68	K5
SW Oph	2.446	−0.28 0.38	0.07 0.73	0.61 0.53	0.45 0.42	−0.75	A0	3.27 3.93	3.39 4.05	0.17 0.08	0.68 0.65	−1.86	(G5)
SZ Oph	3.708	−0.26 0.40	0.09 0.75	0.61 0.52	0.45 0.42	−0.74	A	1.51 2.17	1.57 2.23	0.39 0.30	0.75 0.72	−1.87	(G2)
UU Oph	4.397	−1.07 −0.41	−0.72 −0.06	0.71 0.63	0.61 0.58	−1.13	A0	2.48 3.14	2.56 3.22	0.26 0.18	0.77 0.74	−2.04	(G3)
WZ Oph	4.183	4.35 4.35	4.37 4.37	0.03 0.03	0.05 0.05	−0.12	G0	4.42 4.42	4.44 4.44	0.02 0.02	0.06 0.06	−0.16	G0
VV Ori	1.485	−4.79 −3.50	−3.45 −2.16	1.17 1.01	0.86 0.80	−1.40	B2	−1.78 −0.49	−0.83 0.46	0.80 0.64	0.58 0.52	−0.93	(B5)
U Peg	0.375	3.45 3.60	3.45 3.60	0.14 0.12	0.11 0.10	−0.18	F3	4.06 4.21	4.07 4.22	0.07 0.05	0.11 0.10	−0.26	(F8)
AQ Peg	5.548	−0.33 0.21	−0.08 0.46	0.62 0.55	0.54 0.52	−1.01	A2	3.52 4.06	3.94 4.48	0.14 0.07	0.75 0.73	−2.12	(K2)
AT Peg	1.146	0.10 0.76	0.45 1.11	0.56 0.48	0.38 0.35	−0.57	A0	2.58 3.24	2.58 3.24	0.25 0.17	0.38 0.35	−0.88	(F3)
Z Per	3.056	0.22 0.88	0.57 1.23	0.55 0.46	0.35 0.32	−0.51	A	3.58 4.24	3.77 4.43	0.13 0.04	0.55 0.53	−1.54	(G7)
RT Per	0.849	3.18 3.37	3.19 3.38	0.18 0.15	0.13 0.12	−0.22	F2	5.87 6.06	6.06 6.25	−0.16 −0.18	0.13 0.12	−0.55	(G7)
RV Per	1.973	−0.93 −0.27	−0.58 0.08	0.69 0.61	0.58 0.56	−1.06	A	4.33 4.99	5.89 6.55	0.03 −0.05	0.73 0.70	−2.15	(M)
RY Per	6.864	−3.34 −2.37	−2.51 −1.54	0.99 0.87	0.82 0.78	−1.47	B6	−0.49 0.48	−0.49 0.48	0.64 0.51	0.96 0.92	−2.25	F8
ST Per	2.648	0.86 1.52	1.21 1.57	0.47 0.38	0.22 0.20	−0.21	A	3.95 4.61	4.25 4.91	0.08 −0.00	0.57 0.54	−1.64	(K0)
AB Per	7.160	−0.69 −0.43	−0.68 −0.42	0.66 0.63	0.84 0.83	−1.87	F0	−0.40 −0.14	−0.37 −0.11	0.62 0.59	1.01 1.00	−2.41	(F9)
AG Per	2.029	−3.42 −2.22	−2.21 −1.01	1.00 0.85	0.65 0.60	−0.95	B3	−1.92 −0.72	−0.71 0.49	0.82 0.66	0.50 0.44	−0.67	B3
β Per	2.867	−1.29 −0.50	−0.71 0.08	0.74 0.64	0.54 0.50	−0.87	B8	2.56 3.35	2.60 3.39	0.26 0.15	0.61 0.57	−1.62	(G1)

TABLE 3—*Continued*

Star	Period	First Component						Second Component					
		Abs. Bol. Mag.	Abs. Vis. Mag.	log Mass ⊙	log Radius ⊙	log Dens. ⊙	Sp.	Abs. Bol. Mag.	Abs. Vis. Mag.	log Mass ⊙	log Radius ⊙	log Dens. ⊙	Sp.
Y Psc	3.766	−0.05	0.20	0.58	0.49	−0.88	A2	5.06	6.20	−0.06	0.60	−1.86	(M1)
		0.49	0.74	0.51	0.46			5.60	6.74	−0.12	0.58		
V Pup**	1.455	−5.06	−3.59	1.21	0.85	−1.35	B1	−4.45	−3.24	1.13	0.81	−1.30	B3
		−3.70	−2.23	1.04	0.80			−3.09	−1.88	0.96	0.76		
TY Pup	0.581	3.26	3.27	0.17	0.05	+0.01	F0	3.26	3.27	0.17	0.05	+0.01	(F0)
		3.52	3.53	0.13	0.04			3.52	3.53	0.13	0.04		
XY Pup	13.78	−0.40	−0.20	0.62	0.60	−1.16	A3	2.62	3.10	0.25	0.94	−2.58	(K3)
		0.11	0.31	0.56	0.58			3.13	3.61	0.18	0.92		
U Sge	3.381	−1.34	−0.88	0.74	0.60	−1.07	B9	2.74	2.80	0.23	0.71	−1.91	G2
		−0.61	−0.15	0.65	0.57			3.47	3.53	0.14	0.68		
RS Sgr	2.416	−3.70	−2.75	1.04	0.83	−1.45	B5	−1.30	−1.05	0.74	0.73	−1.46	(A2)
		−2.69	−1.74	0.91	0.79			−0.29	−0.04	0.61	0.69		
SX Sgr	2.077	0.38	0.63	0.53	0.40	−0.67	A2	2.28	2.29	0.29	0.59	−1.47	(F8)
		0.92	1.17	0.45	0.37			2.82	2.83	0.22	0.56		
XY Sgr	2.023	−0.23	0.12	0.60	0.44	−0.73	A0	4.19	4.45	0.05	0.49	−1.42	(G9)
		0.43	0.78	0.52	0.42			4.85	5.11	−0.03	0.46		
YY Sgr	2.628	1.27	1.62	0.42	0.14	−0.01	A0	1.27	1.62	0.42	0.14	−0.01	(A0)
		1.93	2.28	0.33	0.11			1.93	2.28	0.33	0.11		
BQ Sgr	8.020	0.13	0.38	0.56	0.45	−0.80	A2	4.20	4.50	0.05	0.67	−1.97	(K0)
		0.67	0.92	0.49	0.43			4.74	5.04	−0.02	0.65		
DV Sgr	1.863	0.13	0.48	0.56	0.37	−0.56	A	1.18	1.19	0.43	0.56	−1.24	(F2)
		0.79	1.14	0.48	0.34			1.84	1.85	0.34	0.53		
V 356 Sgr	8.897	−3.56	−3.10	1.02	1.05	−2.12	B9	−3.07	−2.95	0.96	1.20	−2.63	A1
		−2.83	−2.37	0.93	1.02			−2.34	−2.22	0.87	1.17		
V 505 Sgr	1.183	0.46	0.71	0.52	0.38	−0.64	A2	4.89	5.12	−0.04	0.37	−1.14	(G8)
		1.00	1.25	0.45	0.36			5.43	5.66	−0.10	0.38		
μ^1 Sco	1.446	−3.58	−2.37	1.02	0.68	−1.02	B3	−3.17	−1.96	0.97	0.73	−1.21	B6
		−2.38	−1.17	0.87	0.63			−1.97	−0.76	0.82	0.68		
RT Scl	0.512	3.04	3.16	0.20	−0.02	+0.24	A5	3.04	3.04	0.20	0.23	−0.49	(F3)
		3.45	3.57	0.14	−0.03			3.45	3.45	0.14	0.21		
U Sct	0.995	1.62	1.63	0.37	0.38	−0.78	F0	5.17	5.40	−0.07	0.22	−0.72	(G3)
		1.88	1.89	0.34	0.37			5.43	5.66	−0.10	0.21		
W Sct	10.27	−2.09	−2.08	0.84	1.12	−2.54	F?	0.06	0.18	0.57	1.12	−2.81	(G5)
		−1.83	−1.82	0.80	1.11			0.32	0.44	0.54	1.11		
V Ser	3.452	−2.02	−1.44	0.83	0.68	−1.22	B8	−1.50	−1.30	0.76	0.78	−1.57	(A3)
		−1.23	−0.65	0.73	0.65			−0.71	−0.51	0.66	0.75		
RS Ser	0.598	3.14	3.14	0.18	0.30	−0.72	F5	3.74	3.74	0.11	0.23	−0.58	(F6)
		3.22	3.22	0.17	0.30			3.82	3.82	0.10	0.22		
RW Tau	2.769	0.48	0.83	0.52	0.30	−0.39	A0	4.90	5.25	−0.04	0.44	−1.37	(K1)
		1.14	1.49	0.43	0.27			5.56	5.91	−0.12	0.42		
SV Tau	2.167	−2.45	−1.99	0.76	0.63	−1.12	B9	0.50	0.85	0.51	0.71	−1.63	A
		−1.72	−1.26	0.66	0.60			1.23	1.58	0.42	0.68		
λ Tau	3.953	−4.61	−3.40	1.15	0.89	−1.52	B3	−1.75	−1.63	0.79	0.89	−1.88	(A5)
		−3.41	−2.20	1.00	0.84			−0.55	−0.43	0.64	0.84		
RV Tel	8.328	−1.32	−0.97	0.74	0.66	−1.24	A0	0.82	0.83	0.47	0.90	−2.22	(G3)
		−0.66	−0.31	0.66	0.63			1.48	1.49	0.39	0.87		
V Tri	0.585	1.86	2.06	0.34	0.14	−0.08	A3	4.95	4.95	−0.04	0.10	−0.34	(F6)
		2.37	2.57	0.28	0.12			5.46	5.46	−0.11	0.08		

** V Pup.—See Table 5 and footnote.

TABLE 3—*Continued*

Star	Period	First Component						Second Component					
		Abs. Bol. Mag.	Abs. Vis. Mag.	log Mass ⊙	log Radius ⊙	log Dens. ⊙	Sp.	Abs. Bol. Mag.	Abs. Vis. Mag.	log Mass ⊙	log Radius ⊙	log Dens. ⊙	Sp.
X Tri	0.972	1.37 1.88	1.57 2.08	0.40 0.34	0.24 0.22	−0.32	A3	4.46 4.97	4.58 5.09	0.02 −0.05	0.30 0.28	−0.89	(G4)
W UMa	0.334	4.89 4.92	4.90 4.93	−0.04 −0.04	−0.08 −0.09	+0.22	F8	4.89 4.92	4.90 4.93	−0.04 −0.04	−0.08 −0.09	+0.22	F8
RW UMa	7.328	3.67 3.69	3.68 3.70	0.12 0.11	0.17 0.17	−0.40	F9	4.28 4.30	4.54 4.56	0.04 0.04	0.63 0.63	−1.84	G9
TX UMa	3.063	−0.25 0.48	0.21 0.94	0.61 0.51	0.39 0.36	−0.56	B9	3.50 4.23	3.70 4.43	0.14 0.04	0.51 0.48	−1.39	A3
UX UMa	0.197	4.82 5.33	5.02 5.53	−0.03 −0.09	−0.45 −0.47	+1.33	A3	8.58 9.09	8.59 9.10	−0.50 −0.56	−0.50 −0.52	+0.99	(G3)
W UMi	1.701	−1.48 −0.82	−1.13 −0.47	0.76 0.68	0.69 0.67	−1.32	A0	2.27 2.93	2.27 2.93	0.29 0.21	0.56 0.53	−1 38	(F3)
UW Vir	1.811	1.13 1.67	1.38 1.92	0.43 0.37	0.25 0.23	−0.32	A2	5.95 6.49	6.37 6.91	−0.17 −0.24	0.39 0.37	−1.34	(K3)
AH Vir	0.408	5.98 5.75	6.28 6.05	−0.17 −0.14	−0.09 −0.08	+0.10	K0	6.26 6.03	6.56 6.33	−0.21 −0.18	−0.05 −0.04	−0.07	(K2)
Z Vul	2.455	−3.11 −1.91	−1.90 −0.70	0.96 0.81	0.59 0.54	−0.81	B3	−0.97 0.23	0.24 1.44	0.70 0.64	0.84 0.74	−1.83	B3
RR Vul	5.050	0.24 0.90	0.59 1.25	0.54 0.46	0.35 0.32	−0.50	A	1.58 2.24	1.60 2.26	0.38 0.29	0.67 0.64	−1.63	(G1)
RS Vul	4.478	−2.95 −2.16	−2.37 −1.58	0.94 0.84	0.87 0.83	−1.66	B8	1.12 1.91	1.58 2.37	0.44 0.34	0.72 0.68	−1.72	B9
B4520	12.00	−6.08 −4.64	−4.48 −3.04	1.34 1.16	1.00 0.94	−1.66	B0	−5.79 −4.35	−4.19 −2.90	1.30 1.12	0.96 0.90	−1.58	(B0)

explanatory. The observed values are taken from *Variable Stars* and from the writer's previous compilation,[25] except the value for μ^1 Scorpii, which is taken from the work of Kuiper;[26] the value for GO Cygni, from unpublished results by N. Pierce;[27] that for TX Ursae Majoris, from O'Keefe's study,[28] and that for Boss 4520 from the writer's.[29] The results are plotted in Fig. 1: evidently the agreement is good—even surprisingly so, considering the uncertainty of the underlying material, and the simplicity of the form adopted for the mass-luminosity relation. As Table 5 and Fig. 1 show, we have two values of the computed masses—those obtained from the simple observational mass-luminosity relation (6 [T]) and those obtained from the theoretically corrected temperature term (4.8 [T]). An inspection of the diagrams suggests that it would be difficult to say which shows a better agreement;

[25] S. Gaposchkin, these Proceedings, 79, 330, 1938.
[26] G. Kuiper, *Ap. J.*, 88, 505, 1938.
[27] Kindly communicated to the author.
[28] *H. B.* 908, 29, 1938.
[29] *Ap. J.*, 89, 125, 322, 1939.

TABLE 4

Absolute Dimensions of Eclipsing Stars (Second Class)

Star	Period	First Component					Second Component				
		Abs. Vis. Mag.	log Mass ⊙	log Radius ⊙	log Dens. ⊙	Sp.	Abs. Vis. Mag.	log Mass ⊙	log Radius ⊙	log Dens. ⊙	Sp.
WW And	23.28	−0.63 −0.22	0.67 0.62	0.74 0.72	−1.56	A5	0.11 0.52	0.56 0.51	0.87 0.85	−2.05	F3
AN And	3.220	−0.75 −0.38	0.68 0.63	0.80 0.78	−1.72	A7	1.52 1.89	0.39 0.34	0.44 0.42	−0.93	A
RS Ari	8.803	4.97 5.00	−0.05 −0.05	−0.09 −0.09	+0.21	F9	6.42 6.45	−0.21 −0.21	0.42 0.42	−1.49	G5
RZ Aur	3.011	−0.99 −0.33	0.74 0.66	0.66 0.63	−1.25	A	0.94 1.60	0.46 0.38	0.68 0.65	−1.57	(F2)
WW Aur	2.525	0.95 1.41	0.48 0.42	0.40 0.38	−0.72	A4	1.26 1.69	0.43 0.37	0 38 0.36	−0.70	A7
AK Aur	7.146	0.34 1.07	0.59 0.50	0.36 0.33	−0.50	B9	1.54 2.27	0.38 0.29	0.65 0.62	−1.56	(F5)
SS Boo	7.606	1.97 1.97	0.33 0.33	0.53 0.53	−1.27	G	6.53 6.53	−0 22 −0.22	0.31 0.31	−1.15	(K1)
X Car	0.541	0.12 0.78	0.60 0.52	0.44 0.41	−0.73	A0	0.17 0.83	0.59 0.51	0.44 0.41	−0.74	A1
AB Cas	1.367	2.23 2.74	0.32 0.26	0.11 0.09	−0.01	A3	7.07 7.58	−0.26 −0.32	0.18 0 16	−0.81	(K1)
AR Cas	6.066	−3.09 −1.89	1.11 0.96	0.83 0.78	−1.37	B3	0.87 2.07	0.51 0.36	0.37 0 32	−0.61	(A0)
RR Cen	0.606	2.28 2.47	0.29 0.27	0.31 0.30	−0.65	F2	6.47 6 66	−0.23 −0.25	−0.31 −0.32	+0.69	(F2)
RZ Cen	1.876	−4.00 −2.71	1.24 1.08	0.97 0.92	−1.66	B2	−1.52 −0.23	0.93 0.77	0 65 0.60	−1.02	(B2)
SS Cen	2.479	0.40 1.13	0.58 0.49	0.35 0.32	−0.46	B9	4.28 5.01	0.06 −0 03	0.54 0.51	−1.57	(G6)
SW Cen	5.219	0.70 1.36	0.53 0.45	0.33 0.30	−0.45	A0	3.74 4.40	0.12 0 04	0.68 0.65	−1.93	(G5)
SY Cen	6.631	−1.36 −0.70	0.79 0.71	0.74 0.71	−1.43	A	0.55 1.21	0.52 0.44	1.14 1.11	−2.89	(G5)
AB Cen	46.85	2.21 2.21	0.30 0.30	0.48 0.48	−1.15	G0	7.44 7.44	−0.17 −0.17	1.18 1.18	−3.71	(M5)
RS Cep	12.42	−0 36 0.30	0.66 0.58	0.54 0.51	−0.96	A	1.56 2.22	0.40 0.32	0.93 0.90	−2.38	(G7)
SS Cet	2.974	1.80 2.46	0.39 0.31	0.11 0.08	−0.08	A0	6.36 7.02	−0.16 −0.24	0.30 0.27	−1.06	(K3)
RW CrB	0.726	1.91 2.25	0.34 0.30	0.28 0.27	−0.50	A8	6.98 7.32	−0.26 −0.30	0.06 0.05	−0.43	(K0)
SY Cyg	6.006	0.45 1.11	0.56 0.48	0.38 0.35	−0.57	A?	3.79 4.45	0.15 0.07	0.74 0.71	−2.08	(K2)
UW Cyg	3.451	−0.31 0.35	0.66 0.58	0.53 0.50	−0.93	A?	2.66 3.32	0.25 0.17	0.60 0.57	−1.56	(G1)
UZ Cyg	31.31	−1.44 −0.93	0.78 0.72	0.84 0.82	−1.75	A3	1 20 1.71	0.47 0.41	1.14 1.12	−2.96	K1
VW Cyg	8.430	−0.72 −0.06	0.71 0.63	0.61 0.58	−1.13	A0	1.67 2.33	0.38 0.30	0.88 0.85	−2.26	(G5)
ZZ Cyg	0.629	1.33 1.99	0.45 0.37	0.20 0.17	−0.15	A?	4.91 5.57	−0.03 −0.11	0.23 0.20	−0.73	(G3)

TABLE 4—*Continued*

Star	Period	First Component: Abs. Vis. Mag.	log Mass ⊙	log Radius ⊙	log Dens. ⊙	Sp.	Second Component: Abs. Vis Mag.	log Mass ⊙	log Radius ⊙	log Dens. ⊙	Sp.
AE Cyg	0.969	1.36 1.77	0.42 0.37	0.35 0.33	−0.62	A5	1.25 1.66	0.42 0.37	0.47 0.45	−0.99	(F0)
RR Dra	2.831	2.17 2.58	0.32 0.27	0.18 0.16	−0.23	A5?	6.81 7.22	−0.19 −0.24	0.42 0.40	−1.45	(K6)
RX Dra	3.786	2.41 2.67	0.28 0.25	0.23 0.22	−0.41	F	3.23 3.49	0.17 0.14	0.12 0.11	−0.19	(F0)
RZ Dra	0.551	1.48 2.14	0.43 0.35	0.17 0.14	−0.08	A	4.78 5.44	−0.01 −0.09	0.29 0.26	−0.87	(G4)
RZ Eri	39.28	−1.20 −0.79	0.74 0.69	0.86 0.84	−1.83	A5	−0.22 0.19	0.60 0.55	0.98 0.96	−2.34	(F4)
AD Her	9.767	0.63 1.09	0.52 0.46	0.46 0.44	−0.86	A4	3.87 4.33	0.13 0.07	0.67 0.65	−1.87	K2
T LMi	3.020	−0.36 0.30	0.66 0.58	0.54 0.51	−0.96	A	4.23 4.89	0.08 0.00	0.57 0.54	−1.64	(G9)
Z Ori	5.203	−1.75 −1.09	0.84 0.76	0.82 0.79	−1.61	A	1.99 2.65	0.33 0.25	0.71 0.68	−1.79	F9
δ Ori	5.732	−5.65 −4.29	1.47 1.27	1.27 1.21	−2.33	B1	−4.00 −2.64	1.24 1.07	1.06 1.00	−1.93	(B2)
RW Per	13.20	−0.59 0.09	0.67 0.59	0.55 0.52	−0.97	A	2.65 3.31	0.28 0.20	0.87 0.84	−2.34	(K0)
RR Pup	6.430	−0.16 0.50	0.64 0.56	0.50 0.47	−0.86	A?	0.36 1.02	0.53 0.45	0.84 0.81	−1.98	(F9)
WX Sgr	2.129	1.33 1.99	0.45 0.37	0.20 0.17	−0.15	A0	3.27 3.93	0.17 0.09	0.25 0.22	−0.57	(F2)
WY Sgr	4.670	0.16 0.89	0.61 0.52	0.40 0.37	−0.58	B9	0.57 1.30	0.51 0.42	0.62 0.59	−1.36	(F0)
XZ Sgr	3.276	1.99 2.53	0.36 0.29	0.13 0.11	−0.03	A2	6.13 6.67	−0.12 −0.19	0.43 0.41	−1.41	(K4)
ZZ Sgr	3.083	−0.23 0.31	0.64 0.57	0.57 0.55	−1.09	A2	4.64 5.18	0.03 −0.04	0.59 0.57	−1.74	(K0)
BN Sgr	5.07	−0.39 −0.31	0.62 0.61	0.98 0.98	−2.30	F5	0.02 0.10	0.58 0.57	1.02 1.02	−2.49	(G1)
μ Sgr*	180.4	−3.64 −2.85	1.10 1.00	1.12 1.09	−2.27	B8	1.37 2.16	0.40 0.30	2.12 2.09	−5.97	(M)
FV Sco	5.728	−1.94 −1.21	0.88 0.79	0.82 0.79	−1.58	B9	0.33 1.06	0.59 0.50	0.71 0.68	−1.54	B9
RS Sct	0.664	3.21 3.47	0.18 0.15	0.07 0.06	−0.03	F	3.21 3.47	0.18 0.15	0.07 0.06	−0.03	(F0)
RY Sct	11.12	−5.47 −4.46	1.38 1.25	1.37 1.33	−2.74	B5	−5.11 −4.10	1.30 1.17	1.37 1.33	−2.81	(B7)
RZ Sct	15.19	−4.67 −3.38	1.33 1.17	1.10 1.05	−1.98	B2	−2.44 −1.15	0.88 0.72	1.40 1.35	−3.33	(G0)
RZ Tau	0.416	2.34 2.60	0.28 0.25	0.24 0.23	−0.44	F0	5.19 5.45	−0.07 −0.10	−0.06 −0.07	+0.11	F0
S Vel	5.934	−0.19 0.47	0.64 0.58	0.50 0.47	−0.87	A0	0.97 1.63	0.46 0.38	0.79 0.76	−1.91	(G1)
RR Vel	1.854	1.02 1.68	0.49 0.41	0.26 0.23	−0.30	A	2.63 3.29	0.25 0.17	0.33 0.30	−0.75	(F2)
AL Vel	96.11	−4.21 −3.70	1.13 1.07	1.40 1.38	−3.06	A3	−3.06 −2.55	0.96 0.90	1.92 1.90	−4.80	(G9)
W Vol	2.758	4.60	0.08	0.42	−1.20	K5	7.22	−0.16	0.45	−1.50	(M3)

* μ Sgr.—We have derived a tentative set of relative photometric elements on the basis of the light curve published by Morgan and Elvey.[24] Since the spectrum of the second component is not observed and the amplitude is very small the results should be considered provisional. We have therefore placed the star in the second group.

[24] *Ap. J.*, 88, 110, 1938.

TABLE 5

COMPARISON OF OBSERVED AND COMPUTED QUANTITIES

Star	Mass of Primary		Mass of Secondary		Spectrum of Secondary	
	Obs.	Comp.	Obs.	Comp.	Obs.	Comp.
AN And	..	..	..	..	A	A7
S Ant	−0.12	0.37 0.32	−0.38	0.23 0.19	A8	A9
σ Aql	0.79	0.73 0.63	0.71	0.68 0.58	B8	B9
RS Ari	..	..	..	..	G5	M
TT Aur	0.83	1.00 0.85	0.72	0.85 0.70	B3	B5
WW Aur	0.34	0.48 0.42	0.28	0 43 0.37	A7	A6
AR Aur	0.43	0.52 0.44	0.40	0.51 0.43	A0	A1
β Aur	0.38	0.64 0.56	0.37	0.64 0.56	A1	A1
ζ Aur	1.22	1.16 1.20	0.98	0.86 0.90	..	..
i Boo	−0.19	−0.13 −0.12	−0.19	−0.13 −0.12	G2	G2
SS Cam	..	..	..	..	F5	G
SV Cam	..	..	..	..	G3	K9
S Cnc	..	..	..	..	G5	G8
RZ Cnc	..	..	..	..	K4	>M
RS CVn	0.27	0.16 0.15	0.24	0.01 −0.01	G8*	>M
UW CMa	1.60	1.55 1.36	1.49	1.48 1.28	O9	O9
RW Cap	..	..	..	..	A4	F3
X Car	..	..	..	..	A1	A0
SS Car	..	..	..	..	A	A2
ST Car	..	..	..	..	F6	F8
SX Cas	..	..	..	..	G6	F8
TV Cas	0.26	0.55 0.56	0.00	0.31 0.29	A0	F4
XX Cas	..	.	..	..	B6	B8

TABLE 5—*Continued*

Star	Mass of Primary		Mass of Secondary		Spectrum of Secondary	
	Obs	Comp.	Obs.	Comp.	Obs.	Comp.
AO Cas	1.56	1.68 1.48	1.59	1.47 1.27	O8.5	O9
U Cep	..	..	..	..	G8*	G3
VV Cep	>1.65	1.72 1.78	>1.55	1.42 1.48	..	..
AH Cep	1.16	1.23 1.05	1.10	1.16 0.98	B0	B7
U CrB	0.72	0.70 0.57	0.30	0.46 0.33	B9	G7
Y Cyg	1.24	1.24 1.05	1.24	1.24 1.05	O9	O9
UZ Cyg	..	..	..	..	K1	K0
GO Cyg	0.15	0.64 0.54	0.08	0.11 0.02	A0	F2
MR Cyg	0.48	0.60 0.52	0.41	0.32 0.23	A0	A8
TW Dra	..	..	..	..	A2	K2
YY Gem	−0.20	−0.28 −0.23	−0.24	−0.31 −0.24	K6	K6
Z Her	0.20	0.28 0.25	0.11	0.22 0.20	F2	G7
RX Her	0.34	0.56 0.48	0.28	0.56 0.48	A0	A0
TX Her	0.32	0.42 0.36	0.25	0.32 0.25	A2	A5
AD Her	..	..	..	..	K2	K0
u Her	0.88	1.17 1.02	0.46	1.14 0.99	B3	<B3
TT Hya	..	..	..	..	G6	K8
RT Lac	0.70	0.37 0.40	0.00	0.29 0.32	K1	K1
SW Lac	.	..	..	..	G3	G6
AR Lac	0.15	−0.03 0.03	0.15	−0.21 −0.18	G5	>M
CM Lac	0.30	0.37 0.30	0.18	0.28 0.21	A8	A9

TABLE 5—*Continued*

Star	Mass of Primary		Mass of Secondary		Spectrum of Secondary	
	Obs.	Comp.	Obs	Comp.	Obs.	Comp.
UX Mon	..	..	..	..	G7	G6
U Oph	0.73	0.85 0.73	0.57	0.83 0.70	B5	B6
RZ Oph	..	..	..	..	K5	K1
WZ Oph	0.15	0.03 0.03	0.13	0.02 0.02	G0	G1
Z Ori	..	..	..	..	F7	F9
AG Per	0.72	1.00 0.85	0.66	0.82 0.66	B3	B5
V Pup	1.33	1.21 1.04	1.17†	1.13 0.96	B3	B2
U Sge	0.83	0.74 0.65	0.31	0.23 0.14	G2*	G4
V 356 Sgr	..	..	..	..	A1	A5
FV Sco	..	..	..	..	B9	A4
μ^1 Sco	1.09	1.02 0.87	1.09	0.97 0.82	B3?	B5
RZ Tau	..	..	..	..	F0	F1
SV Tau	..	..	..	..	A	F2
W UMa	−0.12	−0.04 −0.04	−0.27	−0.04 −0.04	F8	F8
RW UMa	..	..	..	..	G9	M
TX UMa	0.49	0.61 0.51	−0.03	0.14 0.04	A3*	G5
Z Vul	0.73	0.96 0.81	0.37	0.70 0.54	B3*	F2
RS Vul	0.72	0.94 0.84	0.22	0.44 0.34	B9*	F2
B 4520	>1.56	1.34 1.16	>1.56	1.30 1.12	..	..

* Spectra determined during totality.

† A new determination by H. van Gent (B. A. N., 8, 319, 1939). This new solution makes $k_{1,2} = 1.02$; $\gamma_{1,2} = 1.36$; $L_1 = 0.59$; $b/a = 0.83$; $c/a = 0.78$; $a_1 = 0.381$; $i = 76°.9$.

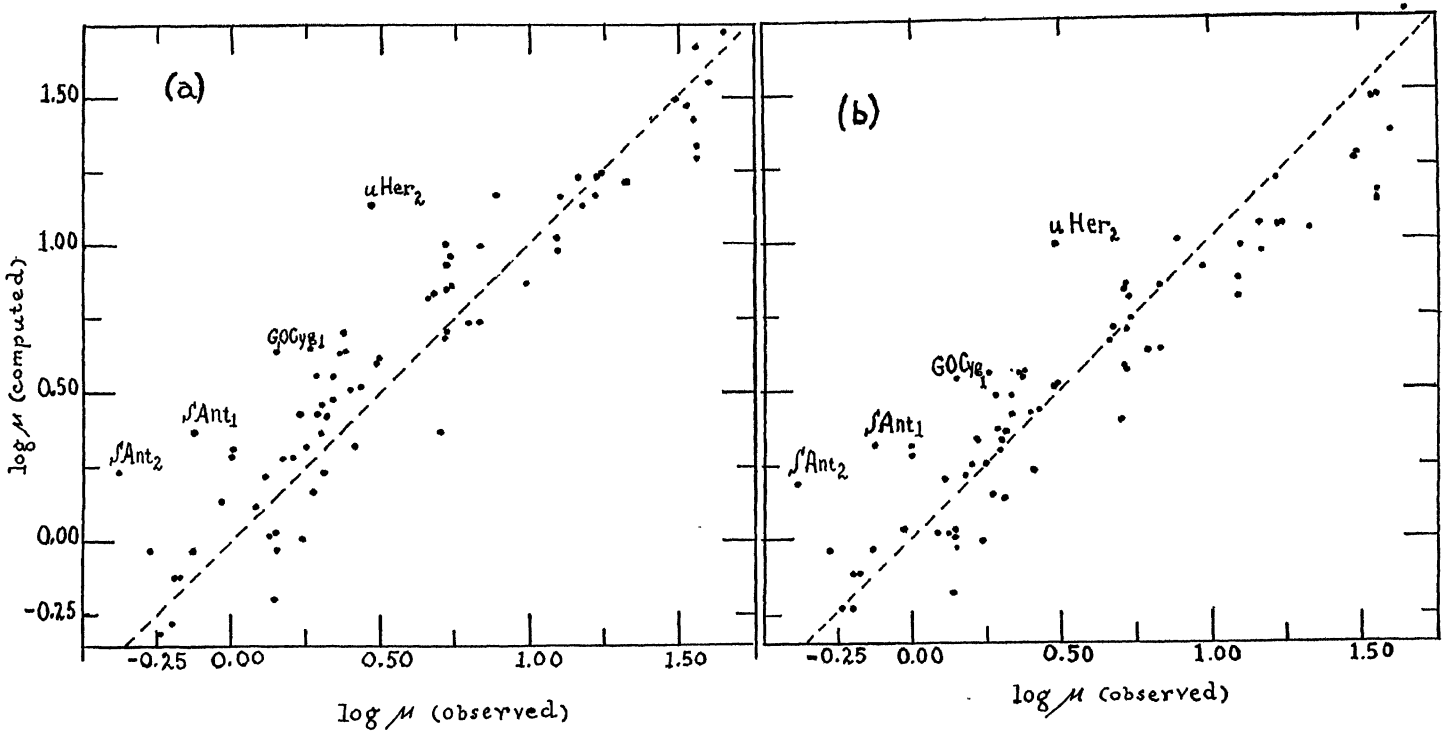

Fig. 1. Comparison of masses (see Table 5)—(*a*) for temperature term 6[T]; (*b*) for temperature term 4.8[T]. Abscissa: logarithm of the mass, based on the orbital velocities of both components and on the inclination of the orbit, deduced from the light curve. Ordinate: logarithm of the mass, based on the spectrum, light curve, and mass-luminosity relation (deduced in this paper).

the upper end of the masses is definitely better in Fig. 1*a*, but the lower and middle parts are better in Fig. 1*b*. If one computes the mean deviation of the values, then it is evident that the lower figure would show a better agreement in general, though the difference does not amount to more than 10 per cent. But there is one point which should be mentioned here, namely, the quantity of the material, particularly in respect to the spectra of the component. For example, one of the best observed systems is Y Cygni; for that star the upper diagram gives an ideal agreement. Also such stars as V Puppis, or UW Canis Majoris, show good agreement with the simple mass-luminosity relation. The uncertainty of the spectral determination for many of the systems is, as has been emphasized several times by the writer, an obstacle in any attempt to be very definite regarding which of the diagrams is better.

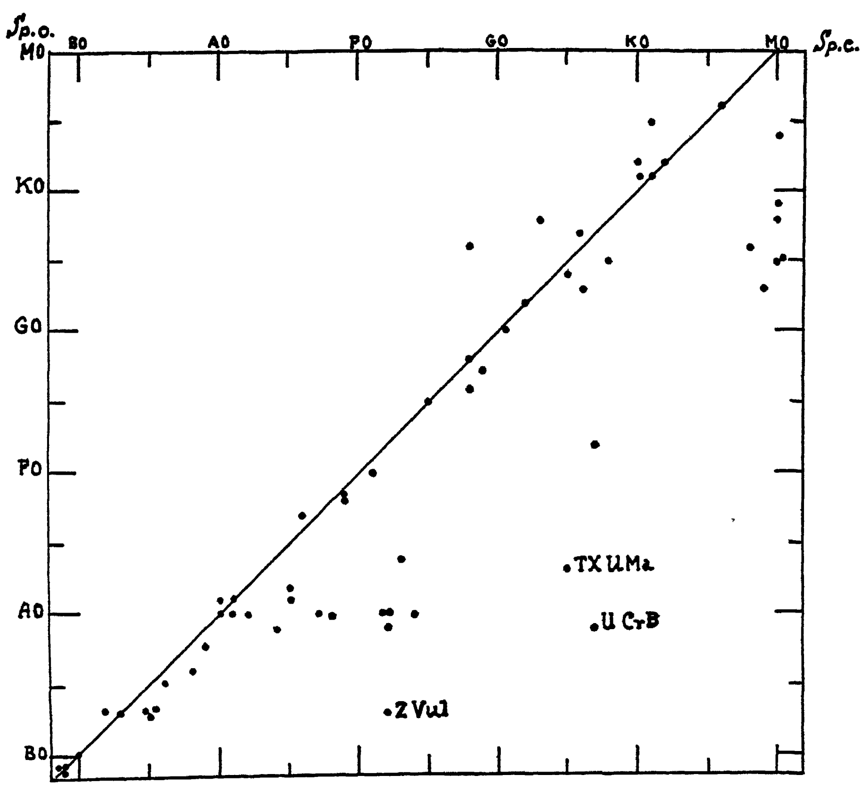

FIG. 2. Comparison of spectra (see Table 5). Abscissae, spectra deduced from relative surface brightness; ordinates, observed spectra.

The spectrum of the secondary component, as derived from the relative surface brightness and the spectrum of the first component, will now be considered. Table 5 gives a comparison between the observed and computed spectra. The relationship is also illustrated in Fig. 2. Stars showing special deviations are indicated by name. It is seen that, although on the average the deviation is not large, the general tendency is for the computed spectra to be too late; one reason may be the uncertainty in the observed spectrum of the second component, since it is difficult to classify when the difference in brightness between the components is large On the other hand, the surface brightness is a very sensitive function of the spectrum, so that we can only be sure of the spectral classification when the depths of the minima are very well determined.

Concerning mass, density, and brightness, it is of interest to inquire how far eclipsing stars differ from the stars in general. Selection by eclipse discriminates strongly against systems in which the fainter star is the smaller. If a system consisted of a normal "giant" and a normal "dwarf" of similar spectrum, the chance of discovery would be very small. This is one reason why our data contain practically no "giants." A combination of two "giants," or of a "late giant" and an early-type star of the Main Sequence, would be detectable, although actually we encounter very few such pairs.

From Tables 3 and 4 we have made a mass-radius diagram (Fig. 3) for over three hundred individual components, using only stars with reliable masses. The figure shows that for the first component *the larger the mass, the larger the dispersion in radius.* From this fact spring some of the most important observed properties of stars. For a given mass, the limited scope of the radius leads to definite limits for the density and the luminosity.

Several points concerning the mass-radius diagram should be mentioned, in order to avoid misunderstanding:

1. Our material is affected by selection in the sense that systems, of which one of the components has a radius approximating that of a white dwarf, will be practically undetectable. If we assume that the number of white dwarfs is actually large, and that there are also numerous intermediates, then the spread of the logarithms of radius for stars with masses near that of the sun may be as great as the spread for large masses.

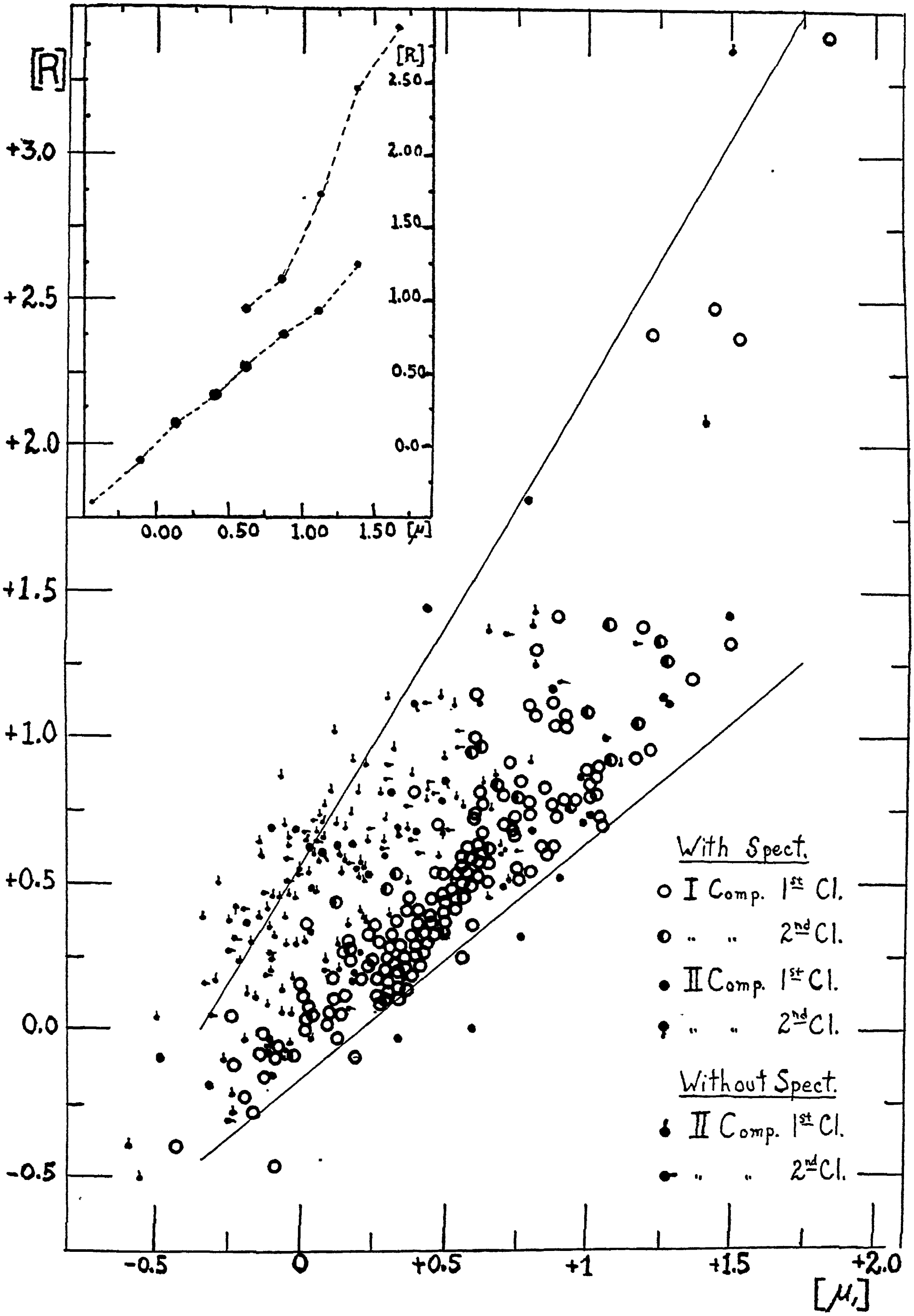

FIG. 3. Mass-radius diagram for the best material here accumulated. Abscissa, logarithm of the mass; ordinate, logarithm of the radius. The inserted graph shows, on a different scale, the results of Table 6.

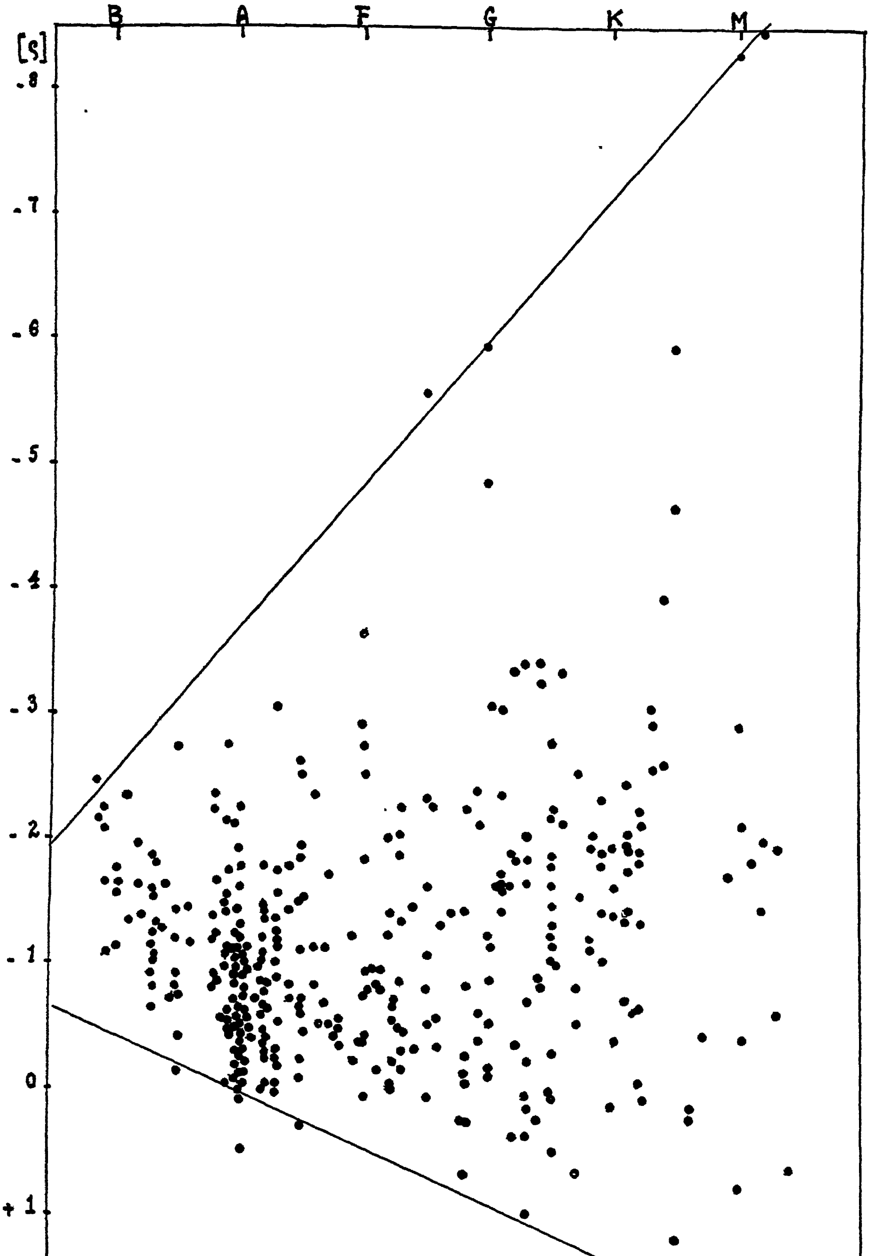

FIG. 4. Density-spectrum relations. The conditions are apparently not fulfilled for an abrupt division of late-type stars into giants and dwarfs.

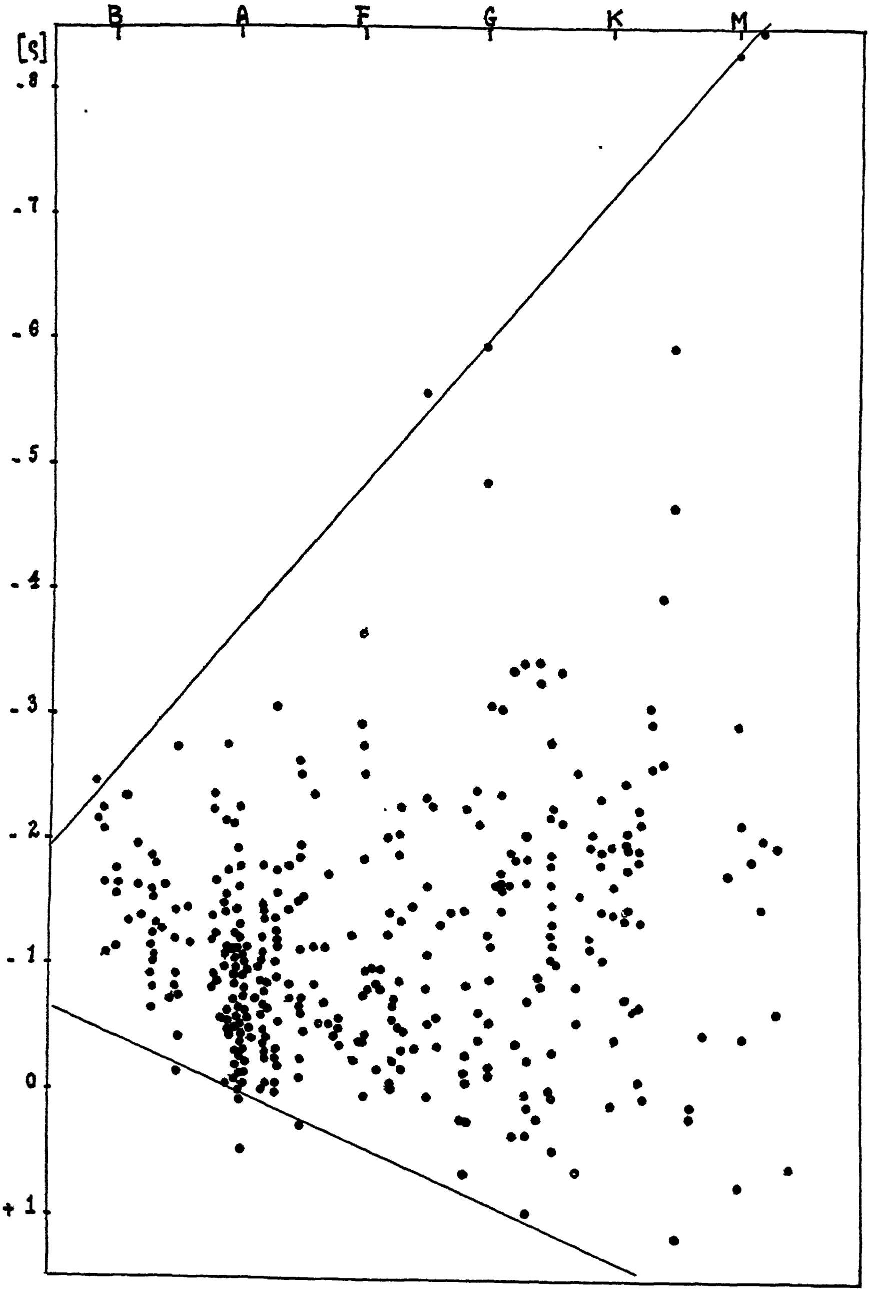

FIG. 4. Density-spectrum relations. The conditions are apparently not fulfilled for an abrupt division of late-type stars into giants and dwarfs.

2. While selection discriminates against the white dwarfs, it operates in favor of the supergiants.

3. A comparison of this diagram with diagrams by Kuiper [30] and Pilowski,[31] which were based on less material, reveals the same trends.

4. Since the numbers of reliable radii and masses obtained here surpass all previous compilations, and since there seems to be no doubt that the relation between mass and radius has only a small dispersion for the first component, we give below a detailed table showing the numerical relations between the two functions. It is remarkable how small the probable errors are.

TABLE 6

MASS-RADIUS RELATION FOR 223 STARS

Average log Mass	p e.	No. of Stars	Main Sequence			Supergiants		
			Average log Radius	p.e.	No. of Stars	Average log Radius	p.e.	No. of Stars
−0.39	±0.07	2	−0.27	±0.09	2			
−0.44		1	−0.40		1			
−0.05	0.02	13	−0.13	0.02	13			
−0.11	0.01	14	−0.11	0.03	14			
+0.11	0.01	18	+0.14	0.02	18			
+0.12	0.01	23	+0.14	0.02	23			
+0.40	0.00	48	+0.28	0.01	48			
+0.40	0.01	73	+0.31	0.01	73			
+0.62	0.01	84	+0.51	0.01	78	+0.91	±0.02	6
+0.61	0.00	68	+0.53	0.00	59	+0.91	0.02	9
+0.86	0.02	28	+0.71	0.02	22	+1.19	0.04	6
+0.87	0.01	24	+0.75	0.02	16	+1.14	0.04	8
+1.12	0.01	19	+0.86	0.03	17	+1.88	0.34	2
+1.11	0.01	14	+0.91	0.03	11	+1.72	0.18	3
+1.37	0.02	7	+1.20	0.05	6	+2.47		1
+1.39	0.03	4	+1.25	0.02	3	+2.47		1
+1.53	0.03	4	+1.33	0.03	2	+2.86	0.34	2
+1.65	0.07	2				+2.89	0.25	2

5. By combining the values in Table 6, we derive the following relation *[7] for 203 stars of the Main Sequence:

$$[R] = 0.82[\mu]; \tag{13}$$

[30] G. Kuiper, *Ap. J.*, **88**, 491, 1938.
[31] K. Pilowski, *Zs. f. Ap.*, **11**, 286, 1936.
*[7] Throughout the present section, square brackets denote logarithms to base 10.

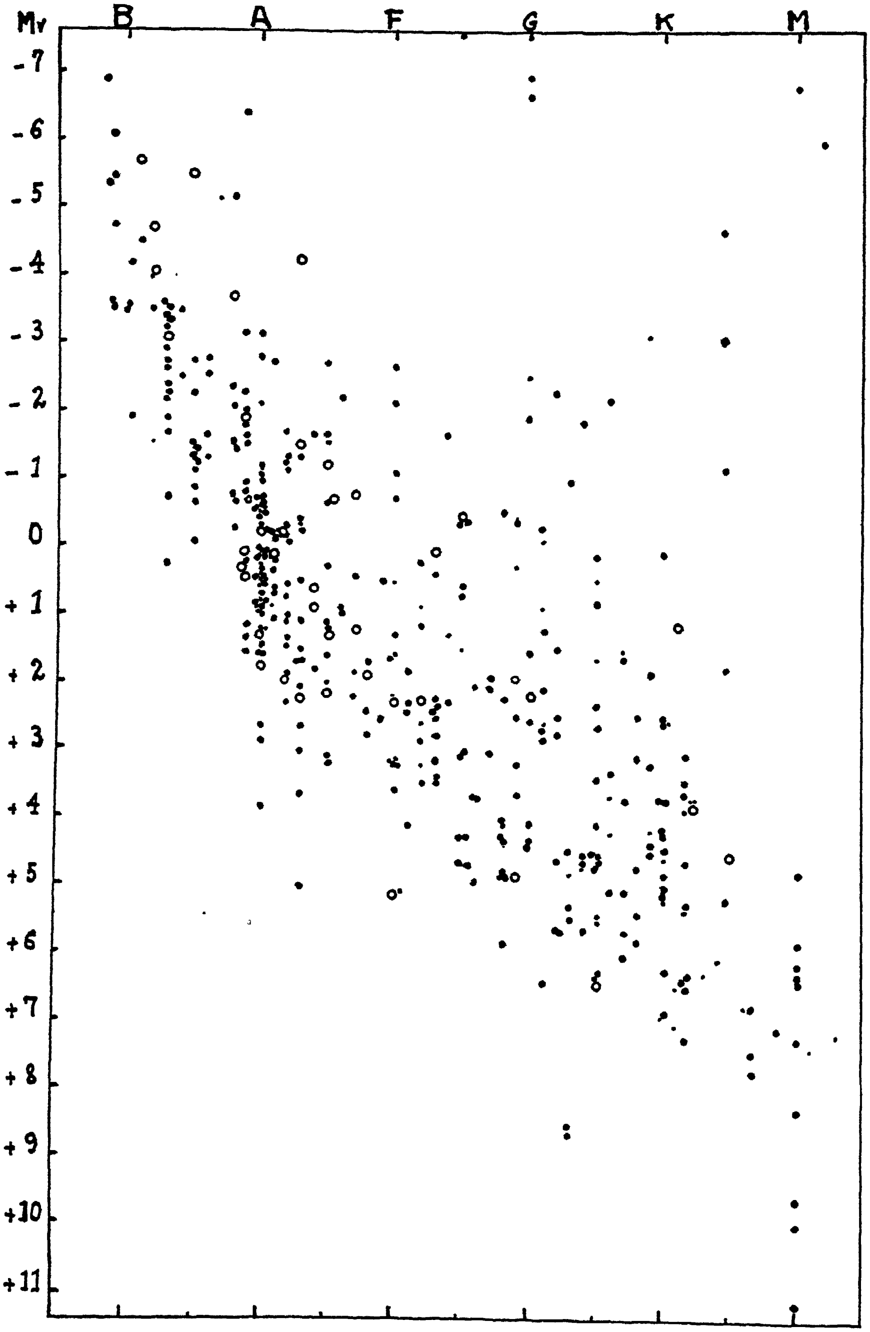

FIG. 5. Russell diagram for greater part of present material. Abscissa, spectrum; ordinate, absolute visual magnitude. The best data are represented by large dots, the less certain by circles, the least certain by small dots.

and for 20 stars of the giant and supergiant branch, with $[\mu] > 0.75$, we obtain:

$$[R] = 2.25([\mu] - 0.75) + 1.00. \tag{14}$$

6. The second component behaves quite differently from the first component.

The stars used in the formation of Table 6 are the heavier components. The values of $\log R$ were divided arbitrarily into two groups, which represent, roughly, the Main Sequence and the supergiants. From the relation $[\mu] - [R] =$ constant, it is seen that the dependence of $[R]$ on $[\mu]$ is independent of the mass-luminosity relation. The two values of $[\mu]$ and $[R]$ in Table 6 are those which were computed with (6 $[T]$) and (4.8 $[T]$). The fact that they are not exactly the same, differing only by a few hundredths, is explained by the shifting of the material from one group to another.

TABLE 7

SPECTRUM AND ABSOLUTE VISUAL MAGNITUDE (MAIN SEQUENCE)*

Spectral Range	Mean Spectrum	Absolute Magnitude	p.e.	No. of Stars	Spectral Range	Mean Spectrum	Absolute Magnitude	p.e.	No. of Stars
O8.5–O9	O8.8	−5.09	±0.29	7	0	F0.0	+1.21	±0.43	9
B0–B2	B1.0	−3.51	0.23	6	F1–F4	F2.4	+2.50	0.15	18
B3–B4	B3.1	−2.63	0.15	14	F5–F7	F5.6	+2.95	0.28	17
B5–B6	B5.3	−1.63	0.13	13	F8–G2	G0.0	+3.79	0.17	27
B8–B9	B8.5	−0.97	0.19	19	G3–G9	G5.3	+4.94	0.11	26
A0	A0.0	+0.19	0.09	54	K0–K3	K0.4	+4.90	0.15	25
A1–A3	A2.0	+0.64	0.14	37	K4–K7	K5.0	+6.84	0.33	4
A4–A9	A5.9	+0.87	0.23	24	K8–M5	M0.0	+7.23	0.22	10

* The following stars were excluded from the means:

Spectral Range	Sub-dwarfs	Subgiants	Giants	Supergiants
B3–B4	Z Vul (+0ᵐ.26)			
B5–B6	U CrB (−0ᵐ.02)			
B8–B9				−5ᵐ.78±0ᵐ.29 (2)
A1–A3	UX UMa (+5ᵐ.02)			
F5–F7			+0ᵐ.32 (2)	ε Aur (−5ᵐ.78)
F8–G2			−0ᵐ.34±0ᵐ.55 (5)	−5ᵐ.25±0ᵐ.82 (3)
G3–G9	+8ᵐ.63±0ᵐ.00 (2)		+0ᵐ.61±0ᵐ.59 (9)	
K0–K3		+1ᵐ.63±0ᵐ.29 (5)		
K4–K7			+0ᵐ.35±1ᵐ.05 (2)	−3ᵐ.86±0ᵐ.37 (2)
K8–M5	RW Dor (+11ᵐ.18)			VV Cep (−6ᵐ.40)

The density-spectrum relations are plotted in Fig. 4. Clearly the conditions for an abrupt division of late stars into "giants" and "dwarfs" are not fulfilled.

With the relation between the mass and the radius is connected the famous one derived by Hertzsprung, of which the Russell diagram is the graphical expression. Figure 5 is the Russell diagram for the greater part of our material. Evidently the dispersion in magnitude for the late spectral classes is very large, as would follow directly from our mass-radius diagram. There is a growing body of other evidence for the existence of a number of "subgiants" in all the late spectral classes. We should mention however that the shifting of the second component parallel to the Main Sequence and its representation as a subgiant are probably produced in part through the computed spectrum, which is generally slightly later than the observed one.

Table 7 gives the average absolute magnitudes for the various spectral classes. Because our material contains so many subgiants, the average magnitudes in the interval G0–M are slightly brighter than those usually adopted. We have limited ourselves in the formation of Table 7 to the first-grade absolute magnitudes only.

I am greatly indebted to Professor Henry Norris Russell and to Dr. Harlow Shapley for their generous discussion of the many problems connected with this work.

THE SOLID-GLASS SCHMIDT CAMERA AND A NEW TYPE NEBULAR SPECTROGRAPH

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(Communicated by Harlow Shapley)

ABSTRACT

The complete fifth-order theory of image errors is applied to both the solid-glass Schmidt camera and to the usual kind of Schmidt camera. The fifth-order defect most important in the formation of images by the Schmidt camera is that called the variation of spherical aberration with angle. The Schmidt camera can be made completely aplanatic, but the resultant optical surfaces differ from those for minimum chromatic aberration. The equation of the meridian section of the correcting surface is developed through tenth-order terms in order to obtain the curve of the correcting surface to sufficient accuracy for the fastest cameras. The fifth-order theory permits a solid Schmidt camera of the limiting focal-ratio of $f/0.30$ and a field of 7° to be made. An exact calculation indicates that the fifth-order terms, even at $f/0.30$ and over a 7° field, represent within ten per cent of the exact value the numerical size of the image aberrations, and are completely adequate for slower cameras and moderate fields.

The solid Schmidt of $f/0.30$ is to be used as the camera in a new type nebular spectrograph. A prism arrangement is presented that affords the advantages of cheapness, compactness, and low light absorption. In addition to one or more small 60° dispersing prisms, from two to four 30–60° prisms are added in order to spread the collimated beam of circular cross-section into a beam of elliptical cross-section, entering the camera. This device yields the above advantages without loss of speed or definition at a given dispersion. The coating of the glass surfaces in such a system is recommended in order that the total efficiency be within 90 per cent of the theoretical limit. The prism device enables the solid Schmidt to be used off-axis without loss of light and thus affords easy access to the focal surface.

RECENTLY Hendrix[2] has published a short description of a thick mirror modification of the Schmidt camera, and still more recently Hendrix and Christie[3] have published an article on Schmidt cameras and on applications of the Schmidt principle to spectroscopy. A Schmidt camera constructed of a single piece of glass is described briefly by Hendrix and Christie for the first time in print. I have heard indirectly that Schmidt himself contemplated such a glass camera, but did not construct one. By the same rumor it seems that the Bergedorf scientists concluded that the thick-mirror type, described later by Hendrix, constitutes the best solution to the problem of obtaining access to the focal sur-

[1] Society of Fellows, Harvard University.
[2] P.A.S.P., 51, 158, (1939).
[3] Sci. Amer., 161, 118, (1939).

face. Unfortunately, the thick-mirror type is optically inferior to the Schmidt camera and cannot be used at the extreme speeds available to the Schmidt camera.[4] Moreover, Hendrix has designed a folded type of solid Schmidt that is convenient in practice and that sacrifices none of the optical performance of the Schmidt camera. The late Sinclair Smith of Mount Wilson came upon the idea of a solid Schmidt camera, but no information concerning his discovery was made public until the acknowledgment by Hendrix and Christie. The present writer discovered the solid Schmidt about the week of Smith's untimely death.

It is the purpose of this article to present the theoretical performance of both the solid and ordinary Schmidt cameras and at the same time to present the design for a relatively simple, fast nebular spectrograph under construction at the Harvard Observatory. The fifth-order theory of both the solid and the usual Schmidt has been complete for over a year, but publication of the theory has been delayed by a desire to have a solid Schmidt camera constructed. Inasmuch as Hendrix has succeeded in completing several of the folded-type solid Schmidts, and has demonstrated that such a system is practical, there is no longer any need for withholding publication of theoretical designs and performance.

The recent work of Blodgett,[5] and of Cartwright and Turner,[6] has permitted stellar and nebular spectrographs to be modified in a way that once might have been thoroughly frowned upon. But now that the coating of glass surfaces [5, 6] for the removal of reflection losses has proved feasible, there can be no objection to the addition of several extra surfaces to a system, if by such an addition some strategic advantage is gained. Moreover, the rule that calls for a small number of glass-air surfaces in a fast spectrograph has been pocketed for some time by the use of fast camera lenses like the Rayton or Bracey lenses, where high speed has more than overcome the inefficiency of many surfaces. All these optical surfaces can now be coated, and the efficiency of existing spectrographs improved by a factor of two or more.

At Mount Wilson it has been the practice to employ the fastest camera lenses available in order that the slit of the nebular spectrograph be as wide open as possible, and that accordingly more light be admitted. Such a fast camera lens reduces the size of a

[4] Sci. Amer., **161**, 314, (1939).
[5] Phys. Rev., **55**, 391, (1939).
[6] Unpublished.

nebula, or a slit-full thereof, to a very small point at the spectrum focus in any particular color. Conversely, the slit-width is so chosen that its monochromatic image at the film or plate is set equal to the resolving power desired, preferably the resolving power of the photographic material. The dispersive unit of the spectrograph then spreads the small image of the slit into a very narrow spectrum. It is necessary, therefore, to broaden the spectrum by trailing to at least the 0.3 mm minimum spectrum width required for measurement and reduction; of course, if a very large nebula is on the slit, and if a strip of the nebula is to be photographed, trailing is unnecessary.

At Lick, on the other hand, Mayall [7] has found that a camera lens working at $f/1.3$ has proved quite satisfactory. Nevertheless, Mayall's instrument, transferred to the 100″ telescope, would be about four times slower for bright objects that do not require trailing, to about twice as slow for small objects that require trailing, compared with the Mount Wilson nebular spectrograph. Mayall loses the extra light afforded by the wider slit used at Mount Wilson, and also loses slightly because of the reciprocity failure of many photographic materials. Where all the light from a small object passes through even a narrow slit, Mayall's spectrograph is on nearly equal footing with the Mount Wilson instrument.

One more important consideration enters. The modern ultra-speed nebular spectrographs are already capable of photographing the spectrum of a slit-full of light from the night-sky background. Regardless of the speed and efficiency of an outfit, the sky background provides the limit to which any nebular spectrograph can go. The only further distinction between nebular spectrographs of the same dispersion concerns the exposure time required to reach a given faintness of the sky background. However, an instrument that is as fast as possible in reaching the limit common to all instruments may employ slower and finer-grain plates or films in order to obtain finer details in the photographed spectrum. If the exposure times are short, photometric work can be carried out on various parts of the same nebula, or radial velocities can be obtained with increased accuracy. An important goal in the design of a nebular spectrograph is to carry both efficiency of the instrument as a whole, and the speed of the camera to the theoretical extremes, consistent with the length of spectrum and the dispersion desired.

[7] P.A.S.P., **48**, 14, (1936).

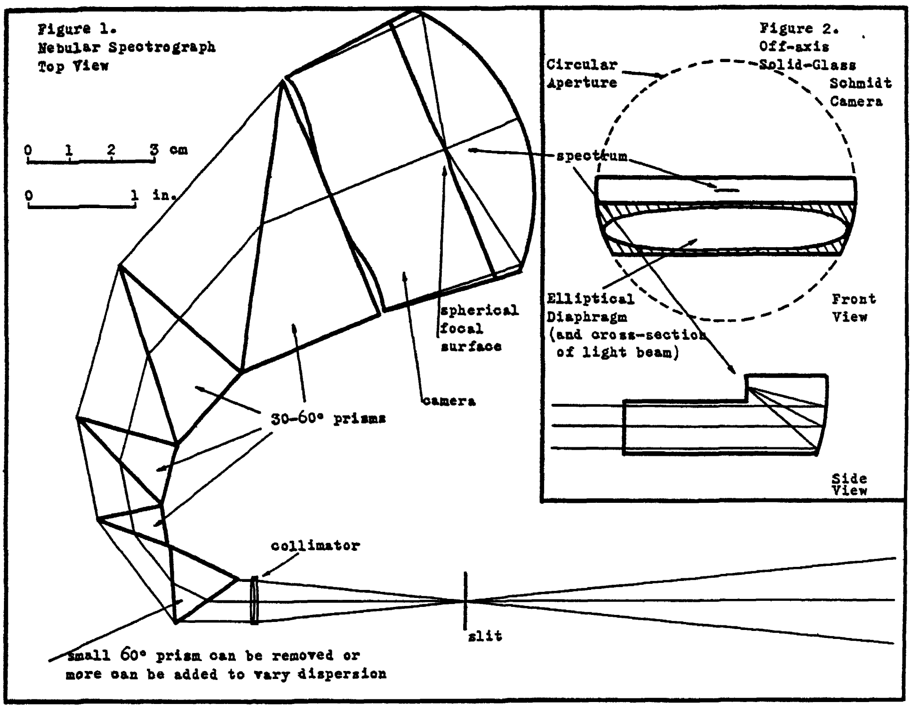
It will be expedient to describe the optics of the spectrograph designed for the Harvard Observatory. Fig. 1 illustrates the complete optical set-up seen from above. The light enters the slit at the Newtonian focus of an $f/5$ reflector, and is brought to a parallel collimated beam by an $f/5$ achromatic cemented doublet of 10 mm aperture, coated on both external faces in order to reduce loss of light by reflection to a negligible amount.

The parallel beam of light then passes through one, two or more identical 60° flint prisms 10 mm thick, and of correspondingly longer sides. Only one such 60° prism is shown in Fig. 1. It should be noted that an inch of glass in a prism base is sufficient to resolve the sodium doublet, and that in the system under discussion, therefore, the final resolution in the spectrum is limited only by photographic grain and not by the dispersive unit at even the highest dispersion.

The light next passes through a series of 30–60° flint glass prisms, the purpose of which is not only to add dispersion, but also to spread the originally circular beam into an elliptical beam of light still optically parallel, by making use of the projection factor related to the angle through which the light beam is turned. The four prisms sketched in Fig. 1 convert the collimated beam from the 60° prisms, which is circular in cross-section and 10 mm in diameter, into a collimated beam the cross-section of which is an ellipse with axes 10 mm and 60 mm. The light emerging from the last prism face is still accurately parallel. Of course, if the 30–60° prisms are not coated, a sixth of the light will be lost at the four faces perpendicular to the emerging beam. If the prism faces of the system are all coated, the photographed light will be almost completely unpolarized. Even if the prisms are not coated, their total reflection losses are no more serious than the losses tolerated in present-day nebular spectrographs in other ways, and yet there remain a number of advantages that recommend this prism arrangement.

The angular width of the slit, as affected by the prism, must next be considered. When a collimated beam of light enters a 30–60° prism and emerges perpendicular to the second surface, the angular width of the slit, as seen in the beam, is decreased. Conversely, when the light beam enters perpendicular to the surface, the emergent light beam is smaller in cross-section, but the angular width of the slit, as seen in the beam, is increased. With the

arrangement here proposed, the angular width of the slit is the same as that viewed from a collimating lens, 60 mm in diameter and 300 mm or 12″ away from the slit. Thus, another effect of the 30–60° prisms is to reduce the size of the spectrograph in all its parts (including ordinarily wasted space), and likewise to replace three or more large prisms (60 mm thick, expensive, in need of temperature control and accurate mounting, the resolving power of which would be largely wasted by the lesser resolving power of



the photographic materials employed) by *small inexpensive* prisms that occupy all the physical space of the spectrograph. Moreover, in order to reduce light-loss by absorption in the prisms, it is desirable to have as small prisms as possible. The system described here provides for a small compact mounting that should be very good for accurate radial velocities. If the usual type of spectrograph were made for an $f/5$ telescope, it would be of the order of a foot and a half long, as compared with six inches for the spectrograph in Fig. 1 of the same final performance. If the compact type of spectrograph described here were fitted to an $f/20$ Cassegrain focus, the new collimator required would have an 8″ focal

length, as compared with a collimator of four-foot focal length in the customary type of spectrograph.

One remembers here that the accepted rule calls for the use of prisms at minimum deviation in order that a pure spectrum can be obtained; nevertheless, if the prism system is arranged in the above fashion, for which the angular width of the slit is smaller than it would be at minimum deviation for a given numerical slit-width, the purity of the spectrum is actually increased.

The reducing factor between the $f/5$ collimating lens and the $f/0.30$ camera described below is $16.67\times$, so that the width of the image of the slit on the photographic plate is about a seventeenth as wide as the slit itself. For the very finest work the resolving power of the slower and finer grain film can be taken as 0.01 mm. For this resolving power the slit can be 0.17 mm wide (a reasonably wide slit for an $f/5$ telescope). For the fastest films, like the Agfa "Superpan Press," the resolving power can be taken as 0.02 mm for which the slit-width can be 0.34 mm. The H and K lines of calcium can still be detected at a dispersion of 500 A/mm if the slit-image is 0.04 mm and the slit-width 0.68 mm. If an $f/20$ Cassegrain were to be used as the light collector, the corresponding slit-width would be 2.7 mm.

The angular height of a given length of the slit will be preserved throughout the system as equivalent to that subtended at the center of the 10 mm collimating lens. Consequently, a square of light on the slit, such that the side of the square is equal to the slit-width, will photograph at the spectrum focus of the camera as a rectangle the height of which is six times its width. In other words a stellar or nebular spectrum will require less trailing, and along the spectrum will still be extremely sharp, even though the slit is wide so that a great deal of light can enter.

So far, therefore, we have a collimating and dispersive system that is more than 90 per cent efficient if made according to the latest technical advances, and that yields several other advantages. Any fast camera lens of high quality with surfaces coated to prevent reflection losses, could now be used to photograph the spectrum without exhausting the resolving power of the prisms. The prism arrangement affords an added advantage, common to most cameras, concerning the imaging quality of the camera. The rays of an incident beam on the circular aperture of a camera that cause the largest image errors are the skew rays, parallel to a

meridian plane, but above and below this plane, and which at the same time are tilted. By filling the camera aperture only partially with the long elliptical beam of small height afforded by the prism arrangement, one can remove the most damaging rays as by a diaphragm, so important to circular apertures, and thus one can increase the extent of useful field.

The author has long desired a camera working at the extreme speed of $f/0.30$. This desire is met by the Schmidt camera in glass, which was studied extensively in order to ascertain the speed to which one could push its design. I was very glad to find that even at this upper limit to all speeds involving photography, I still should be able to obtain from two to three millimeters of spectrum in satisfactory focus. Another advantage afforded in the case of the solid Schmidt by the prism arrangement is that by using the camera off-axis in the direction perpendicular to the spectrum and major axis of the elliptical diaphragm, one can obtain access to the focal surface, which one could otherwise achieve only by the hole-in-the-glass type or the folded-type of Hendrix, and that therefore 8 mm roll film can be used to make loading and observing a convenience. Of course, oil immersion is necessary in order that the light can emerge from the glass at the focal surface. The arrangement of the camera is shown in the three plan views in the diagrams, front and side views in Fig. 2, and top view in Fig. 1. The elliptical diaphragm and cross-section of the beam entering the camera is shown in the face view of the camera in Fig. 2. The camera can be constructed of slab glass of highest quality.

A minor advantage of the design is that the depth of focus is comparatively great vertically, because the camera works at a speed of $f/1.8$ in the vertical direction. Of course the depth of focus along the spectrum is practically zero, as is the case with all fast cameras. If the glass camera is made precisely for focus in parallel light, then the focusing in the spectrograph can be done with the collimator lens. A motion of 1 mm of the collimator lens along the axis changes the focus at the camera by only 0.004 mm so that critical focusing is both practical and easy.

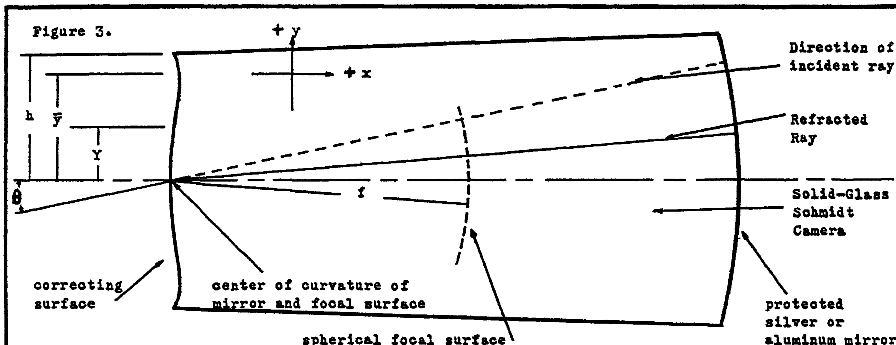
This camera works at $f/0.30$ along the spectrum and at $f/1.8$ in the breadth of the spectrum. Therefore, a nebula that just fills a slit of 0.50 mm width will photograph into a spectrum 0.18 mm broad; the image of the slit in the direction of the spectrum length will be 0.03 mm.

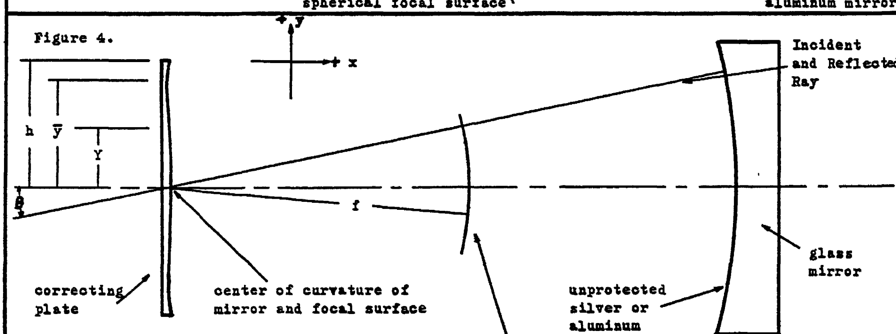
Thus the arrangement described here offers a number of advantages and no serious disadvantages except for certain special kinds of work. The entire optical train can be made of slab glass of highest quality, 12 mm thick, and therefore quite inexpensive. The camera, though hard to make, more than justifies itself in simplicity and effectiveness. The Harvard spectrograph will work in the spectrum range from 3900 to 6700A at linear dispersions varying between 500 and 250 A/mm at H β. The prism system will be adjustable along with the camera in order to provide for this range with but two settings at the various dispersions. The film will be in 8 mm strips fed between spools and pressed against the spherical focal surface by means of a plunger. Of course, with the fastest photographic materials the sky background will be reached in only a few minutes; fine grain films or thin glass plates will ordinarily be employed.

At this point a relatively new principle in stellar or nebular spectrographs is encountered. When the camera is working at the highest possible speed, $f/0.30$, the slit is open as wide as is theoretically possible. (The limit to the camera speed is set by the index of refraction of optical glasses, and by the amount of coma permissible. A system fully corrected for coma cannot attain this focal-ratio, unless glasses with very high index of refraction are employed.) Regardless of the size of the spectrograph, the amount of light admitted through the slit is exactly the same and the purity of the resulting spectrum the same. The only other detail concerns the dispersion desired. Whether one obtains linear dispersion by a long focal-length camera of large diameter, and a few large prisms, or a short focal-length camera of small diameter and a larger number of small prisms, the resulting spectrum will be the same. The rule followed here is to reduce the size of the spectrograph so that there is a minimum amount of glass in the system consistent with the assigned dispersion and length of spectrum desired on a single photograph. A practical lower limit to the size of the spectrograph is given by setting the resolving power of the glass prisms equal to the resolving power of the fine grain emulsions at the highest linear dispersion contemplated.

Where the entire range from 3900 to 6700A is desired on a single spectrum, the camera aperture must be reduced in speed in order to cut off more of the damaging rays. There are two ways of doing this, with resultant efficiency dependent upon the character

of the nebula being photographed. If the nebula is smaller than the slit-width permitted by the $f/0.30$ arrangement, the camera should be used behind the first two 30–60° prisms and thus work at $f/0.60$; the beam entering the camera aperture will be an ellipse with axes 10 mm and 30 mm. The slit will be only half as wide as in the fastest arrangement, but most of the nebular light will still enter, so that the relative speeds of the two arrangements will not differ by as much as a factor of two. If the nebula is already





larger than the slit-width permitted by the $f/0.30$ arrangement, then either the camera can be stopped down to $f/0.60$ or else the alternative arrangement described above can be used with about the same efficiency.

This sketch of the optics of the spectrograph is now completed. The following deals with a summary of the fifth-order theory of the solid Schmidt and of the ordinary Schmidt. All the following equations are given with R, the radius of curvature of the spherical mirror, as unit length. For the solid-glass Schmidt, as presented in Fig. 3, the equation of the meridian cut of the correcting surface

for minimum chromatic aberration [8] is

$$x = ay^2 + by^4 + cy^6 + \cdots,^{9} \tag{1a}$$

where

$$\begin{aligned} a &= +\frac{3}{8}\frac{n}{n-1}h^2\left[1 + \frac{9}{16}h^2 + \cdots\right] \\ b &= -\frac{1}{4}\frac{n}{n-1}\left[1 - \frac{3}{2}h^2 - \cdots\right] \\ c &= -\frac{3}{8}\frac{n}{n-1}\left[1 - \cdots\right]. \end{aligned} \tag{2a}$$

In formulæ (1a, 2a) n is the index of refraction, preferably high, and h is the radius of the circular aperture. For the solid Schmidt of minimum chromatic aberration the equivalent focal length is

$$f' = +\frac{1}{2n}\left[1 + \frac{3}{8}h^2 + \frac{45}{128}h^4 + \cdots\right].^{9} \tag{3a}$$

The distance marked f in Fig. 3 is exactly equal to nf'. The separation of the correcting surface or correcting plate from the primary

[8] Cf. Strömgren, B. Vierteljahresschrift, **70**, 65 (1935); Smiley, C. Pop. Ast., **44**, 415, (1936).

[9] The author has extended the coefficients for the extremely fast cameras (for minimum chromatic aberration) as follows:

$$x = ay^2 + by^4 + cy^6 + dy^8 + ey^{10} + \cdots, \tag{i}$$

where

$$\begin{aligned} a &= +\frac{3}{8}\frac{n}{n-1}h^2\left[1 + \frac{9}{16}h^2 + \frac{135}{256}h^4 + \left(\frac{1865}{1024} - \frac{1}{16}\frac{n}{n-1}\right)h^6 + \cdots\right] \\ b &= -\frac{1}{4}\frac{n}{n-1}\left[1 - \frac{3}{2}h^2 - \frac{9}{32}h^4 + \left(\frac{27}{512} - \frac{27}{64}\frac{n}{n-1}\right)h^6 + \cdots\right] \\ c &= -\frac{3}{8}\frac{n}{n-1}\left[1 - 2h^2 - \left(\frac{3}{8} - \frac{15}{16}\frac{n}{n-1}\right)h^4 + \cdots\right] \\ d &= -\frac{45}{64}\frac{n}{n-1}\left[1 - \left(\frac{32}{15} + \frac{8}{15}\frac{n}{n-1}\right)h^2 + \cdots\right] \\ e &= -\frac{n}{n-1}\left[\frac{61}{40} + \frac{1}{8}\frac{n}{n-1}\right] + \cdots. \end{aligned} \tag{ii}$$

Also

$$f' = \frac{1}{2n}\left[1 + \frac{3}{8}h^2 + \frac{45}{128}h^4 + \frac{27\cdot 31}{2^{11}}h^6 + \left(\frac{3\cdot 5377}{2^{14}} - \frac{3}{2^7}\frac{n}{n-1}\right)h^8 + \cdots\right], \tag{iii}$$

$$\bar{y} = \frac{1}{2}\sqrt{3}\,h\left[1 + \frac{3}{16}h^2 + \frac{9}{64}h^4 + \left(\frac{1867}{2^{12}} - \frac{1}{32}\frac{n}{n-1}\right)h^6 + \cdots\right], \tag{v}$$

$$\Upsilon = \frac{1}{2}h\left[1 + \frac{9}{16}h^2 + \frac{9}{16}h^4 + \left(\frac{9\cdot 583}{2^{12}} - \frac{9}{2^7}\frac{n}{n-1}\right)h^6 + \cdots\right]. \tag{vi}$$

For the extreme focal-ratios and long focal-length cameras, even the above expansions will prove insufficient; for such extreme cases either a differential correction or else an integration based on ray-tracing formulæ must be carried through (v. Smiley, *op. cit.*).

mirror is much less critical than the separation of the focal surface from the primary mirror. This latter critical distance is equal to $(R - f)$ such that

$$(R - f) = (1 - f) = +\frac{1}{2}\left[1 - \frac{3}{8}h^2 - \frac{45}{128}h^4 - \cdots\right].^9 \quad (4a)$$

If the correcting surface is not at the center of curvature of the spherical mirror (the diaphragm being coincident with the correcting plate) third-order coma is introduced in the ratio of Δ/R of the third-order coma of the mirror made into the usual paraboloidal type without correcting plate, where Δ is the error or departure of the correcting plate from the center of curvature.

In Fig. 3, the distance $\bar{y}$ represents the height of the zone above the x-axis for which incident rays parallel to the x-axis are undeviated. At $\bar{y}$ the normal to the correcting surface is parallel to the x-axis. In terms of h

$$\bar{y} = \frac{1}{2}\sqrt{3}h\left[1 + \frac{3}{16}h^2 + \cdots\right].^9 \quad (5a)$$

The distance Y represents the height of the zone above the x-axis for which incident rays parallel to the x-axis are deviated in a convergent sense exactly as much as rays incident at height h are deviated in a divergent sense. It is this condition of equality in slope that minimizes the greatest lens action of the correcting surface and hence chromatic aberration. In terms of h

$$Y = \frac{1}{2}h\left[1 + \frac{9}{16}h^2 + \cdots\right].^9 \quad (6a)$$

For the ordinary Schmidt illustrated in Fig. 4, the equations corresponding to (1a) and (2a) are consecutively

$$x = ay^2 + by^4 + cy^6 + \cdots,^{10} \quad (1b)$$

where

$$\begin{aligned} a &= -\frac{3}{8(n-1)}h^2\left[1 + \frac{9}{16}h^2 + \cdots\right] \\ b &= +\frac{1}{4(n-1)}\left[1 - \frac{3}{2}h^2 - \cdots\right] \\ c &= +\frac{3}{8(n-1)}[1 - \cdots], \end{aligned} \quad (2b)$$

[10] The equations (i–vi) hold for the ordinary Schmidt, if $\frac{n}{n-1}$ is replaced everywhere it occurs by $\frac{1}{n-1}$.

$$f' = \frac{1}{2}\left[1 + \frac{3}{8}h^2 + \frac{45}{128}h^4 + \cdots\right],^{10} \tag{3b}$$

$$(R - f) = (1 - f) = \frac{1}{2}\left[1 - \frac{3}{8}h^2 - \frac{45}{128}h^4 - \cdots\right], \tag{4b}$$

$$\bar{y} = \frac{1}{2}\sqrt{3}h\left[1 + \frac{3}{16}h^2 + \cdots\right], \tag{5b}$$

$$Y = \frac{1}{2}h\left[1 + \frac{9}{16}h^2 + \cdots\right]. \tag{6b}$$

For the same physical size, the two correcting surface curves (1a) and (1b) differ only by the factor n, so that compared with the ordinary Schmidt, the solid Schmidt has the deeper correcting surface. In equation (3b), the equivalent focal length f' is equal exactly to the distance marked f in Fig. 4. As the reversal of sign in (2b), compared with (2a), indicates, for theoretical exactness the flat side of the Schmidt plate should be toward the infinite light source and away from the mirror. Thereby, all considerations of the thickness of the correcting plate disappear, and the best possible symmetry is obtained. One can gain nothing optically by curving both sides of the correcting plate. On the other hand, if the correcting plate be very thin, one need have no compunctions about curving both sides of the correcting plate, in order to make manufacturing easier. The optical errors introduced are of the seventh order if the thickness is of the second order (*i.e.*, in this case, a second order variation of the fifth order coefficient), and are therefore negligible.

For the same physical size (R and h are the same) the solid Schmidt is n^2 faster than the usual Schmidt for direct photography of objects of finite size, and the curvature of the focal surface the same (*i.e.*, the focal surface coincides with the surface of a sphere of radius f given by equation (4) the center of which coincides with the center of the correcting plate).

The above equations are all given in terms of the semi-aperture h, for minimum chromatic aberration. However, h may be regarded as a parameter. The relation $Y < \bar{y} < h$ holds accurately. If h is set equal to zero, the center part of the correcting surface has zero power. For each value of h, corresponding values of Y, $\bar{y}$, and f' are obtained. The chromatic aberration will be a minimum only when h is set equal to the semi-aperture to be used.

Strömgren[11] has worked out the third-order theory of the

[11] Strömgren, B., *op. cit.*

Schmidt camera, but in view of the fact that all the Seidel monochromatic aberrations are zero, most of his discussion deals with the design of the correcting surface and with its color troubles. The present writer has considered it of interest to work out the fifth-order aberrations of the Schmidt camera in order to ascertain the utmost usefulness of the arrangement. These aberrations arise from the small uncompensated lens errors of the correcting surface. They are absent in the case of a sphere used with a diaphragm and in this case the uncompensated spherical aberration of the mirror is the only, but ruinous, aberration of the system. Certain arrangements employing the sphere and diaphragm are useful. For moderate apertures it would be effective to use such a sphere and diaphragm for out-of-focus photometry. Certainly there would be no color troubles or distance correction, since the spherical aberration of the mirror is constant over the unvignetted field. The illumination of the out-of-focus circle of spherical aberration would not be uniform if high apertures were attempted.

We now proceed to a consideration of the fifth-order errors of the solid-glass Schmidt and the ordinary Schmidt. Let θ_0 be the angle between the optical axis and the principal ray of an incident pencil as shown in Fig. 3. Let μ_0 be the sine of this angle. Let the optical axis of the system be the x-axis and let y_0 and z_0 be the coordinates of the point at which an incident ray of the pencil strikes the first lens surface (and diaphragm). We need consider only rays parallel to the xy plane, because of the circular symmetry of the system. Let $(\xi_0, \eta_0, 0)$ define the position of the point in the xy plane at which the principal ray defined by $(\mu_0, 0, 0)$ strikes the focal surface, such that $\xi_0^2 + \eta_0^2 = f^2$. Let $\Delta\eta$ and $\Delta\zeta$ express the departure of the intercept of the ray defined by (μ_0, y_0, z_0) with the focal surface from the point $(\xi_0, \eta_0, 0)$ in the focal surface, where $\Delta\eta$ is measured in the y-direction and $\Delta\zeta$ in the z-direction. In spectroscopy and general astronomical photography one is interested primarily in $\Delta\eta$ and $\Delta\zeta$ which express the errors in the imaging quality of the system.

The table furnishes the power series by which one can map the aperture or any part thereof on the image plane or focal surface. The table is given for the solid-glass Schmidt. The expansion for the ordinary Schmidt is briefly

$$\eta = \eta_0 + \Delta\eta; \quad \zeta = \Delta\zeta, \tag{7}$$

where

$$\eta_0 = f'\mu_0, \tag{8}$$

$$\Delta\eta = -\frac{3(2n+1)}{16n}h^2\mu_0^2y_0 + \left(1+\frac{1}{4n}\right)\mu_0^2y_0^3 + \left(\frac{1}{2}+\frac{1}{4n}\right)\mu_0^2y_0z_0^2, \tag{9}$$

$$\Delta\zeta = -\frac{3}{16n}h^2\mu_0^2z_0 + \frac{1}{4n}\mu_0^2z_0^3 + \left(\frac{1}{2}+\frac{1}{4n}\right)\mu_0^2y_0^2z_0. \tag{10}$$

The form of the Schmidt correcting plate that most nearly realizes the Abbe sine condition as ascertained by exact calculation has no central bulge but increases in depth monotonically to the edge. This follows from the relation: focal-ratio $= f/2h = 0.5/n$ in limit, if Abbe's sine condition holds. For $n = 1.7$, the limiting focal ratio $= 0.30$ but the practical amount is focal ratio $= 0.35$ or smaller. The slope at the edge is nearly 0.7 with consequent considerable chromatic aberration. In the form of camera designed for Harvard, the correcting surface is minimized for chromatic aberration. The coma introduced by minimized color aberration is less than the tolerable amount at the focal length considered. For this form, the focal ratio of $f/0.30$ can actually be obtained.

The Seidel aberrations are invariant with color and are all zero with the single exception of the variation of spherical aberration with color (the well-known color error of the Schmidt). This color error is n times smaller for the solid Schmidt than for the usual Schmidt of the same physical size, although the solid Schmidt is n^2 faster. Let σ be the diameter of the circle of confusion for a particular wave-length in units of R. Then, for the solid Schmidt

$$\sigma = \frac{h^3}{4n(n-1)}\Delta n. \tag{11a}$$

Δn is the difference between the index of refraction n, corresponding to the wave-length selected for perfect focus, and the index of refraction corresponding to the particular wave-length mentioned above. For the usual Schmidt

$$\sigma = \frac{h^3}{4(n-1)}\Delta n.^{12} \tag{11b}$$

[12] Cf. Strömgren, B., *op. cit.*

For an $f/0.30$ solid Schmidt of 60 mm aperture in barium crown $\sigma = 0.04$ mm for the C line, if perfectly corrected for the F line. However, by using a barium crown correcting plate ce-

THE OPTICAL PERFORMANCE OF THE SOLID-GLASS SCHMIDT CAMERA

	Image Errors	$\eta = \eta_0 + \Delta\eta =$	$\zeta = \Delta\zeta =$
First Order	Equivalent Focal Length	$\{\mu_0\} = f'$	
	Gaussian Focus	$\{y_0\} = 0$	$\{z_0\} = 0$
Third Order	Spherical Aberration	$\{y_0^3\} = 0$	$\{z_0^3\} = 0$
		$\{y_0 z_0^2\} = 0$	$\{y_0^2 z_0\} = 0$
	Coma	$\{\mu_0 y_0^2\} = 0$	$\{\mu_0 y_0 z_0\} = 0$
		$\{\mu_0 z_0^2\} = 0$	
	Astigmatism	$\{\mu_0^2 y_0\} = -\dfrac{3(n+2)}{16n^2} h^2$	$\{\mu_0^2 z_0\} = -\dfrac{3}{16n} h^2$
	Distortion	$\{\mu_0^3\} = 0$	
Fifth Order	Secondary Spherical Aberration	$\{y_0^5\} = 0$	$\{z_0^5\} = 0$
		$\{y_0 z_0^4\} = 0$	$\{y_0^4 z_0\} = 0$
		$\{y_0^3 z_0^2\} = 0$	$\{y_0^2 z_0^3\} = 0$
	Secondary Coma	$\{\mu_0 y_0^4\} = 0$	$\{\mu_0 y_0 z_0^3\} = 0$
		$\{\mu_0 z_0^4\} = 0$	$\{\mu_0 y_0^3 z_0\} = 0$
		$\{\mu_0 y_0^2 z_0^2\} = 0$	
	Variation of Spherical Aberration with Angle and Flügelfehler	$\{\mu_0^2 y_0^3\} = \dfrac{1}{4n} + \dfrac{1}{n^2}$	$\{\mu_0^2 z_0^3\} = \dfrac{1}{4n}$
		$\{\mu_0^2 y_0 z_0^2\} = \dfrac{1}{4n} + \dfrac{1}{2n^2}$	$\{\mu_0^2 y_0^2 z_0\} = \dfrac{1}{4n} + \dfrac{1}{2n^2}$
	Variation of Coma with Angle and Pfeilfehler	$\{\mu_0^3 y_0^2\} = 0$	$\{\mu_0^3 y_0 z_0\} = 0$
		$\{\mu_0^3 z_0^2\} = 0$	
	Secondary Astigmatism	$\{\mu_0^4 y_0\} = 0$	$\{\mu_0^4 z_0\} = 0$
	Secondary Distortion	$\{\mu_0^5\} = 0$	

Spherical Aberration is zero to all orders. Residual Coma is very small, and is minimum when h is near zero. Variation of Third Order Errors with Color is zero (v. text).

The braces symbolize the coefficient of the power term enclosed.

mented to a flint base of the same index, complete achromatism can be obtained. The Harvard camera employs such an achromatic plate.

Likewise, it is an easy matter to design an achromatic plate that will remove all practical color troubles of the ordinary Schmidt camera; where extreme focal-ratios and long focal-lengths are involved, such achromatism of the correcting plate is imperative, if fine definition over all colors is to be achieved. The suggestion of making a Schmidt correcting surface of a tilted mirror is not to be recommended, except perhaps for slower cameras. The mounting of a telescope and the accuracy required on the elliptically figured mirror would consume more time than the making of an achromatic correcting plate. Moreover, it should not be very difficult to make an achromatic combination. The crown, being more steeply curved, can be made by template and brought to a full polish on a flexible tool. All of the final figuring can be left to the lesser curvature of the flint, and the combination will be still achromatic for all practical purposes. The main pitfall would probably come in the presence of sharp zones. The steep curves required by complete achromatization should not be used, but the correction should be stopped at a point sufficient to cover the tolerance assigned.

For the most part the index of refraction reduces the optical errors of the solid Schmidt compared with the ordinary Schmidt. The glass Schmidt, though simple in design, is excellent in its performance. If one includes the position of the diaphragm in the theory, one can easily show that the diaphragm is in its best position when coincident with the correcting surface, even for fifth order aberrations.

The writer has compared the fifth-order formulæ with the precise results of ray-tracing. The fifth order terms represent the characteristic function of the Schmidt so accurately that even at the extreme focal ratio of $f/0.29$ the fifth order terms are only 10 per cent in error over a 7° field; for cameras slower than $f/0.6$ and for moderate fields the fifth-order terms are completely adequate. The rapid convergence of the power-series expansions is quite encouraging in view of the insuperable complexity of the exact expressions for characteristic functions of lens systems.

A FAMILY OF FLAT-FIELD CAMERAS, EQUIVALENT IN PERFORMANCE TO THE SCHMIDT CAMERA

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(Communicated by Harlow Shapley)

ABSTRACT

A two parameter family of telescopes is described that have the desirable properties of being aplanatic and anastigmatic on a flat field. Distortion, unimportant for astronomical purposes, is small; for one member of the family distortion is zero. Each member of this family of cameras is equivalent in optical performance to the Schmidt camera. The tube length can be made as short as one-third the focal length.

All members of the family of cameras have a correcting plate, a primary concave mirror, and a secondary convex mirror. The two parameters remaining after the conditions for aplanatism and anastigmatism have been satisfied, are the distance of the correcting plate from the primary mirror and the distance of the photographic plate from the secondary mirror. A large field can be obtained free from vignetting.

The two mirror surfaces are aspherical for all but a single parameter family for which the surfaces are so closely spherical as to require but a small fraction of the figuring of a paraboloid of the same focal length. Even in the case where both mirrors are aspherical, there exist no inflection points to complicate manufacture.

The curves of the optical surfaces can all be expressed as rational functions of the parameters. The properties of the families of the Schmidt and Schwarzschild mirror systems are discussed relative to this family of cameras.

MANY years ago Schwarzschild [2] set out to find the most useful system of two mirrors possible in an astronomical camera. He found that although there exists a two-mirror system that is aplanatic and anastigmatic on a flat field, the system is of small practical use because of the nearly total silhouetting of one mirror by the other. Moreover, the primary mirror of the system is convex, and the secondary concave. Siedentopf's dark-field cardioid condenser makes use of a modification of this solution in microscopy. A second aplanatic and flat-field anastigmatic solution also exists in the Schwarzschild equations, such that the image plane is virtual and therefore impractical without further optical aid.

Schwarzschild next obtained the equations of an aplanatic family that retains the image defect of astigmatism, but the mean focal surface of which is very nearly plane. Since the astigmatism

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[2] *Gött. Mitt.*, IX–XI (1905).

is not excessive, one can obtain a moderately large, useful field. The best-known member of the family employs a secondary one-half the diameter of the primary. Both vignetting and silhouetting are necessarily present. Both mirrors of the system are aspherical, but within figuring distance of a conic section. The tube length is long in comparison with the focal length, so that the tube flexure is of concern to the maker of a Schwarzschild system.

Improvements in the imaging quality of reflectors were slight thereafter until Schmidt [3] introduced the important idea that small deformations in an otherwise weak lens can be very effective in eliminating the image defects of spherical aberration and coma. The camera that resulted from Schmidt's application of his idea is too well-known to need description here. Although it is not likely to be superseded in simplicity or effectiveness by any other type of astronomical camera, the Schmidt has two defects. The first is that the star images come to a focus on a spherical surface the curvature of which is the reciprocal of the equivalent focal length. The second is that the overall tube length is at least twice the focal length. Tube flexure and temperature expansion are serious only between the film holder and the mirror.

Wright [4] and Väisälä [5] have generalized the original Schmidt camera into an entire family of Schmidt cameras and have shown that the two surfaces of astigmatism cannot coalesce in a flat surface by means of a correcting plate and an aspherical mirror. The Wright camera, called the "short" camera to distinguish the member of his family that has a flat field, corresponds to the Schwarzschild two-mirror system.

Wright's work in turn can easily be generalized to show that astigmatism cannot be eliminated on a flat field even when several correcting plates are employed, all separated, as long as the total powers of the correcting plates are of the second order with respect to the mirror.

Three mirrors with aspherical surfaces afford enough unknowns to yield excellent performance mathematically, but the silhouetting and vignetting in such a system render it useless. Furthermore, if correcting plates are employed in the converging beam of a mirror telescope, the resulting performance is none too effective.

[3] Schmidt, B., *Mitt. Hamb. Sternwarte in Bergedorf*, 7, 36, 15. Stromgren, B., *Vierteljahresschrift*, 70, 65 (1935).
[4] *P.A.S.P*, 48, 14 (1936).
[5] Väisälä, Y., *A.N.*, 259, 198.

Thus, only one more elementary possibility remains of using mirrors to gain great convergence without color defects, and that is a system of one correcting plate and two mirrors. One can see at once that such a system properly designed will be free not only of spherical aberration and coma of all orders, but will also be anastigmatic on a flat field through third-order theory. Fig. 1 portrays the optical arrangement of the general member of such a system.

Even after all the conditions for exact aplanatism and after the third-order equations for anastigmatism and flat-field are satisfied,

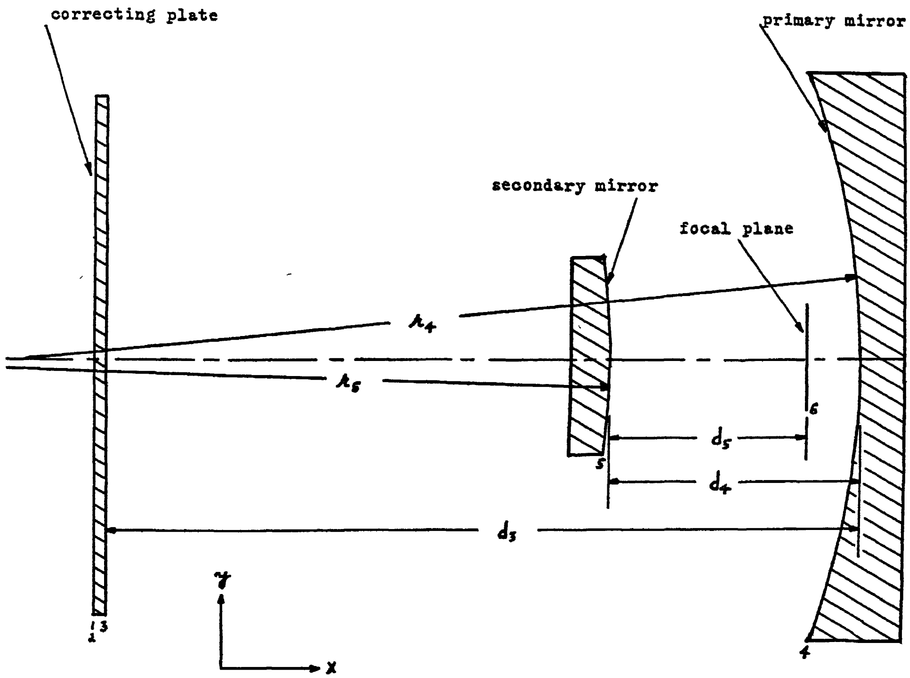

FIG. 1. Cross-section of general member of the three-component systems.

there remain two free parameters. These two parameters are the distance of the correcting plate from the primary mirror (d_3), and the distance of the photographic plate from the secondary mirror (d_5). All of the constants of the system can be expressed as simple rational functions of the two parameters. In all the following equations the equivalent focal length is taken as unit length. In order to compute the constants of a given system, one must first decide upon the value of d_5. Then the distance between the two mirrors is given by

$$d_4 = (1 - d_5)^2. \tag{1}$$

The two mirrors have the same tangent spheres at their vertices, so that

$$r_4 = r_5 = -2(1 - d_5), \tag{2}$$

where r_4 and r_5 are the radii of the corresponding tangent spheres. Also

$$A = \alpha_1 + \beta_1 S_5 + \gamma_1 S_4, \tag{3}$$

$$B = \alpha_2 + \beta_2 S_5, \tag{4}$$

$$C = \alpha_3 + \beta_3 S_5, \tag{5}$$

$$D = \alpha_4 + \beta_4 S_5, \tag{6}$$

where

$$\alpha_1 = -\frac{1 + 2d_5 - d_5^2}{8(1 - d_5)} \quad \beta_1 = +8d_5^4 \quad \gamma_1 = -8$$

$$\alpha_2 = \frac{6 + 6d_5 - 12d_5^2 + 3d_5^3}{8(1 - d_5)} \quad \beta_2 = +24d_5^3(1 - d_5)^2$$

$$\alpha_3 = -\frac{3}{2} - \frac{3}{2}d_5 + \frac{15}{8}d_5^2 - \frac{3}{8}d_5^3 \quad \beta_3 = +24d_5^2(1 - d_5)^4$$

$$\alpha_4 = \frac{10 + d_5 - 13d_5^2 + 7d_5^3 - d_5^4}{8} \quad \beta_4 = +8d_5(1 - d_5)^6.$$

The quantities A, B, C, and D contain the two unknowns S_4 and S_5. These unknowns have the following significance. In the coordinate system of Fig. 1 let the equation of the ith surface expanded about its vertex be

$$x_i = a_i y_i^2 + b_i y_i^4 + \cdots. \tag{7}$$

Then $a_i = \dfrac{1}{2r_i}$, where r_i is the radius of the sphere tangent to the surface at the vertex. If the surface is spherical, $b_i = a_i^3$. The quantity S_i represents the departure from a sphere such that for an aspherical surface the expansion runs

$$x_i = a_i y_i^2 + (a_i^3 - S_i)y_i^4 + \cdots \tag{8}$$

and

$$b_i = a_i^3 - S_i. \tag{9}$$

In order to evaluate S_4 and S_5, the following equations must be satisfied.

$$C + Bd_3 = 0, \tag{10}$$

$$B + 3Ad_3 = 0. \tag{11}$$

Thus, for each value of d_3, one obtains particular values for S_4 and S_5. If distortion is also to be zero (a condition usually unimportant in celestial photography), then

$$D + Ad_3^3 = \tfrac{1}{2}, \tag{12}$$

so that even d_3 is determined. Finally,

$$b_3 = -\frac{A}{4(n-1)}. \tag{13}$$

Consideration of tube-length and of manufacturing difficulties identifies four important single parameter families of nearly equivalent performance in the solution of the above equations.

Case A. When the correcting plate is located very near to the secondary mirror, ($d_3 = d_4$), the tube-length can reach the very

TABLE I

Type	d_5	r_5	S_5	d_4	r_4	S_4	d_3	$b_3(n-1)$
A-1	0.1	−1.8	4.69393	0.81	−1.8	0.02572	0.9	0.09182
A-2	0.2	−1.6	1.80054	0.64	−1.6	0.04273	0.8	0.13281
A-3	0.3	−1.4	1.27045	0.49	−1.4	0.07289	0.7	0.19260
A-4	0.35	−1.3	1.19713	0.4225	−1.3	0.09672	0.65	0.23336
A-5	0.4	−1.2	1.19358	0.36	−1.2	0.13021	0.6	0.28472

low value of one-third of the focal length. The two mirror surfaces then depart beyond figuring distance from the spheres tangent at the vertices. The maximum curvature of the correcting plate is also quite large. Table I provides the details of this class of camera, except that the correcting plate has been placed at the Gaussian focus of the primary mirror ($d_3 = -\frac{1}{2}r_4$). The two mirror surfaces are more steeply curved than the tangent spheres and fortunately possess no point of inflection to complicate their manufacture further.

Case B. If the distance d_3 is fixed by the criterion that the secondary mirror shall be accurately spherical, one finds a practical solution in the form of a single parameter family of cameras, one of which is shown in Fig. 2b. The departure of the primary from a spherical surface is fortunately very small in comparison with the parabolization of a mirror of the same central radius of curvature. For example, a twelve-inch disk with a central radius of curvature of 36 inches departs from a sphere by less than three wave-lengths at the edge. Unfortunately, there is no conic of

revolution closer to the required surface than the sphere, so that zonal testing is required. On the other hand, the convex spherical secondary can be most conveniently tested by interference against the primary to which it is tangent at the center. The slight departure of the primary from a sphere is still inappreciable near the center of the mirror, unless the mirror is very large. Moreover, an intermediary camera between Case B and Case C below, but

TABLE II

Type	d_5	r_5	S_5	d_4	r_4	S_4	d_3	$b_3(n-1)$
B-1	0.1	-1.8	0.0	0.81	-1.8	0.000044	1.812077	0.041408
B-2	0.2	-1.6	0.0	0.64	-1.6	0.000212	1.639858	0.053549
B-3	0.3	-1.4	0.0	0.49	-1.4	0.000601	1.475033	0.068613
B-4	0.35	-1.3	0.0	0.4225	-1.3	0.000938	1.393661	0.077717
B-5	0.4	-1.2	0.0	0.36	-1.2	0.001428	1.312230	0.088273

very close to Case B, can easily be determined so that the secondary mirror is accurately tangent to the primary. The requirement through the third order equations is simply that $S_4 = S_5$. The slight correction required for exactness can be taken to make the mirrors part of the same surface of revolution. Table II contains the essential information for cameras of class B.

Case C. If the primary mirror is to be made precisely spherical, then the secondary in turn departs slightly from a sphere. The amount of aspherical figuring is approximately that of Case B, but is more difficult to test for exactness. Table III provides the

TABLE III

Type	d_5	r_5	S_5	d_4	r_4	S_4	d_3	$b_3(n-1)$
C-1	0.1	-1.8	-0.014838	0.81	-1.8	0.0	1.815254	0.041323
C-2	0.2	-1.6	-0.015259	0.64	-1.6	0.0	1.648485	0.053174
C-3	0.3	-1.4	-0.016782	0.49	-1.4	0.0	1.488732	0.067683
C-4	0.35	-1.3	-0.018041	0.4225	-1.3	0.0	1.409451	0.076383
C-5	0.4	-1.2	-0.019728	0.36	-1.2	0.0	1.329729	0.086427

essential information for this class of camera. Case C differs so slightly from Case B that Fig. 2b well represents the general appearance of both types.

Case D. If distortion is to be made zero, then the distance d_3 is completely determined along with the aspherical figures of the mirrors. Table IV contains the essential information for this class of cameras. Distortion is of little consequence astronomically.

Cameras of this type may prove useful as landscape cameras of high magnification and great illumination. Because of the high correction possible for distortion, they would also make useful aerial survey cameras. Fig. 2c portrays the camera arrangement.

For comparison purposes the usual form of Schmidt camera is presented in Fig. 2d. All cameras in Fig. 2 have the same equivalent focal length, unvignetted field, and light efficiency. Accordingly, the apertures of the new types are slightly increased over the Schmidt in order to compensate for the loss of light by the second mirror in the system. Each of the cameras in Fig. 2 is an effective $f/1.8$ and covers an unvignetted field of 8°.

In the cameras of Fig. 2, including the Schmidt, the best position for the diaphragm depends upon the relative sizes of the correcting plate and primary mirror. Since each system is fully

TABLE IV

Type	d_5	r_5	S_5	d_4	r_4	S_4	d_3	$b_3(n-1)$
D-1	0.1	−1.8	0.377557	0.81	−1.8	0.001217	1.731932	0.043678
D-2	0.2	−1.6	0.244141	0.64	−1.6	0.003815	1.505882	0.059974
D-3	0.3	−1.4	0.224573	0.49	−1.4	0.009240	1.302650	0.082253
D-4	0.35	−1.3	0.232143	0.4225	−1.3	0.013945	1.206259	0.096764
D-5	0.4	−1.2	0.249895	0.36	−1.2	0.020940	1.112195	0.114502

corrected in the aperture, and also for third order astigmatism, the position of the diaphragm depends on the residual higher order aberrations. Since these are small for the field permissible in the Schmidt and the new type cameras of small focal ratios, it becomes more important to choose the position of the diaphragm by the criterion of the largest unvignetted field at a given aperture. Such an arrangement serves to best advantage, for in cases where vignetting is unimportant the aperture can be increased and where vignetting is a serious defect (rarely the case) the aperture can be reduced without sacrificing more light than is necessary. The correct position of the diaphragm therefore depends upon the relative sizes of correcting plate and primary mirror selected in construction. In Fig. 2, the correcting plate is only slightly smaller than the primary, in order to allow for the slight divergence of the rays, and the diaphragm is located half-way between the correcting plate and the primary mirror. The secondary mirror must be of a size sufficient to accommodate all the light from the primary mirror. Of course one must consider the difficulty of making a

larger correcting plate than would be necessary were the diaphragm coincident with it (the usual rule), but that question can be answered only in individual cases. The other extreme position of the

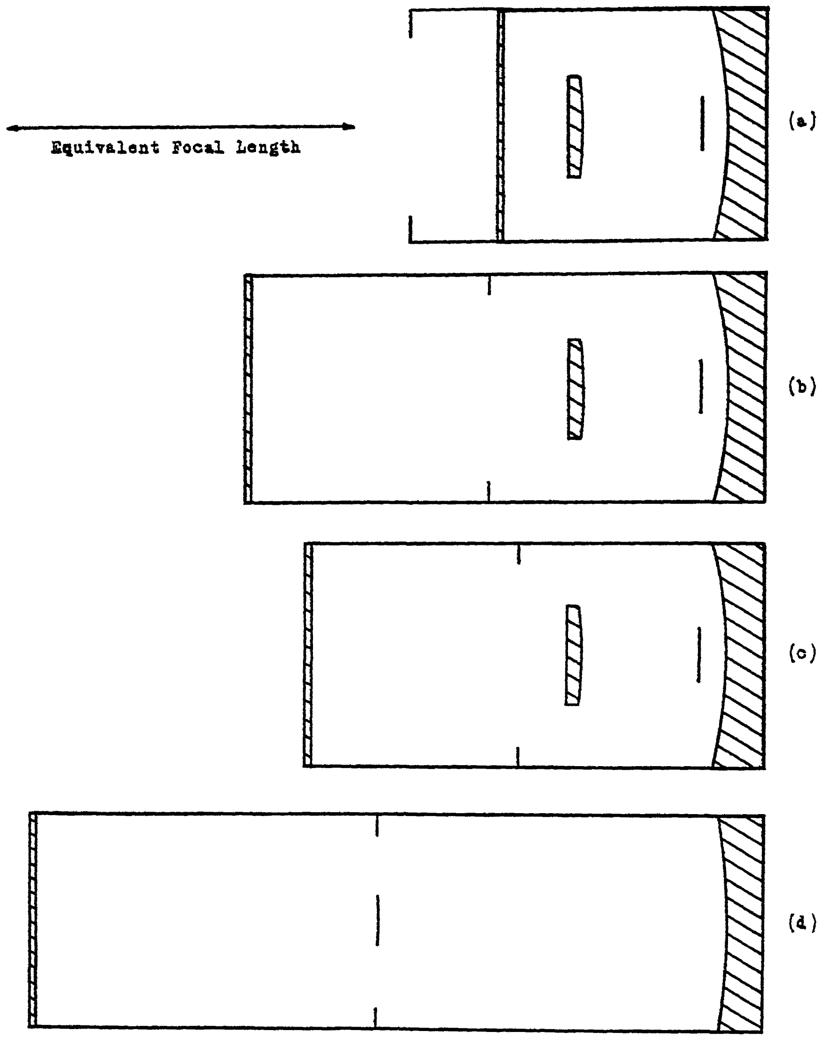

FIG. 2. Comparison of types A, B, C, and D with the Schmidt camera.

diaphragm is coincident with the primary mirror surface; for this arrangement the difference in diameter of correcting plate and primary mirror becomes twice the diameter of the photographic plate for zero vignetting. The requirements of any individual case are easily worked out graphically or by Gaussian optics to quite

sufficient accuracy. In cameras of type A, the position of the diaphragm is so determined as to eliminate direct sky fog on the photographic plate. The diaphragm need be only a slip cover on the tube of the camera and could help serve as a dew cap.

The correcting plate of each of the new type cameras is of higher curvature than the Schmidt of the same focal length and aperture. Consequently, the dispersion of the correcting plate causes a somewhat more serious color error than is true in the case of the Schmidt, depending upon the value of the coefficient b_3 in the tables. For the Schmidt camera $b_3\,(n - 1) = 0.03125$. As in the Schmidt, it is desirable to introduce a central bulge into the

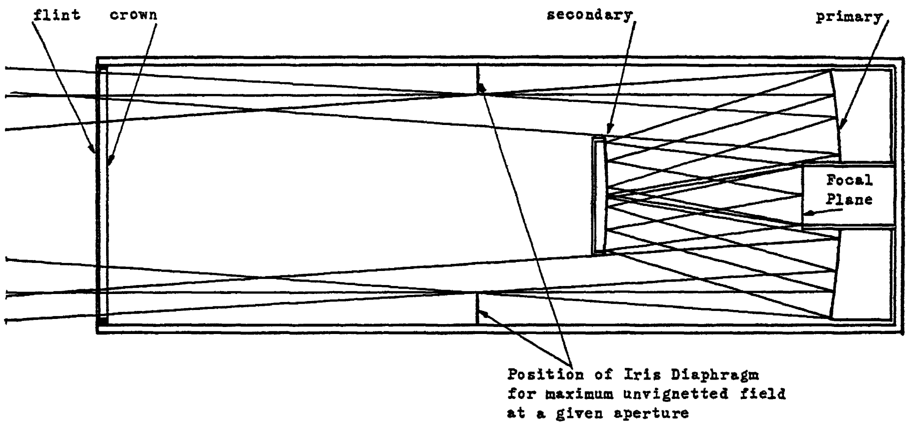

FIG. 3. Camera B-4. The important extreme rays of field and aperture illustrate the nature of the camera.

correcting plate so that the maximum slope of any part of the correcting plate is minimized. The equation for the new form of correcting plate is

$$x_3 = -\frac{3}{2}\,h^2 b_3 y_3{}^2 + b_3 y_3{}^4 + \cdots, \tag{14}$$

where b_3 is to be obtained from the tables.[6] The quantity h is the radius of the correcting-plate clear aperture. As in the case of the Schmidt the central bulge of the correcting plate, added to minimize color errors, reintroduces astigmatism of a very small amount.[7] In the new type cameras this residual astigmatism can again be made zero by a small variation of the constants of the system. The complete tables for all apertures and focal lengths will appear

[6] Strömgren, B., Wright, F., *op. cit.*

in a later communication. Tables I–IV are exact for a correcting plate without central bulge, with flat side facing the incident parallel light. The word exact here means complete annulment of third order aberrations.

The introduction of the bulge in the center part of the correcting plate in camera B has the effect of making the departure of the primary from a sphere even smaller. At approximately $f/1.5$ the primary becomes spherical through the third-order equations, but the fifth-order corrections make the numerical departure from the sphere still appreciable. In order to make any camera in Fig. 2 independent of the central thickness of the correcting plate, the plane side should be turned toward the incident parallel light.

For faster cameras and for cameras requiring excellent color correction, an achromatic correcting plate should be made. A later communication will give the necessary tabular material for the constants of the systems, both in terms of the kinds of glass employed, and, since the first surface of the correcting plate is no longer flat, of the central thickness of the correcting plate. A very transparent correcting plate can be constructed of Vitaglass, with a very thin section of flint glass cemented to one side and figured to the requisite curves. Even for the achromatic plate a central bulge is to be recommended in order to lessen manufacturing difficulties.

Both the Schmidt and new type systems are aplanatic and free of third-order image defects. The first error of importance to appear in either type is a fifth-order error called the variation of spherical aberration with angle. This error is more important to the aperture than to the field and therefore enters into consideration before the field errors in these types of fast cameras. For that reason the new type is equivalent in performance to the Schmidt camera. Where large fields and small apertures are concerned, as in cameras for high dispersion spectrographs, the Schmidt once again becomes superior because of its symmetry.

In order to illustrate the performance of the systems described above, the author has traced a number of rays accurately through camera B-4. The correcting plate was taken without central bulge for ease of computation. A differential correction made the system accurately aplanatic. Table V illustrates the small numerical corrections required to make the third-order results of Table II completely exact. A series of rays distributed around the aper-

TABLE V

y_3	Third-Order x_3	Exact x_3	Difference	y_4	Exact x_4	Difference Exact − Third-Order	Difference Exact − Sphere
0.00	0.0000000	0.0000000	0.0000000	0.00	0.0000000	0.0000000	0.0000000
0.04	0.0000004	0.0000004	0.0000000	0.04	−0.0006155	−0.0000000	−0.0000000
0.08	0.0000064	0.0000064	0.0000000	0.08	−0.0024639	−0.0000000	−0.0000000
0.12	0.0000322	0.0000325	0.0000003	0.12	−0.0055505	−0.0000000	−0.0000002
0.16	0.0001019	0.0001036	0.0000017	0.16	−0.0098843	−0.0000002	−0.0000006
0.20	0.0002487	0.0002554	0.0000067	0.20	−0.0154783	−0.0000012	−0.0000015

ture circle and incident at the angle of 4° (field of 8°) were traced through the system. The aperture was taken to be four-tenths of the focal length, and the diaphragm coincident with the correcting plate. For a focal length of 75 cm the maximum error in the image proved to be 0.023 mm as compared with 0.035 mm for the Schmidt camera of the same focal ratio and focal length. For the same light-gathering power the new types must be increased slightly in aperture over the Schmidt in order to overcome the loss of light by the secondary mirror. Accordingly, the image size of the new type is enlarged and becomes equal very nearly to the Schmidt of the same light efficiency. Thus, equivalent performance is assured.

Work has been begun on an $f/2.4$ camera of type B-4 with achromatic correcting plate and effective focal ratio of about $f/2.7$. The mirror and correcting plate will be 12″ in diameter. The image errors over a field of 7° will not amount to more than 0.01 mm anywhere.

The author is much indebted to Mr. C. D. Blake and to Mr. Chester Cook of the Boston Amateur Telescope Makers, who have enthusiastically undertaken the optical work, and to Dr. Shapley and Dr. Menzel of the Observatory who have furnished the inspiration for the entire optical program.

STATUS OF NUCLEAR THEORY[1]

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I WILL permit myself to begin by making the paradoxical statement that nuclear theory is older than the nucleus, or rather, older than the concept of the nucleus. This is because the nucleus contains almost the whole of the mass of the atom, so that everybody who has theorized about atomic weights has been theorizing about the nucleus whether or not he knew it—and this applies to many chemists and physicists of the nineteenth century, whereas the nucleus was first imagined in the twentieth.

I will now remind you of two cardinal facts, two cardinal properties of the chemical atomic weights which attracted their interest. They emerge just as clearly from a glance at the first ten or twelve of the elements as from a contemplation of them all. I write down the chemical atomic weights of these first twelve, beginning with oxygen, of which the weight is *by definition* sixteen of the chemical units. Including it among the other twelve, we have:

1. H	1.008		7. N	14.008	
2. He	4.002		8. O	16.000	(def.)
3. Li	6.940	6.017 7.018	9. F	19.00	
4. Be	9.02		10. Ne	20.183	19.998 21.998
5. B	10.82	10.016 11.013	11. Na	22.997	
6. C	12.01		12. Mg	24.32	23.992 24.993 25.990

The first cardinal fact is that seven of these twelve—not counting oxygen, of course—have values which are remarkably close to integers; and the second cardinal fact is the contrast between these and the other four, which have values clearly far from integral.

I will call this last the "contrast between integrity and non-integrity" and dispose of it right away, for it can easily be disposed

[1] Delivered before the American Philosophical Society at Philadelphia on November 18, 1939.

of. The discovery of isotopes explained it. I might say that the non-integrity of lithium and boron is a result of their duplicity, for each is a mixture of two isotopes. I should also have to speak of triplicity and other -icities as far up as ten or eleven, since some of the elements have many isotopes. Indeed many of these first twelve elements have three or more, but I have not bothered to indicate them all except in the case of magnesium, because they are mostly so rare as not to influence the atomic weight to any great extent. The weights of all these isotopes are close to integers, a rule illustrated by the examples given above to the right of the chemical atomic weights of several of the elements. We thus are left with the first cardinal fact: the closeness of atomic weights to integers, in all cases where they are truly the weights of single kinds of atoms.

This is the fact which caused Prout in 1815 to suggest that all atoms are clumps of hydrogen atoms. Now to give you this idea in its modern form, I must introduce the concept of the nucleus.

The nucleus of an atom of atomic number Z and mass-number A—the mass-number being the nearest integer to the actual weight—is pictured as a body of positive electric charge Ze and mass equal to the actual mass within a tenth of a per cent. (The tenth-of-one-per-cent is the sum of the masses of the surrounding orbital electrons, which we do not bother about.) The lightest nucleus of all is that of the lightest hydrogen isotope; its mass when it is free is about 1.008, it is called the "proton." Can we state Prout's idea in the form that all nuclei are clumps of protons? Certainly not! for take helium as an instance; if we tried to duplicate its charge of $+2e$ by putting two protons together the resulting mass would be only 2 instead of 4, while if we tried to duplicate its mass of four units the resulting charge would be $+4e$ instead of $+2e$. We must therefore assume that in all nuclei but the proton itself, there are protons burdened by an extra chargeless mass. It is simplest to suppose that this extra mass itself consists of particles of about the same weight as the proton. As I once wrote in this journal, these particles have lately been legitimized by the strongest procedure possible, that of setting them free and observing them rushing about in their freedom. The mass when free is indeed so close to that of the proton that even yet it is not sure which if either is the greater: I therefore put 1.008 for it. Their name, of course, is "neutron."

Thus the nucleus of atomic number Z and mass-number A—call it the nucleus (Z, A) for short—is conceived as a clump or cluster of A particles of mass 1.008, of which Z are protons and $(A-Z)$ are neutrons. Its mass must then—so one might pardonably think—be equal to A times 1.008. But actually, in every case which I have written down and in all the rest, the mass is so close to A times 1.000 that it is obviously and quite certainly less than A times 1.008. This mighty fact may seem quite fatal to the idea of the nucleus as a clump of protons and neutrons—but actually, as Einstein showed us in advance, it is just what is needed to make the idea work.

The point may be paraphrased as follows. If one assumes that a body is made up of particles one is perforce assuming also that the particles stick together. "Stick together" is of course a layman's term; a physicist will say that energy must be supplied to tear the particles apart. But *energy is always weighted down with mass*, and accordingly the mass of the system of particles must increase when they are torn apart. Reverse the process (in thought): when the particles come together and stick and form the body, its mass must be less than the aggregate of the masses which they had when free. Thus when the actual nucleus (Z, A) is found to be less massive than all of its particles considered separately, the fact is an indication of the forces which are holding the particles together.

The discrepancy or deficiency of mass is called the "binding energy of the nucleus." I want to improve the name, even though my improvement may never get beyond these pages: *the deficiency of mass is the binding lack-of-energy of the nucleus.* You notice that I am now using "mass" and "energy" as if they were synonyms. That is a habit one gets into when studying nuclear physics, and on the whole it is a sound one. As a corollary, one gets into the habit of using the units of energy or mass indifferently to apply to quantities which one has been regarding as the one or the other. Thus, we often say that the mass of the proton or neutron at rest is about one billion electron-volts; that of the electron, about half-a-million. To be slightly more exact, our unit of mass amounts to 931 millions of electron-volts (931 MeV); thus if a nucleus should have exactly an integer mass, its protons and its neutrons would have lost on the average $8\frac{1}{2}$ million eV apiece in the process of getting stuck together.

Can these deficiencies of mass, these binding lacks-of-energy, assist us in finding out the forces which hold the nuclei together? Well, at the very least they ought to serve for testing any laws of force which we may postulate. Of these laws there should be three: those of the proton-neutron, proton-proton and neutron-neutron forces respectively, or, as we say for short, the *np*, *pp* and *nn* forces. If we make assumptions as to these and then set out to calculate the masses of nuclei, the calculations are bound to be hard, and the chance of finishing them successfully is better the fewer the particles are. This is why experimenters and theorists alike have a very special interest in the masses of the lightest composite nuclei H^2, H^3, He^3 and He^4 comprising two, three, three and four particles apiece. But even for these the problem is anything but easy, and it is fortunate that we should have another way.

It is generally known how Rutherford established the nuclear atom-model by studying the deflections, or as we usually say the "scattering," of alpha-particles by atoms. The laws of scattering permitted and indeed they compelled the conclusion that at the centers of the atoms there are nuclei which act upon the alpha-particles by an inverse-square force. In this we recognize the ordinary force of electrostatic repulsion between bodies with charges of identical sign. Now as I have hinted already, it is feasible to form beams of fast-moving protons, and possible also (though more difficult) to make beams of fast-moving neutrons. May we aspire to discover the *np*, the *pp*, and the *nn* forces by arranging for these beams of protons and neutrons to be scattered by neutrons or protons?

The experiments are very hard, but not impossible save in the third case. The scattering of neutrons by neutrons is still beyond our grasp, and what I shall say later about the *nn* forces is deduced from the masses of nuclei. Beams of protons or of neutrons may however be projected into gaseous hydrogen, where some of them are deflected through passing close by the nuclei in hydrogen molecules. These nuclei being themselves protons, the *pp* and the *np* forces are concerned in the scattering of protons and neutrons respectively, and become available to us for study. The importance attached to them by physicists I can most forcefully convey, to those who know the cost of publication in science, by one remark: the *Physical Review*, which can hardly ever be persuaded to give a dozen pages to one article, gave twenty to the description

and forty-six to the analysis of just one such experiment. I will add that in these researches the protons derive their energy from cyclotrons or high-voltage machines, the neutrons are released from transmutations usually caused by similar engines; here accordingly we have a third use for these novel types of apparatus to be set beside their use in transmutation and their therapeutic value.

The experiments tell us that there are powerful short-range forces acting between proton and proton, and also between proton and neutron. You may feel that we should have guessed this already, since obviously such forces are just what are needed to hold the nuclei together. However, our notion that nuclei are composed only of protons and neutrons is not a self-evident fact, but merely an assumption. As an assumption it is very much strengthened by the fact that these *np* and *pp* forces manifest themselves in scattering, and the strengthening is welcome. Also the experiments give us information about the range and magnitude of these two forces.

As to the range I will say only that the forces are practically confined to within $5 \cdot 10^{-13}$ cm between the centers of the particles. These therefore must huddle together so closely as that in order to hang together; our knowledge of the size of nuclei confirms this consequence.

As to the *relative* magnitudes: you will often see in the recent literature the statement that the *np*, *pp* and *nn* forces are all equal: or rather, that the three "interactions" are all equal, "force" being a word which has fallen out of fashion in nuclear physics. This is poorly expressed, and I will try to improve the statement both for its own sake and because I shall thus be able to introduce the concept of "spin."

Many clever experiments have shown that protons, neutrons, and many composite nuclei have angular momentum: we must picture them as incessantly spinning. This property of spin is "quantized," which means that it has features so curious that they would be incredible if they had not been demonstrated. Of these I will mention three.

First, the spin of any body simple or composite must have one or another of certain specific values; both the proton and the neutron have the lowest (excepting zero) of these permitted values; I must therefore denote the spins of both by equal short arrows.

Second, when a neutron and a proton approach one another they automatically set themselves in either of two positions—either with their spins pointing oppositely or with their spins pointing in the same direction, $\rightleftarrows$ and $\rightrightarrows$, the "anti-parallel case" and the "parallel case." Therefore there are two laws of force or interaction, one for the anti-parallel and one for the parallel case.

But third, when proton approaches proton or neutron approaches neutron, the parallel case is mysteriously prevented—seemingly just because the particles are alike. For these, then, only the law of force for the anti-parallel case ever comes into operation.

Now what the experiments seem to be telling us is this: in the anti-parallel case the interactions *np* and *pp* and *nn* are all about the same, and yet on the average neutrons and protons are much more strongly attracted by their opposites than by their mates, because very often they approach one another in such a way as to present the parallel case and suffer the interaction peculiar thereto, which is by far the strongest of the four:

$$\underset{np}{\rightrightarrows} \gg \underset{np}{\rightleftarrows} = \underset{pp}{\rightleftarrows} = \underset{nn}{\rightleftarrows}$$

Of course the equality-signs don't mean very much until you are told just how exact the conclusions are, and it would take a long and searching analysis to do that. I might mention that from the latest work it is inferred that the *np* and the *pp* interactions differ by 2 per cent, and if this is so, then by calling them equal one is making a remarkably small mistake.

May a neutron and a proton stick together? Evidently so, for this is just how the deuteron—the H^2 or heavy-hydrogen nucleus—is constructed. An experiment which I cannot here describe shows beyond a doubt that in the deuteron, the spins of neutron and proton are parallel: it is the "parallel interaction" which is holding them together. Deuterons with anti-parallel spins of the component particles have not been found nor pairs of protons hanging together without a neutron, nor pairs of neutrons hanging together without a proton. These are special cases of the more general rule that all known nuclei (the solitary proton and the solitary neutron alone excepted) are composed of both protons and neutrons in almost equal proportions. The anti-parallel interaction thus appears to be too feeble to hold a nucleus together,

and there must be dissimilar particles to bring into play the other and stronger one.

But why should the anti-parallel interaction be unable to hold the particles together, since it is strong enough to be detectable in the scattering experiments? and why indeed should the parallel and the anti-parallel forces together not be able to go on hooking particle after particle into a nucleus, until it swells so large that all the protons and all the neutrons in the world are gathered into it? We may have to invent something new to explain this, but at present it is thought that we can lay it all to the account of the ordinary electrostatic forces of repulsion between proton and proton. You may have been bothered by the fact that I did not earlier mention these, but now at the end I will make up for my neglect. They are revealed by the experiments on scattering, and they have their normal character of inverse-square, that is to say of *long-range* forces. The interactions of which I have thus far spoken are, as you will recall, short-range forces. Therefore as protons and neutrons are heaped together into a compact nucleus, each of them can attract only its nearer neighbors, but every proton can repel all the other protons. One readily sees that this must set a limit to the size of nuclei, and this is believed to be the very limit set by Nature, to wit, the 92 protons and 147 neutrons of the largest nucleus known.

THEORY OF LIGHT ABSORPTION IN SIMPLE AROMATIC COMPOUNDS

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THE problem of the relation of the absorption spectrum of organic compounds to their molecular structure is very important from several viewpoints: one wants to know how to put a molecule together to give it a certain color—how to make a dye—and conversely how to use the spectrum analytically as a clue to the structure, in organic chemistry and even more so in biochemistry.

The chemists have developed empirical rules, but no detailed explanation of these rules from the standpoint of theoretical physics existed up to recently. It was our purpose to find this explanation and that implied that we should try to calculate the absorption spectrum from the structure of the molecules.

The chemists have found that a molecule to be a dye has to have certain carbon skeletons involving conjugated double bonds (structures called chromophores) but that the color could be deepened by introducing certain substituents into the molecule (called auxochromes). We wanted to know what these auxochromes did, whether they shifted bands more toward the visible, or strengthened weak bands or created completely new bands. As most of the dyes seemed to contain aromatic rings, we decided that for a systematic investigation it would be best to start with the simplest aromatic compound, benzene C_6H_6, and then proceed to investigate the effect of substitutions. As a matter of fact, it turned out that this was a bad guess in so far as strongly colored substances in general are concerned. In the meantime, Mulliken [1] has shown that in some strong plant dyes like carotene an aliphatic chain with alternate double bonds is responsible for the color, and Pauling has attacked boldly very complicated compounds like aurin.[2]

[1] R. Mulliken, *Journ. Chem. Phys.*, 7, 14, 20, 121, 339, 353, 356, 364 (1939). The papers subsidized by the grant from the American Philosophical Society are marked with a star (*).

[2] L. Pauling, *Proc. Nat. Ac. Sc.*, Nov. 1939. Gilman's *Organic Chemistry*, Vol. II, p. 850, New York, 1938.

Nonetheless, we think that much can still be learned from an investigation of simple molecules.

Color is determined by the position and strength of the absorption bands; if there is an absorption band in the blue, the substance will be yellow, deeply yellow if the absorption band is strong, slightly yellow if it is weak. An absorption occurs if a quantum of the light to be absorbed has just the right energy to excite a molecule from the unexcited or ground state to an excited state, the strength of the absorption depending on a quantity called "transition probability."

The problem resolves itself therefore to a calculation of the different possible electron states of the benzene molecule. This problem has been attacked repeatedly.[3]

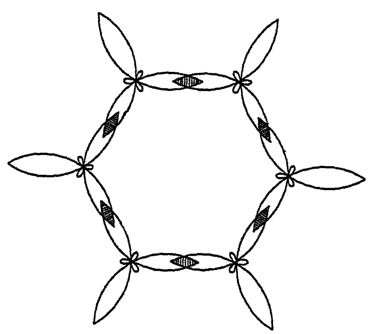

FIG. 1 (according to Sklar). In-plane bonds in benzene, C_6H_6. Charge distribution for bonding electrons.

The benzene molecule forms a ring of six carbon atoms, with six hydrogen atoms outside, so that one carbon atom is linked to two other carbon atoms and one hydrogen atom. A hydrogen atom has one electron, each carbon atom four. To make each normal link, 2 electrons are needed, so that the H – C link takes one electron from H and one from C, each C – C link one electron from each of the bound carbons (Fig. 1); that leaves one electron from each carbon atom free; these six electrons are responsible for the aromatic properties, while the other 24 behave like electrons in simple aliphatic compounds which only absorb in the far ultraviolet.

The next task set is to find the charge distribution of these six

[3] W. Heitler and F. London, *Z. f. Ph.*, **44**, 455 (1927); L. Pauling, *Journ. Am. Chem. Soc.*, **53**, 1367 (1931); J. C. Slater, *Phys. Rev.*, **37**, 481 (1931), **38**, 1109 (1931). For more literature see L. Pauling and E. B. Wilson, *Introduction to Quantum Mechanics.*

"unsaturation" electrons; this is accomplished by calculating with the help of the quantum theory a function ψ, the square of which gives the average charge density. However, the problem is so complicated that approximations must be used.

One starts out by finding ψ as it would be on a single carbon atom, if the other atoms had no effect on the particular electron. It is then found that the electron is in a so called $2\pi p$ state; that means that the average charge distribution is of the shape of a dumbbell, the middle of which is centered in the carbon atom considered and the axis of which is normal to the ring (Fig. 2).

We have next to consider the interaction energy of these six "unsaturation" electrons. Two methods exist for that purpose. The first is called the Heitler-London-Pauling-Slater [3] (H. L. P. S.) method. In it, the interaction of the electrons is considered in

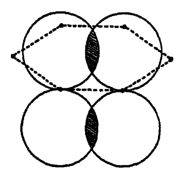

FIG. 2 (according to Sklar). Charge distribution for two unsaturation electrons forming a double bond. The charge distribution for each electron is of dumbbell shape, with the axis normal to the plane of the benzene ring. The part where the two charge distributions overlap is shaded.

pairs. But it is essential for aromatic compounds that for that pairing or "bonding" the electrons can be grouped in several ways. This phenomenon called "resonance" is responsible for the difference in chemical behavior of ethylene, which has only one such pair or double bond and is not very stable chemically, and benzene, which has three and is extremely stable.

This method, first applied by Hückel,[4] has been extended to the discussion of the optical properties by Sklar [5] with the results shown in Fig. 3 by B_1 and B_2. The latter includes the possibility that occasionally more than one unsaturation electron might sit on the same carbon atom, leaving another carbon atom bare (polar states). The dots in Fig. 3 represent the experimental results to be discussed later.

The second method, that of molecular orbitals, or Hund-Mulliken method, had also first been applied to benzene by Hückel,

[4] E. Hückel, *Z. f. Ph.*, 70, 204 (1931).

[5] A. L. Sklar, *Journ. Phys. Chem.*, 5, 669 (1933).

and was then used on it and improved by Goeppert-Mayer and Sklar.[6] This check, the results of which are found in Fig. 3*A*, was necessary because one can really trust such approximate calculations only if the two methods agree fairly well. In the following we will use the language of the molecular orbital method. It considers each electron, skipping so to speak from one dumbbell shaped distribution to the other all around the ring. There are six such

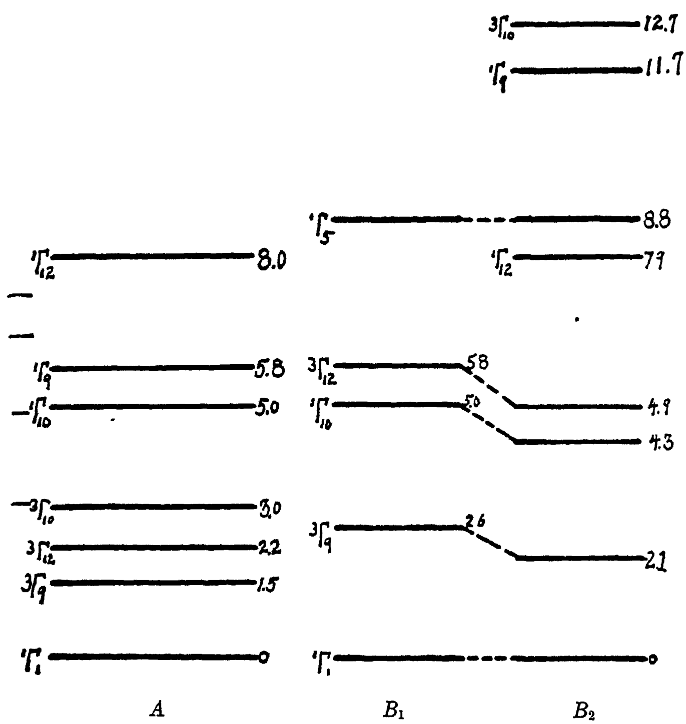

FIG. 3. Distribution of low lying energy levels according to Goeppert-Mayer and Sklar. The experimental values are indicated by dots at the left side.

possible orbits, characterized by the numbers 0, $+1$, -1, $+2$, -2, 3.

These numbers signify the angular momentum the electron has on its path around the ring in multiples of the Bohr unit $\frac{h}{2\pi}$. The $+$ and $-$ signify opposite directions of rotation (or rather of the angular momentum). According to the Pauli principle, not more than two electrons can go into every orbit. To take that into

*[6] M. Goeppert-Mayer and A. L. Sklar, *Journ. Chem. Phys.*, 6, 645 (1938)

account, Goeppert-Mayer and Sklar "antisymmetrized" the function ψ, which process guarantees the fulfillment of the Pauli principle. Then, in the normal or ground state, the electrons try to go into the states with lowest energy, which in this case means lowest angular momentum.

Therefore, in the ground state, there are two electrons each in the orbit 0, $+1$, -1, while $+2$, -2, 3 remain empty. Excitation consists in taking one electron out of a full state and putting it into an empty state.[7] It is necessary to calculate the interaction of the electron on its way around the orbit with all the charges in the molecule; that necessitated the calculation of lengthy tables.[8]

It turns out that the lowest excited state or rather the lowest two excited states result from taking one electron out of the orbital $+1$ and putting it into orbital -2 or out of the orbit -1 and putting it into $+2$ (these are marked ${}^1\Gamma_{10}$ and ${}^1\Gamma_9$ in Fig. 3*A*), while the next excited state is found by taking an electron from $+1$ into $+2$ or -1 into -2.

To the left in Fig. 3 are marked the theoretical excitation energies of 5, 5.8 and 8 volts, corresponding to absorption wave lengths of 2500Å, 2140Å, and 1600Å.

How does that compare with experiment? It was known that benzene had a weak band around 2500Å and a strong one in the region of 2000Å; the total absorption in the weak band being several thousand times less than in the D line of sodium vapor.

That suggests that the weak band around 2500 is a "forbidden band." The electric field of the light wave, to affect the molecule at all, must get hold of positive and negative charges at different ends of the molecule, or to be more precise, change the "electrical dipole moment." The ground state has the complete symmetry of a hexagon while ${}^1\Gamma_{10}$ has the symmetry of an equilateral triangle and therefore also no dipole moment. That this absorption is not completely absent is due to the fact that the molecule is not at rest, but vibrates. The benzene molecule has 20 different modes of vibration (Fig. 4).[9] Of these, the ones marked B_{2u} distort the molecule in such a way that if they are present either in the normal or the excited state, a transition can be induced by the light. The

[7] Of course it is possible to put an electron even out of the dumbbell distribution. This, however, amounts to the excitation of a carbon atom and needs much more energy, *i.e.* the absorption is in the extreme ultraviolet.

* [8] A. L. Sklar and R. H. Lyddane, *Journ. Chem. Phys.*, **7**, 374 (1939).

[9] E. B. Wilson, *Phys. Rev.*, **45**, 706 (1934); C. K. Ingold, *Proc. Roy. Soc.*, **169**, 149 (1938).

totally symmetrical vibrations are without effect and therefore may or may not be present. A detailed analysis of the benzene spectrum near 2500 can therefore help to prove the correctness of the calculated electronic state. It has been undertaken by Sponer, Nordheim, Sklar, and Teller,[10] based on measurements made by W. Radle in a Catholic University dissertation not yet published.

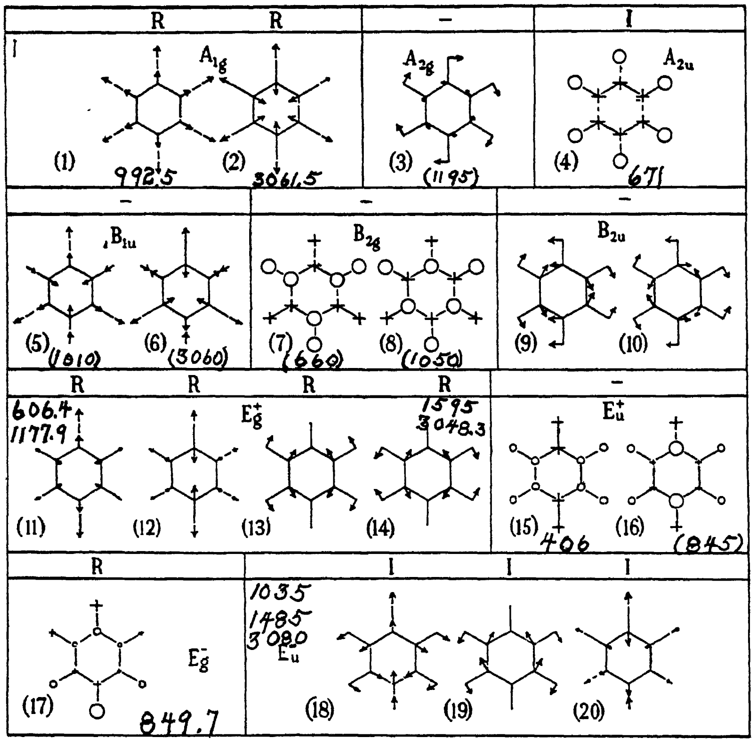

FIG. 4. Vibrations of the benzene molecule according to Ingold. The symmetries and wave numbers are indicated.

In it practically all strong bands, more than 70 in number, have been explained (Fig. 5).

On the other hand, the transition to the state marked $^1\Gamma_{12}$ is allowed and the band should be strong. However, the calculated wave length 1600Å is shorter than the observed one. It is possible (and there are even experimental indications for it) that the measured band is a superposition of $^1\Gamma_{12}$ and the lower lying $^1\Gamma_9$.

In the figure, there are three levels lying even lower than $^1\Gamma_{10}$ and marked $^3\Gamma_9$, $^3\Gamma_{12}$ and $^3\Gamma_{10}$. They are triplet levels, that is, the

* [10] H. Sponer, G. Nordheim, A. L. Sklar and E. Teller, *J. Chem. Phys.*, 7, 207 (1939).

spin of one electron (which makes it act like a little magnetic needle) is turned around, while in the singlet levels discussed before the spins of the electrons are pair-wise in opposite directions. Now transitions from triplet to singlet (intercombination lines) are al-

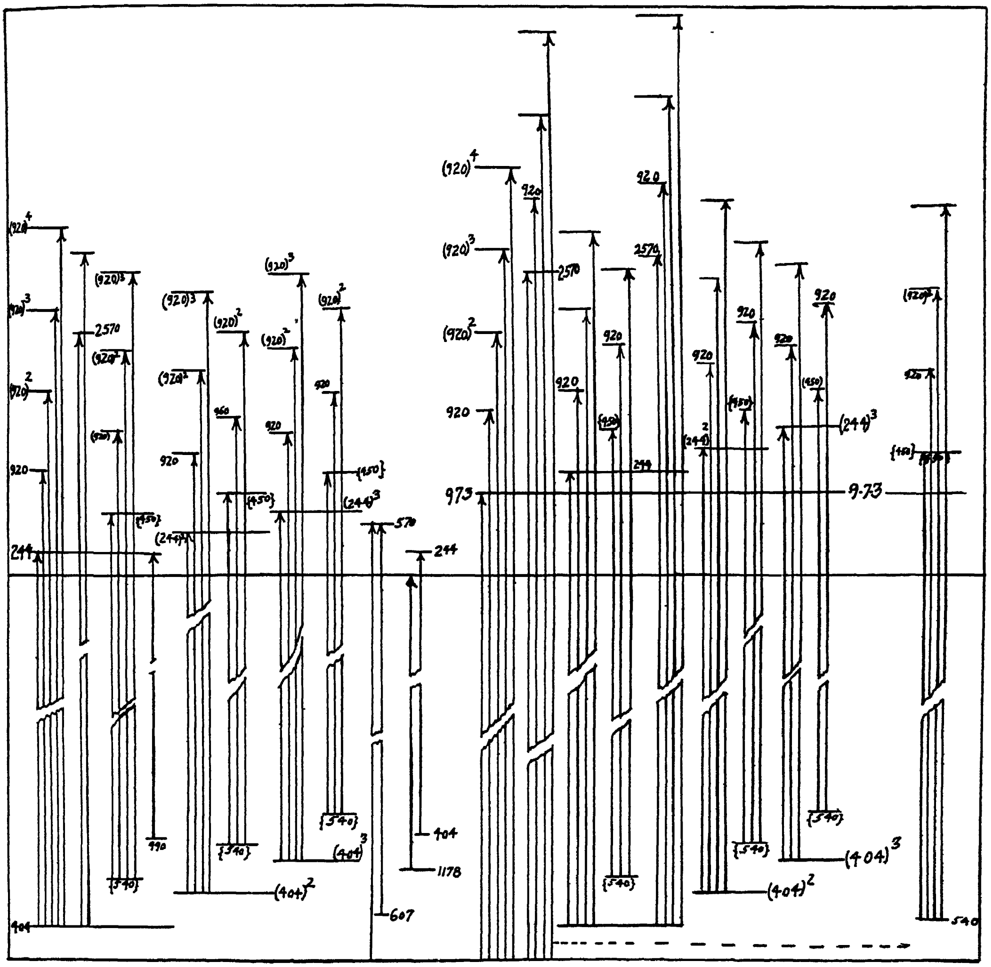

FIG. 5. Interpretation of the absorption bands of benzene vapor near 2500Å (according to Sponer, Nordheim, Sklar, and Teller).

ways weak. Dr. Sklar[11] has, however, found a new absorption band at 3500Å (3,5 volts), so weak that it only comes out in 10 inches of liquid benzene, which is probably the transition from the ground state to $^3\Gamma_{12}$.

[11] Unpublished.

Next comes the question of the influence of substituting another group for one hydrogen atom. The following groups have been investigated:[12] CH_3, F, OH, and NH_2. Of these OH and NH_2 strengthen the weak band at 2600 appreciably, CH_3 and F do not. It turns out that the effect is due to the partial transition of an electron from the substituent into one of the empty orbitals of the ring. ("Partial transition" can be interpreted so that the electron spends part of the time there.)

The ground state is now one with six electrons in the orbitals 0, +1, −1, and part of an additional electron in 2; while the excited state has two electrons in 0, 3 in +1 and −1 together, one in +2 or −2 and part of a seventh distributed over 1, 2 and 3.

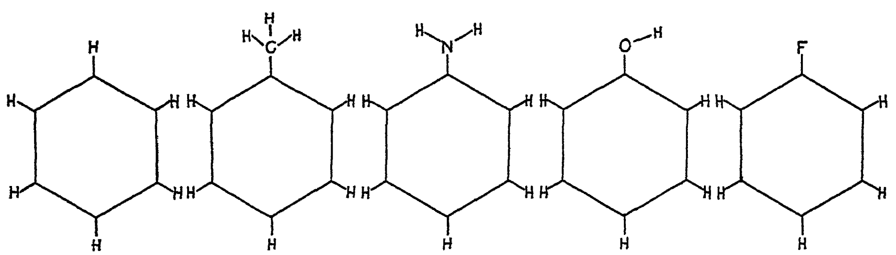

FIG. 6. Monosubstituted benzenes.

Because of this additional electron, the transition becomes allowed to the extent of its (the electron's) presence in the ring.

The problem then becomes the calculation of the extent to which an electron of the substituent is drawn into the ring. This extent increases the closer the substituent comes to the ring (overlap of wave functions). Furthermore, it always takes energy to take the electron from the substituent and put it into the ring. This amount of energy is equal to the difference of electron affinities of substituent and ring, and the smaller this difference is the more migration takes place.

In addition only dumbbell shaped distributions (πp electrons) can migrate into the ring, and if the axis of the dumbbell is inclined toward the ring normal, the migration becomes less.

The following table shows the strength of the absorption bands in units of one ten-thousandth of the absorption in the D lines of

* [12] A. L. Sklar, *J. Chem. Phys.*, 7, 98 (1939).

as many sodium atoms as there are benzene molecules (oscillator strength):

	f_{theor}	f_{exp}
$C_6H_5CH_3$	5	8
$C_6H_5NH_2$	140	150
C_6H_5OH	60	120
C_6H_5F	3	6 (C_6H_5Cl)

It is to be pointed out that the measurements [13] have been made in solution, while the theory applies to the gas.

The reason for the high increase in absorption strength in amine and phenol is the low electron affinity of $-O-$ and $-N<$; the intensity for NH_2 would be even higher if the pyramidal shape of the group did not cause an inclination of the dumbbells in respect to the ring. F has very high electron affinity, while in CH_3 the electrons are taken up by the $C-H$ bonds, strongly bound and far away.

The case of double substitution is taken up in a paper not yet published. Again the effect of the groups is a migration of electrons into the ring with a corresponding perturbation of symmetry. The two groups can be 60° (ortho), 120° (meta), 180° (para) distant from each other. It turns out that the effect of two groups can be treated as if the coefficient giving the migration of each group were a vector. One then puts the vectors of the two groups together under the angle 2χ ($\chi = 60, 120, 180$) and the vector sum measures the migration in the combined groups.[14] The intensity is proportional to the square of the migration. If one has two similar groups, the enhancement of the intensity for monosubstituted to ortho to meta to para $= 1 : 1 : 1 : 4$.

For two groups of similar strength but opposite sign the same ratio is $1 : 3 : 3 : 0$.

For weak groups, $-Cl$, $-CH_3$, $-C = N$, $-N = C$, these results are in reasonably good agreement with experiment. Not so for strongly effective groups like $-OH$ or $-NH_2$, where obviously a first approximation as used here is not sufficient.

[13] K. L. Wolff and K. Herold, *Z. f. phys. Chem.*, **B13**, 201 (1931).

[14] A similar rule has been found empirically for the change in frequency of the absorption bands in disubstituted benzenes by H. Conrad-Billroth, *Z. f. phys. Chem.*, **B20**, 227 (1933), **25**, 139, 217 (1934).

TIME VARIABILITY OF HYDROGRAPHIC ELEMENTS DETERMINING THE DYNAMIC SITUATION IN THE WESTERN NORTH ATLANTIC[1]

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Abstract

In 1936, 1937, and 1938 repeated temperature (and salinity) measurements over periods of 57 to 314 hours, at fixed depths (measured from the physical sea surface) in the western North Atlantic have revealed that, because of significant daily and day by day changes in the observed state of the sea, a measure of average conditions is not generally obtained from isolated individual samplings of the water column; a circumstance which reduces the significance of detail in patterns of geographic variation of oceanographic properties (as deduced from data of station networks). Remarks on this phenomenon in the Atlantic basins by various authors have recently been summarized by Seiwell[2] and are dispensed with here. In this paper it is proposed, first, to present a resumé of the results of repeated temperature measurements at three "Atlantis" Stations (2886, 2887, 3091) in the western basin of the North Atlantic, and, second, to apply the results of one of these (2887) to illustrate the effect of short period variations on dynamic computations of the state of the western Sargasso Sea and of the Gulf Stream system off the American coast.

Contents

[1] Supported by a grant from the American Philosophical Society. Contribution No. 62 from the Woods Hole Oceanographic Institution.

[2] The Effect of Short Period Variations of Temperature and Salinity on Calculations in Dynamic Oceanography. Papers in *Physical Oceanography and Meteorology*, Vol. VII, No. 3, 1939.

Short Period Temperature Variations at Fixed Depths and Related Vertical Displacements in the Western Sargasso Sea

Station Data

Results of repeated temperature measurements to be discussed were obtained from "Atlantis" stations located in the western Atlantic basin between Bermuda and the Gulf Stream system, where, for considerable distance, in all directions, horizontal gradients of oceanographic properties are small. The temperature observations comprising a single sampling of the water column were made almost simultaneously at 18 levels between surface and 1200–1400 meters by using 18 pairs of reversing thermometers on a single wire; the time between successive samplings (comprising 18 items each) was about 60 minutes. At the stations discussed "Atlantis" was not anchored, but during operation, frequent observations of ship's drift (and favorable weather) enabled samplings to be made generally within a radius of 2 to 3 miles (in no case exceeding 5 miles) from the mean station positions tabulated in Table 1. Mean geographic positions of Stations 2887 and 3091 are nearly identical, both being located about 110 miles northwest of Bermuda; Station 2886 (about 100 miles to the south) is approximately 60 miles west of Bermuda.

TABLE 1

Station	λ	φ	Depth	Duration of Sampling 60° Time	Number of Samplings
2886	65° 59′ W	32° 19′ N	4830	$23^h\,46^m$, June 16–$08^h\,28^m$, June 19, 1937	50
2887	65° 56′ W	34° 01′ N	5216	$23^h\,16^m$, June 19–$13^h\,34^m$, June 22, 1937	41
				$07^h\,57^m$, June 24–$08^h\,24^m$, June 28, 1937	72
3091	65° 54′ W	34° 02′ N	5133	$12^h\,21^m$, June 16–$14^h\,22^m$, June 29, 1938	194

To reduce observations for analysis, individual temperature depth curves for each sampling were plotted and scaled both for temperatures at standard depths and for depths of standard isotherms; the scaled items being consolidated into averages, or used individually, as required. The density distributions at each station were determined by combining average temperatures at

standard depths and average depths of standard isotherms with salinity values scaled from the individual temperature salinity relationships.[3]

Daily and Total Temperature Ranges at Standard Depths ("Atlantis" Stations 2886, 2887, 3091)

Total temperature ranges [4] at standard depths (Table 2) were between 0.08° and 0.76° during $56^h\ 42^m$ of observation at Station 2886, between 0.50° and 2.71° during $158^h\ 45^m$ at Station 2887,

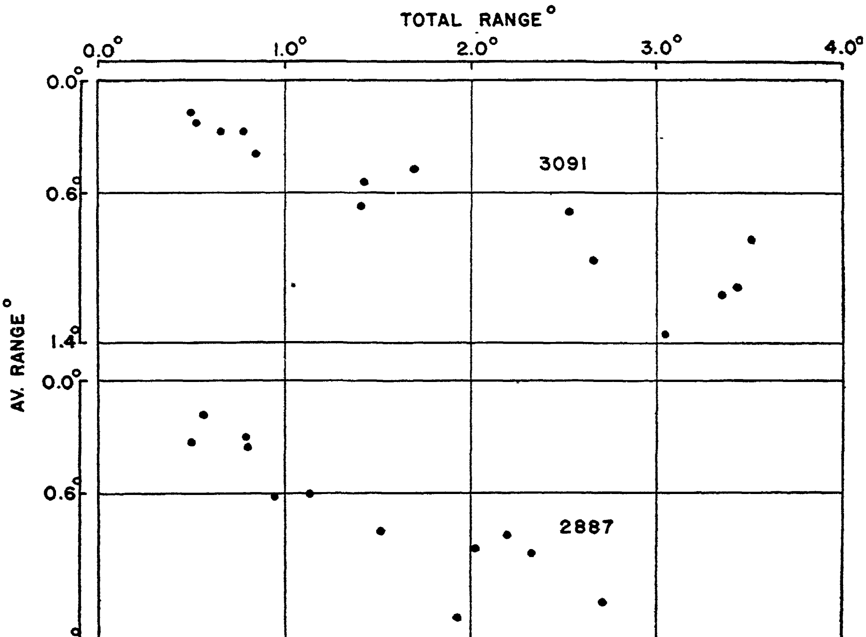

FIG. 1. Relationship. Total temperature range to average daily temperature range at standard depths for Stations 2887 and 3091, scales in degrees centigrade.

and between 0.50° and 3.52° during $314^h\ 01^m$ at Station 3091. The average daily (24 hour) temperature ranges [5] for these same stations were between 0.08° and 0.65° at 2886, between 0.18° and 1.26° at 2887, and between 0.17° and 1.35° at 3091 (Table 2). Vertical variations in magnitude of both the above ranges are closely related; depths characterized by the larger total variations

[3] Salinity values were determined for approximately 15 per cent of the samplings, and, thence, combined with corresponding temperatures to form temperature salinity relationships for each station (see Seiwell, 1939, footnote 2).

[4] Maximum temperature minus minimum temperature for the period specified.

[5] Average of all daily ranges extending from midnight to midnight.

are similarly characterized by the larger average daily ranges and vice versa (Fig. 1). Individual daily temperature ranges entering into computation of the average daily temperature ranges are subject to wide variability; for instance, daily ranges at depths of the minimum average daily range varied between 0.04° and 0.55° at Station 2887, and between 0.06° and 0.26° at Station 3091; likewise, at the depth of the maximum average daily range individual

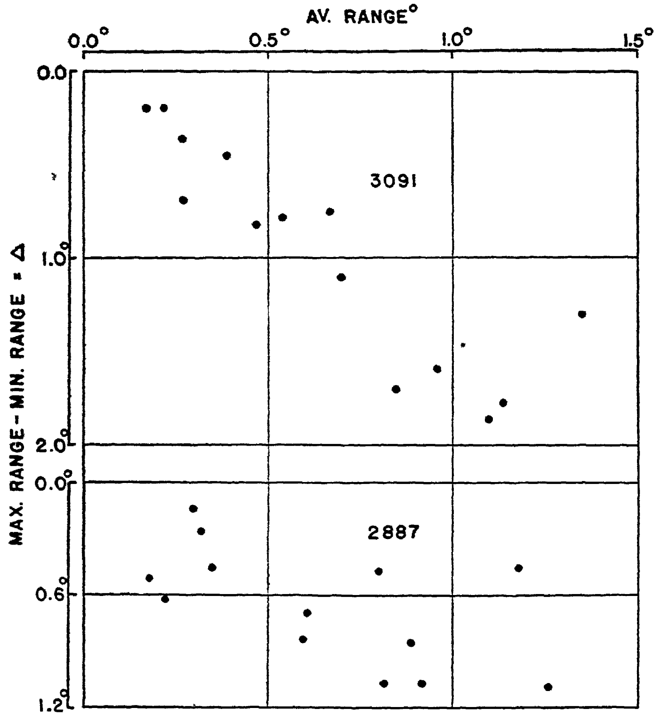

FIG. 2. Relationship. Average daily temperature range to difference between maximum and minimum individual daily temperature ranges (maximum range − minimum range = Δ) at standard depths for Stations 2887 and 3091.

daily temperature ranges were between 0.63° and 1.71° and between 0.90° and 2.20°, respectively. Greater differences in individual daily ranges (maximum daily range − minimum daily range) occur at depths characterized by the higher average daily temperature ranges, the general linear relationship between these statistics being illustrated by Fig. 2.

Other details of temperature variations at standard depths are given by Table 2, and examination shows that for any particular depth significant day by day variations occur in magnitude of the

TABLE 2

Depth	Average Temperature	Average Daily Temperature Range	Minimum Daily Temperature Range	Maximum Daily Temperature Range	Total Temperature Range
	Station 2886				
0	25.22°	—	—	—	—
50	—	—	—	—	—
100	18.71°	0.64°	0.59°	0.69°	0.74°
200	18.17°	0.08°	0.07°	0.08°	0.08°
300	17.99°	0.18°	0.17°	0.19°	0.19°
400	17.46°	0.13°	0.13°	0.13°	0.13°
500	16.83°	0.31°	0.30°	0.31°	0.39°
600	15.48°	0.47°	0.47°	0.47°	0.58°
700	13.51°	0.63°	0.62°	0.64°	0.76°
800	11.35°	0.65°	0.60°	0.69°	0.71°
900	9.03°	0.65°	0.60°	0.69°	0.69°
1000	7.19°	0.46°	0.27°	0.65°	0.65°
1100	5.87°	0.40°	0.38°	0.41°	0.41°
1200	5.19°	0.25°	0.21°	0.28°	0.28°
1300	—	—	—	—	—
	Station 2887				
0	24.55°	—	—	—	—
50	—	—	—	—	—
100	19.92°	0.89°	0.57°	1.42°	2.03°
200	18.27°	0.30°	0.18°	0.32°	0.79°
300	18.02°	0.32°	0.16°	0.42°	0.50°
400	17.53°	0.35°	0.10°	0.55°	0.80°
500	17.46°	0.18°	0.04°	0.55°	0.56°
600	17.13°	0.61°	0.24°	0.93°	0.95°
700	15.74°	1.26°	0.63°	1.71°	1.93°
800	13.56°	0.92°	0.36°	1.53°	2.33°
900	11.96°	0.82°	0.45°	1.51°	2.20°
1000	9.90°	1.18°	0.94°	1.39°	2.71°
1100	7.61°	0.80°	0.64°	1.11°	1.52°
1200	6.13°	0.60°	0.27°	1.10°	1.13°
1300	—	—	—	—	—
	Station 3091				
0	24.34°	—	—	—	—
50	20.08°	1.35°	0.90°	2.20°	3.05°
100	18.44°	0.54°	0.22°	1.01°	1.43°
200	17.91°	0.27°	0.08°	0.77°	0.78°
300	17.67°	0.17°	0.06°	0.26°	0.50°
400	17.38°	0.27°	0.15°	0.51°	0.66°
500	16.78°	0.67°	0.34°	1.09°	1.41°
600	15.51°	0.96°	0.36°	1.95°	2.66°
700	13.73°	1.10°	0.47°	2.33°	3.44°
800	11.65°	1.14°	0.73°	2.50°	3.36°
900	9.44°	0.85°	0.38°	2.08°	3.52°
1000	7.56°	0.70°	0.35°	1.45°	2.53°
1100	6.28°	0.47°	0.24°	1.06°	1.70°
1200	5.50°	0.39°	0.22°	0.67°	0.85°
1300	4.99°	0.22°	0.11°	0.31°	0.53°

temperature variations, and, that all minimum or maximum temperature variations (average daily ranges, variations in individual daily ranges, total ranges, etc.) occur at similar parts of the water column. The first characteristic, indicating that the magnitude

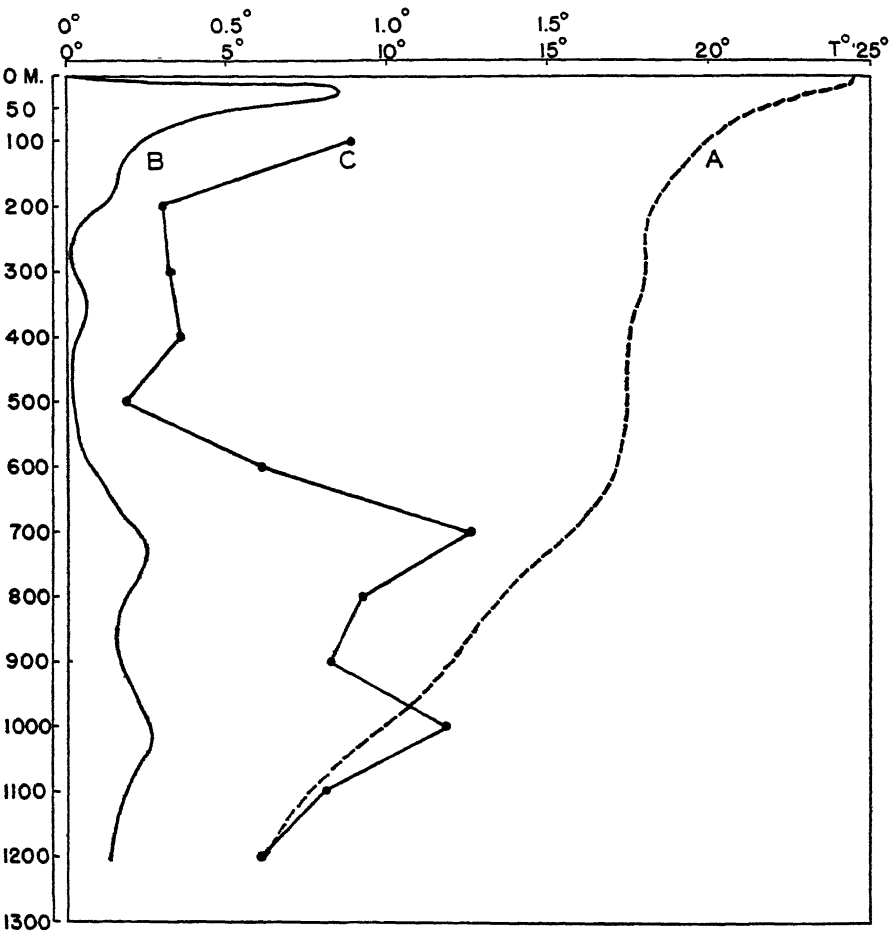

FIG. 3. Temperature variations during 6 days at Station 2887. A = average vertical temperature distribution; B = average vertical variation of temperature, $\frac{\Delta T^\circ}{\Delta z} \times 10^2$ degrees per meter (referred to uppermost scale); C = average daily temperature ranges at standard depths referred to uppermost scale.

of the disturbance of the water column varies from day to day, is discussed with reference to vertical displacements, whereas the second is further examined with reference to the vertical temperature stratification of the water column.

Daily temperature ranges are larger for depths where thermal stratification is greatest, a circumstance brought out for Stations 2886, 2887, and 3091 by Figs. 3 and 4. Conditions at all three stations are essentially similar, and, as illustrated for the two latter,

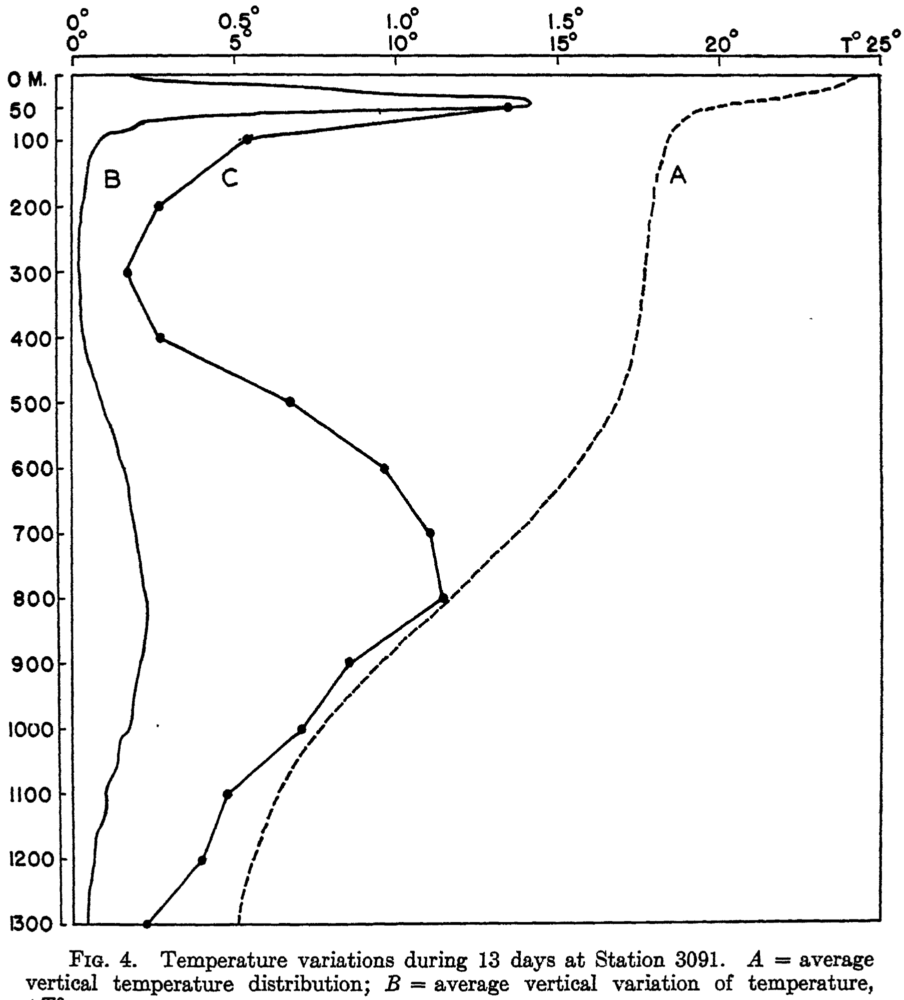

FIG. 4. Temperature variations during 13 days at Station 3091. A = average vertical temperature distribution; B = average vertical variation of temperature, $\frac{\Delta T^\circ}{\Delta z} \times 10^2$ degrees per meter referred to uppermost scale; C = average daily temperature ranges at standard depths referred to uppermost scale.

changes in magnitude of average daily temperature ranges are sensitive to small variations in thermal stratification. Thus, at Stations 2887 and 3091, between the 17° and 18° isotherms, where temperature (and density) changes slowly with depth, $\frac{\Delta T^\circ}{\Delta z} = 0.08°$

$\times 10^{-2}$ to 0.9×10^{-2} per meter, average daily temperature ranges were only 0.17° to 0.35°; in the overlying surface layer (in the vicinity of 20°–21°) where $\frac{\Delta T^\circ}{\Delta z}$ attains values up to $14.2° \times 10^{-2}$, average daily temperature ranges were up to 1.35°, while, in the intermediately stratified deeper water (between 17° and 6°), values of $\frac{\Delta T^\circ}{\Delta z} = 2.3 \times 10^{-2}$ to 2.5×10^{-2}, average daily temperature ranges up to 1.14°–1.26° were observed.

Likewise, total temperature ranges (for the different observation periods) and variations in individual daily temperature ranges will bear the same relationship to thermal stratification as the average daily temperature ranges. Thus, during the 13 days of observation at Station 3091, the smallest total variation of 0.50° occurred in the least stratified water at 300 meters depth and the largest of 3.52° occurred in the deeper more stratified water at 900 meters depth. Other corresponding relationships are brought out by comparison of Figs. 1, 2, 3, and 4; the minimum temperature disturbances occur where the water column is least stratified thermally, and, maximum where it is most strongly stratified.

Daily and Total Vertical Displacements of the Water Column ("Atlantis" Stations 2886, 2887, 3091)

To specify vertical displacements at particular depths in the water column, reference is made to various isotherms (average depths tabulated in Table 3), the apparent changes in positions of which (in the western Sargasso Sea) may be considered as resulting from vertical movements of the water particles, a circumstance deduced because of the small horizontal isothermal gradients.[6]

Total vertical displacement ranges of water particles as shown by standard isotherms were between 39 and 165 meters during $158^h 45^m$ at Station 2887 and between 38 and 227 meters during $314^h 01^m$ at Station 3091. These large vertical displacements result from short period vertical oscillations having periods of 24 lunar hours or less superimposed on longer period trends; characterizations of the latter, by fitting straight lines to the 12 daily

[6] For example, in this region the maximum gradient of 10° isotherm is 1.33 meters per kilometer, and of the 17° isotherm 1.00 meter per kilometer (see papers in *Physical Oceanography and Meteorology*, Vol. V, No. 2, page 17).

TABLE 3

Isotherm	Station 2886		Station 2887					Station 3091				
	Average Depth	Average Daily Range	Average Depth	Average Daily Range	Minimum Daily Range	Maximum Daily Range	Total Range	Average Depth	Average Daily Range	Minimum Daily Range	Maximum Daily Range	Total Range
20.00°	—	—	98	26	21	23	39	56	19	5	28	38
19.00°	84	40	150	25	12	35	41	79	25	11	55	82
18.00°	288	54	300	74	57	94	124	179	69	29	107	196
17.75°	—	—	—	—	—	—	—	260	81	40	106	227
17.50°	393	49	449	97	36	161	165	367	73	34	116	170
17.25°	—	—	—	—	—	—	—	438	83	39	142	159
17.00°	482	36	618	81	43	117	131	476	79	42	149	172
16.00°	567	30	687	70	51	97	129	566	67	27	144	172
15.00°	625	24	733	57	39	80	115	631	59	26	120	156
14.00°	676	29	776	54	27	74	117	686	53	24	103	144
13.00°	725	38	829	57	27	85	112	737	53	30	108	145
12.00°	772	32	899	49	41	67	98	784	53	30	110	155
11.00°	814	25	954	52	47	61	117	829	48	30	110	172
10.00°	856	29	995	47	39	59	106	873	43	21	111	170
9.00°	901	31	1035	47	34	62	98	921	40	18	106	162
8.00°	953	21	1079	44	28	63	96	974	42	18	103	157
7.00°	1012	30	1135	42	33	58	81	1039	47	21	101	172
6.00°	1085	46	1211	45	34	69	74	1131	53	24	110	168
5.00°	1241	48	—	—	—	—	—	1293	53	27	85	122

TABLE 4

Isotherm	$y=a+bx$		Average Depth June 17	Average Depth June 28
	a	b		
20.00°	59.26	−0.59	56.2	50.1
19.00°	88.05	−1.37	83.3	67.9
18.00°	235.30	−8.39	200.6	139.0
17.75°	324.16	−9.77	282.1	212.2
17.50°	360.97	0.85	345.2	386.0
17.25°	402.40	5.28	394.0	469.3
17.00°	433.61	6.26	427.4	506.1
16.00°	530.41	5.38	525.3	592.5
15.00°	598.58	4.82	596.5	657.9
14.00°	653.81	4.86	653.6	714.5
13.00°	705.00	4.77	706.8	765.4
12.00°	752.80	4.60	755.2	809.4
11.00°	799.71	4.28	802.5	851.3
10.00°	845.67	4.08	849.3	895.1
9.00°	894.26	3.89	898.7	941.6
8.00°	949.11	3.59	954.2	994.5
7.00°	1013.63	3.67	1016.7	1058.6
6.00°	1109.57	3.07	1110.1	1152.9
5.00°	1269.71	3.65	1288.9 (June 18)	1328.4

average depths of each isotherm at Station 3091, and determining the coefficients a and b of the general equation:

$$y = a + bx$$

by the method of least squares, are given in Table 4. The coefficient, b, denoting the daily change in average depth of isotherms shows a pronounced relationship to vertical variation of density. In the surface layer, between 20.0° and 17.75°, the downward slope

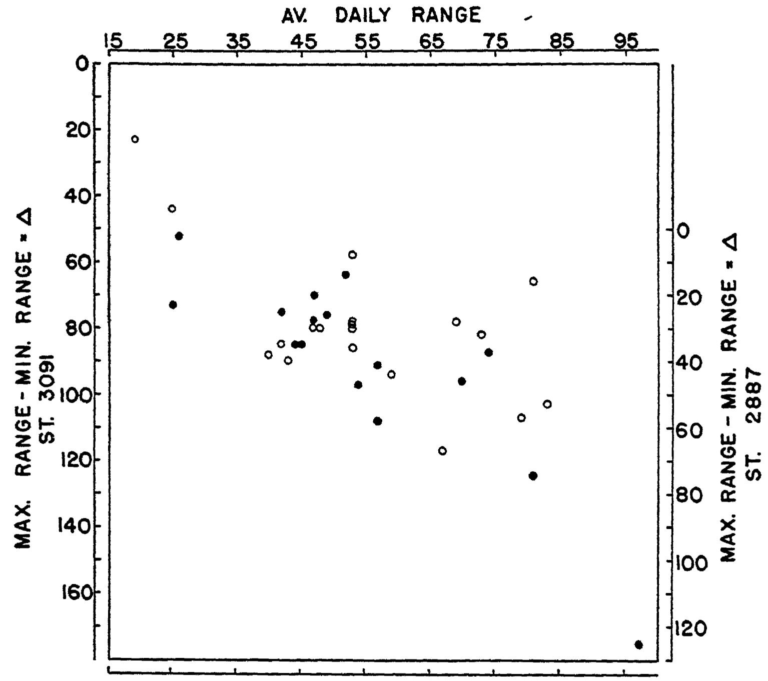

FIG. 5. Relationship. Average daily vertical displacement range of isotherms to difference between their maximum and minimum individual daily vertical ranges (maximum range − minimum range = Δ) for Stations 2887 (closed circles) and 3091 (open circles), scales in meters.

of isotherms increased from 0.59 to 9.77 meters per day; in the least stratified part of the water column, between 17.75° and 17.50°, there occurred an abrupt reversal of slope, and slightly deeper, the trend of the 17.0° isotherm sloping upward at an average rate of 6.26 meters per day. In still deeper water, with increasing stability, isothermal slopes continue upward, but at a slower rate, at 6° the linear rate being reduced to 3.07 meters per day. Thus, because of the abrupt phase reversal between 17° and 18° iso-

therms, the average vertical distance between them increased from 227 meters on June 17 to 367 meters on June 28; above 17.5° the water became increasingly warmer and below (to 1200 meters) increasingly colder.

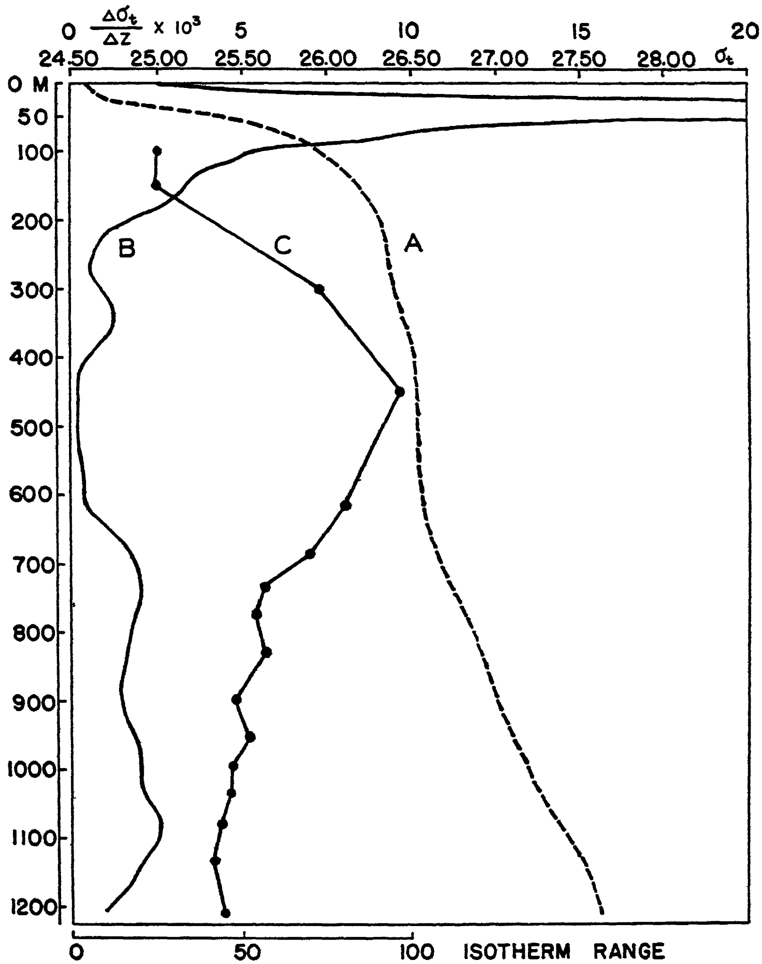

FIG. 6. Density (σ_t) distribution and vertical displacement of isotherms during 6 days at Station 2887. A = average vertical distribution of density, σ_t; B = average vertical variation of density, $\frac{\Delta\sigma_t}{\Delta z} \times 10^3$, units of σ_t per meter (uppermost scale); C = average daily vertical displacement ranges of isotherms (20°–6°, in meters, lower scale).

Average daily vertical displacement ranges [7] of the above isotherms (Table 3) were between 24 and 54 meters at Station 2886,

[7] Average of all daily vertical displacement ranges from midnight to midnight.

between 25 and 97 at Station 2887 and between 19 and 83 meters at Station 3091. Vertical variations in magnitudes both of daily average and total ranges are related, isotherms characterized by

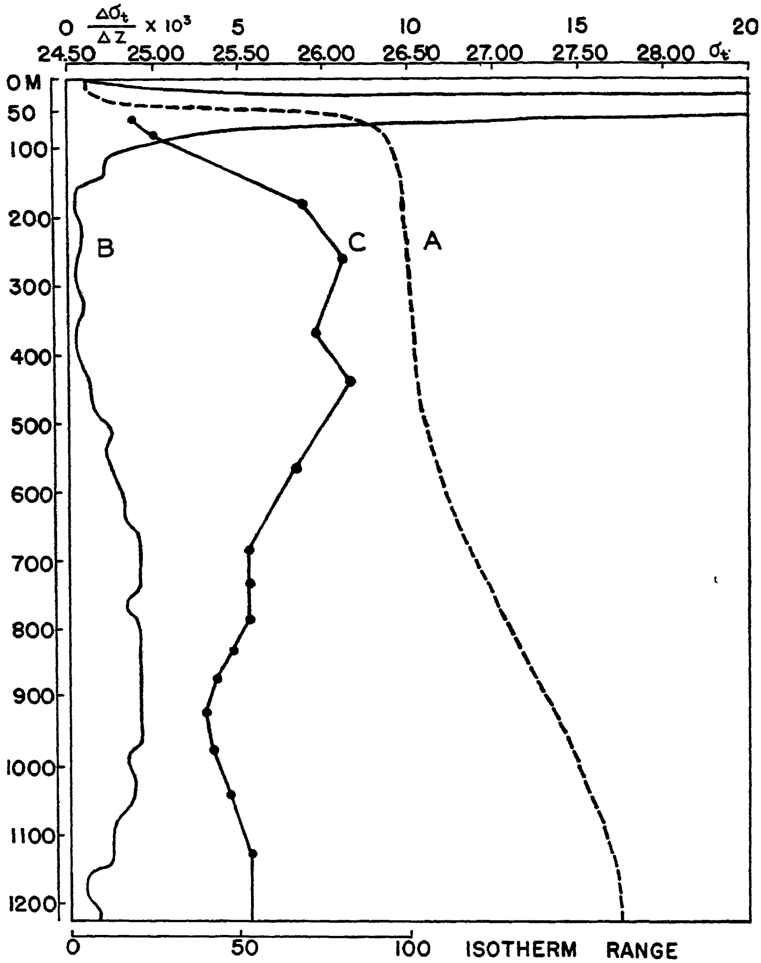

FIG. 7. Density (σ_t) distribution and vertical displacements of isotherms during 13 days at Station 3091. A = average vertical distribution of density, σ_t; B = average vertical variation of density, $\frac{\Delta\sigma_t}{\Delta z} \times 10^3$, units of σ_t per meter (uppermost scale); C = average daily vertical displacement ranges of isotherms (20°–5°, in meters, lower scale).

the larger total variations being, in general, characterized by the larger average daily vertical ranges and vice versa. Certain exceptions to this generalization occur within the least stratified water, between the 17° and 18° isotherms, where there is a phase

reversal in long period trends as previously brought out. Daily vertical displacement ranges entering into the computation of the average daily vertical displacement ranges of individual isotherms are, like temperature ranges, subject to wide variability. Thus, the individual daily ranges of the isotherms having the minimum average daily vertical range varied between 12 and 35 meters at Station 2887 (19° isotherm) and between 5 and 28 meters (20° isotherm) at Station 3091, and, for the isotherms showing maximum daily average ranges, individual daily ranges were between 36 and 161 meters (17.5° isotherm) at 2887 and between 39 and 142 meters (17.25° isotherm) at Station 3091; isotherms characterized by the larger average daily ranges showed, in general, greater variability in their individual daily ranges. The latter being illustrated by Fig. 5 where are plotted (for Station 2887 and 3091) differences between minimum and maximum daily ranges (Δ) against average daily displacement ranges; the relationship is similar to that for temperature variations at standard depths.

Although ranges of vertical displacements of isotherms vary from day to day (Table 3) a definite relationship to the vertical density distribution $\left(\frac{\Delta\sigma_t}{\Delta z}\right)$ persists. Thus, at Stations 2887 (Fig. 6) and 3091 (Fig. 7) magnitudes of vertical displacements, at a maximum where the water is least stable (to depths of 1200–1300 meters, $\frac{\Delta\sigma_t}{\Delta z}$ = approximately 2×10^{-4}), are suppressed with increasing stratification. In this least stratified water, between 17° and 18° isotherms,[8] average daily vertical displacements at Station 2887 were 81 to 97 meters and at Station 3091, 73 to 83 meters; in the intermediately stratified deep water $\left(\frac{\Delta\sigma_t}{\Delta z} = 2.6 \times 10^{-3}\right)$, average daily ranges of isotherms (15°–6°) decreased to 42–57 meters at

[8] At both stations the least stratified water was bounded by the 17° and 18° isotherms, and although observations were made at identical dates on successive years (Table 1) their average depths differed respectively by 121 and 142 meters, thus:

		Depth	$\frac{\Delta\sigma_t}{\Delta z} \times 10^3$	σ_t
Station 2887	18°	300	0.2–1.2	26.41
	17°	618		26.57
Station 3091	18°	179	0.2–0.6	26.46
	17°	476		26.56

Station 2887 and to 40–59 meters at Station 3091; and, in the strongly stratified surface layer $\left(\frac{\Delta\sigma_t}{\Delta z} = 74 \times 10^{-3}\right)$, average daily ranges of isotherms (20° and 19°) were reduced to 19–26 meters (Table 3).

Relationship of Daily Temperature and Vertical Displacement Ranges to Temperature Stratification of the Water Column and their Apparent Day by Day Changes

The inverted relationship between magnitudes of daily temperature and daily displacement ranges to vertical temperature stratification [9] of the water column (illustrated by average daily values for Stations 2887 and 3091, Fig. 8) brings out that damping of vertical displacements by increased vertical stratification is insufficient to offset an increasing time variation of temperature at the fixed depths concerned. Thus, for instance, at Stations 2887 and 3091 as the vertical temperature gradient $\left(\frac{\Delta T°}{\Delta z} \times 10^2 \text{ per m}\right)$ increased tenfold, from 0.2° to 2.0°, the average daily temperature range at the depths concerned increased approximately fivefold to correspond to a damping of average vertical displacements of the water particles by approximately one-half.

As a result of the preceding discussions of day by day changes in daily temperature ranges at standard depths, and of the magnitudes of daily displacements of isotherms, it is inferred that the internal wave mechanism causes day by day changes in the observed state of the sea; and a qualitative comparison based on average daily vertical displacements of whole degree isotherms, between 19° and 6°, for Stations 2887 and 3091 (Fig. 9) shows that, relatively speaking, both quiet and disturbed days may be expected. Thus, for instance, at Station 3091, during the 12 day period of observation, maximum disturbances of the water column on June 22 were nearly three times the minimum disturbances five days later, on June 27; abrupt changes in displacements of the water column may also occur from one day to the next; as, for instance (at Station 3091), on June 21, the average displacements of whole degree isotherms (between 19° and 6°) during the 24 hours

[9] Comparisons of Figs. 3, 4, 6, and 7 show that the vertical distribution of density (σ_t) is chiefly controlled by the vertical temperature distribution; consequently, since temperature stratification and density stratification are nearly identical the terms may, for purposes of this discussion, be used synonomously.

of 50 meters, increased to 93 meters on June 22, and then dropped off to 41 meters on June 23. Other day by day changes for stations concerned are brought out by Fig. 9. The close correspondence between magnitudes of average vertical displacements

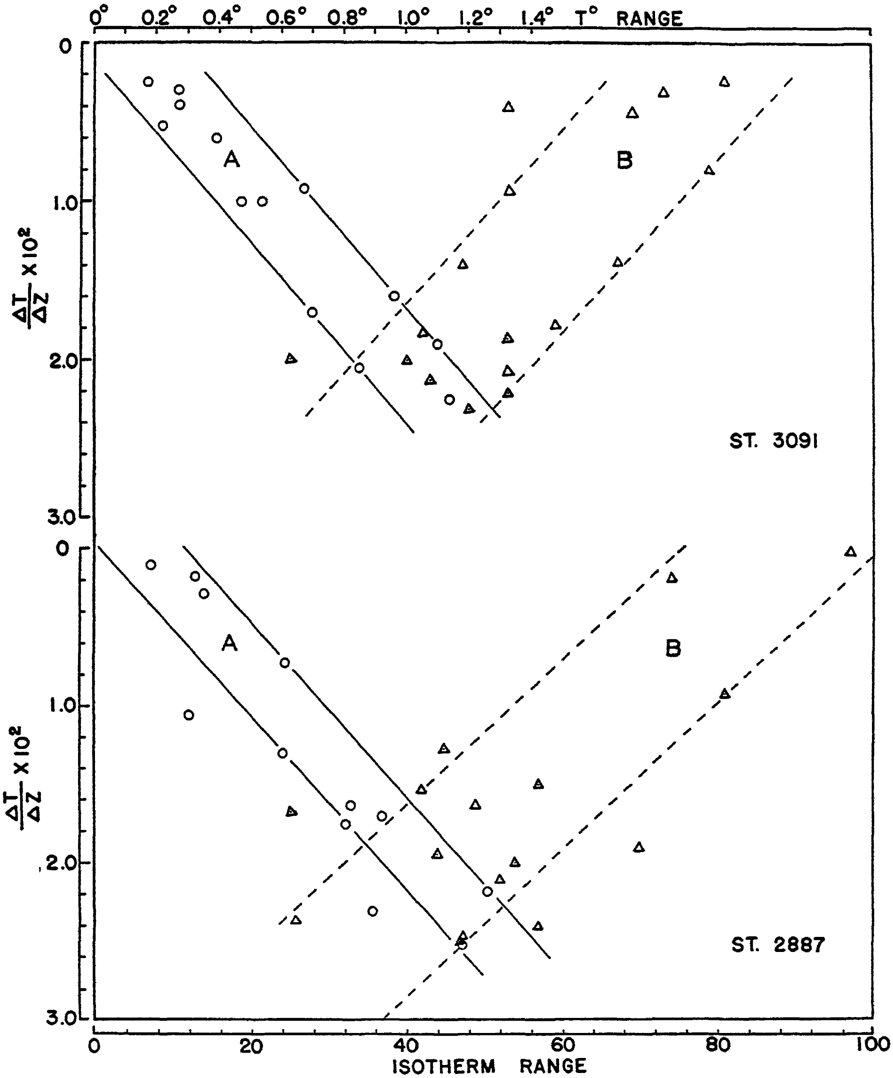

FIG. 8. Relationship. Average daily temperature ranges at standard depths, *A*, and average daily vertical displacement ranges of isotherms, *B*, to the average vertical variations of temperature $\frac{\Delta T^\circ}{\Delta z} \times 10^2$ (degrees per meter) for the water column of Stations 2887 and 3091.

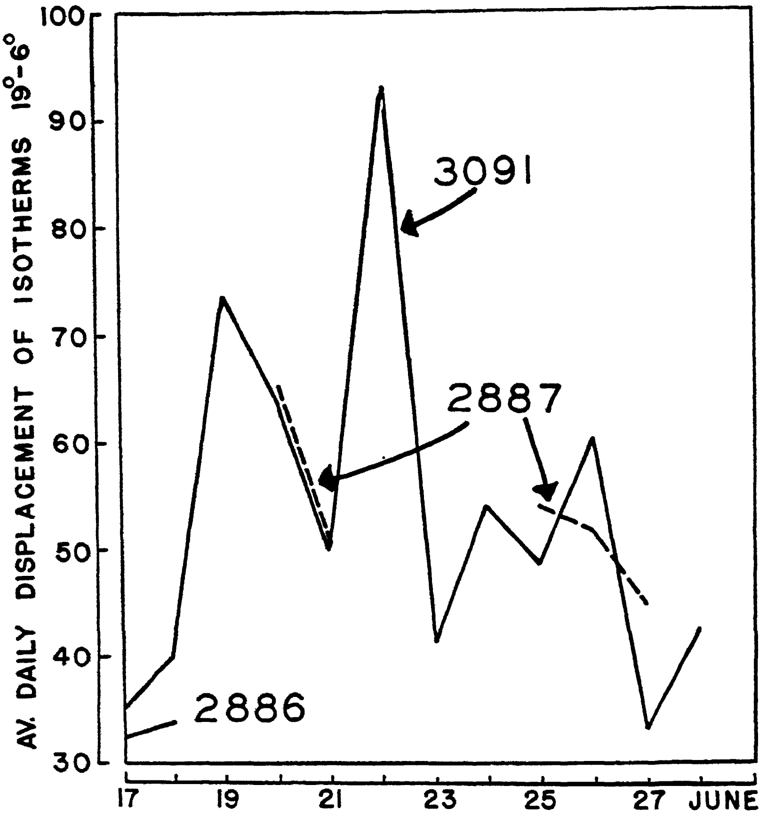

FIG. 9. Average daily vertical displacements of all unit isotherms between 19° and 6° for Stations 2886 (2 days), 2887 (5 days) in 1937 and for Station 3091 (12 days) in 1938.

of whole degree isotherms, between 19° and 6°, for Stations 2886, 2887, and 3091, for identical dates in 1937 and 1938 may be purely coincidental.

TIME ALTERATIONS IN DYNAMIC COMPUTATIONS FOR INDIVIDUAL VERTICALS IN THE SEA RESULTING FROM SHORT PERIOD VARIATIONS OF OCEANOGRAPHIC PROPERTIES

Application to the Western Sargasso Sea (24 hour period at Station 2887)

For representing the equilibrium state of the sea, as deduced from temperature and salinity observations along a series of verticals, it is convenient to compute dynamic heights of isobaric surfaces above a reference level, lying usually between 1200 and 2000

decibars.[10] Hence, for illustrative purposes of time variations in computed dynamic heights of isobaric surfaces as may occur along a single vertical in the western Sargasso Sea during a 24 hour period, the serial temperature and salinity measurements taken between $07^h\,57^m$, June 24, and $07^h\,33^m$, June 25, at "Atlantis" Station 2887 are considered, results of computations being summarized in Tables 5 and 6. In the present case, the variations in dynamic heights of isobaric surfaces result from the uneven changes in dynamic thicknesses of isobaric sheets; details of this are given in Table 5, as anomalies of dynamic thickness of successive isobaric sheets (bounded by standard isobaric surfaces) for 9 samplings of the water column during the 24-hour period.[11] Columns headed Δ (Table 5) represent differences in dynamic thicknesses between successive samplings (or the computed amounts of shrinking and stretching that isobaric sheets have undergone during the time interval), and illustrate (on comparison with Table 6) that changes in thickness of individual isobaric sheets frequently are greater than the total integrated changes in dynamic height at the sea surface of the whole water column. Thus, between series C ($13^h\,01^m$) and B ($10^h\,25^m$), the change in dynamic height, above the 1200 decibar surface, was only 0.20 dynamic centimeters (Table 6), whereas thickness of isobaric sheets between 100–200 decibars and between 200–300 decibars increased 0.80 and 1.00 dynamic centimeters, respectively (Table 5), while at the same time that between 1100 and 1200 decibars decreased 0.80 dynamic centimeters. Likewise, between series F ($22^h\,12^m$) and E ($18^h\,28^m$), variations in dynamic thickness of isobaric sheets, while not so large, were completely balanced; the summation of the total changes over the water column equaled zero (Table 6). The time change between series H ($04^h\,05^m$) and I ($07^h\,33^m$) was the only case in which every single variation in dynamic thickness of the isobaric sheets was in the same (negative) direction hence, summation gave the maximum effect at the surface. Large variations occurred in the highly stable water above 200 decibars, and smaller ones where the water column was least stable (between 400 and 500 decibars).

Successive upward summations of time variation in dynamic

[10] For details and tables, see: Bjerkness, *Dynamic Meteorology and Hydrography*, Publication No. 88, Carnegie Institution of Washington, 1910.

[11] Anomalies of dynamic height are obtained by successive summations of principal columns (A, B, etc.) in Table 5.

TABLE 5

Stratum D-Bars	A 6/24/37 $7^h\ 57^m$	B − A = Δ cms.	B 6/24/37 $10^h\ 25^m$	C − B = Δ cms.	C 6/24/37 $13^h\ 01^m$	D − C = Δ cms.	D 6/24/37 $16^h\ 02^m$	E − D = Δ cms.	E 6/24/37 $18^h\ 28^m$	F − E = Δ cms.	F 6/24/37 $22^h\ 12^m$	G − F = Δ cms.	G 6/25/37 $01^h\ 37^m$	H − G = Δ cms.	H 6/25/37 $04^h\ 05^m$	I − H = Δ cms.	I 6/25/37 $7^h\ 33^m$
0– 50	14.30	0.05	14.35	0.05	14.40	0.10	14.50	0.55	15.05	−0.40	14.65	−0.25	14.40	−0.10	14.30	−0.35	13.95
50– 100	11.60	−0.05	11.55	−0.05	11.50	0.20	11.70	0.70	12.40	−0.40	12.00	−0.10	11.90	−0.40	11.50	−0.70	10.80
100– 200	19.80	−0.60	19.20	0.80	20.00	−0.60	19.40	0.30	19.70	−0.20	19.50	0.40	19.90	−0.80	19.10	−0.60	18.50
200– 300	17.60	−0.40	17.20	1.00	18.20	−0.70	17.50	0.10	17.60	−0.20	17.40	−0.10	17.30	0.10	17.40	−0.30	17.10
300– 400	17.00	−0.20	16.80	0.20	17.00	0.10	17.10	0.00	17.10	−0.10	17.00	−0.20	16.80	0.20	17.00	−0.20	16.80
400– 500	16.80	0.00	16.80	−0.10	16.70	0.10	16.80	0.00	16.80	0.00	16.80	0.00	16.80	0.00	16.80	−0.10	16.70
500– 600	16.80	−0.10	16.70	0.10	16.80	0.20	17.00	−0.20	16.80	0.20	17.00	0.00	17.00	0.00	17.00	−0.10	16.90
600– 700	16.00	−0.10	15.90	0.10	16.00	0.40	16.40	−0.40	16.00	0.60	16.60	0.00	16.60	0.20	16.80	−0.30	16.50
700– 800	14.60	0.00	14.60	0.00	14.60	0.30	14.90	−0.30	14.60	0.60	15.20	0.10	15.30	0.10	15.40	−0.50	14.90
800– 900	13.60	0.00	13.60	−0.30	13.30	0.00	13.30	−0.10	13.20	0.50	13.70	−0.20	13.50	0.50	14.00	−0.50	13.50
900–1000	12.30	−0.20	12.10	−0.30	11.80	−0.30	11.50	0.20	11.70	0.20	11.90	−0.30	11.60	0.70	12.30	−0.20	12.10
1000–1100	10.00	0.10	10.10	−0.50	9.60	−0.40	9.20	0.50	9.70	−0.30	9.40	0.10	9.50	0.40	9.90	−0.10	9.80
1100–1200	7.70	0.60	8.30	−0.80	7.50	−0.20	7.30	0.40	7.70	−0.50	7.20	0.40	7.60	0.10	7.70	0.00	7.70

Time variations in anomalies of dynamic thickness of standard isobaric sheets during a 24 hour period, $7^h\ 57^m$, June 24–$7^h\ 33^m$, June 25, 1937, at Station 2887.

thicknesses (Δ values in Table 5) of isobaric sheets give amounts of variation in dynamic heights of standard isobaric surfaces as computed for successive samplings of the water column (Table 6), and illustrate the magnitude of the discrepancies that may be induced in projections of dynamic topographies of these surfaces in a region such as the one represented by Station 2887. Time variations in computed dynamic heights are irregular throughout the water column and those at the sea surface are not proportional to those at deeper lying isobaric surfaces. Thus, for example, between the series taken at $10^h 25^m$ and $13^h 01^m$ (Table 7), dy-

TABLE 6

Pressure D-Bars	$10^h 25^m$–$7^h 57^m$	$13^h 01^m$–$10^h 25^m$	$16^h 02^m$–$13^h 01^m$	$18^h 28^m$–$16^h 02^m$	$22^h 12^m$–$18^h 28^m$	$01^h 37^m$–$22^h 12^m$	$04^h 05^m$–$01^h 37^m$	$07^h 33^m$–$04^h 05^m$
0	−0.90	0.20	−0.80	1.75	0.00	−0.15	1.00	−3.90
50	−0.95	0.15	−0.90	1.20	0.40	0.10	1.10	−3.60
100	−0.90	0.20	−1.10	0.50	0.80	0.20	1.50	−2.90
200	−0.30	−0.60	−0.50	0.20	1.00	−0.20	2.30	−2.30
300	0.10	−1.60	0.20	0.10	1.20	−0.10	2.20	−2.00
400	0.30	−1.80	0.10	0.10	1.30	0.10	2.00	−1.80
500	0.30	−1.70	0.00	0.10	1.30	0.10	2.00	−1.70
600	0.40	−1.80	−0.20	0.30	1.10	0.10	2.00	−1.60
700	0.50	−1.90	−0.60	0.70	0.50	0.10	1.80	−1.30
800	0.50	−1.90	−0.90	1.00	−0.10	0.00	1.70	−0.80
900	0.50	−1.60	−0.90	1.10	−0.60	0.20	1.20	−0.30
1000	0.70	−1.30	−0.60	0.90	−0.80	0.50	0.50	−0.10
1100	0.60	−0.80	−0.20	0.40	−0.50	0.40	0.10	0.00
1200	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00

Time change in dynamic heights (dynamic centimeters) of standard isobaric surfaces between successive observation series during a 24 hour period at Station 2887 (calculated from a reference surface of 1200 decibars).

namic height of the sea surface increased 0.20 dynamic centimeters, but at the 700 and 800 decibar surfaces it decreased 1.90 dynamic centimeters; whereas, between $04^h 05^m$ and $7^h 33^m$, change in dynamic height of the sea surface was at a maximum (representing a continuous accumulation of changes in the deeper water).

Application to the Gulf Stream (24 hour period at Station 2855)

Dissimilarities between dynamic situations of the western Sargasso Sea and the Gulf Stream system (along the American coast) as illustrated by vertical distributions of density (Fig. 10) are such that not only will vertical oscillations cause greater variations of oceanographic characteristics in the latter, but horizontal displace-

ments of the water masses may also be effective in bringing about temperature variations at standard depths. Thus, for illustrative purposes, time variations based on four series of observations

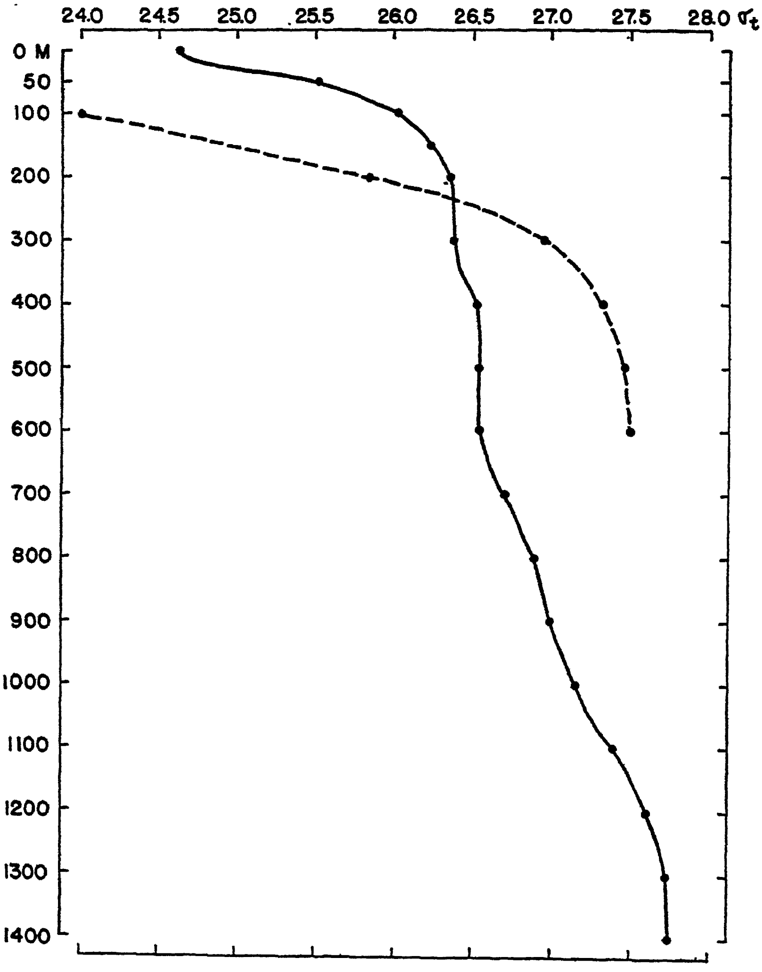

FIG. 10. Vertical distribution of density (σ_t) at Station 2887 (full line) in the Western Sargasso Sea, and at Station 2855 (dashed line) in the Gulf Stream off the Florida coast.

(A: $21^h\ 31^m$, April 18; B: $03^h\ 23^m$, April 19; C: $11^h\ 23^m$, April 19; D: $15^h\ 21^m$, April 19, 1937) at "Atlantis" anchor Station 2855 in the Straits of Florida[12] (25° 37′ N, 79° 51′ W, depth 680 meters)

[12] These 4 samplings were selected, as being sufficient for illustrative purposes, from a series of 12 at Station 2855, which is one of a group of 5 anchor Stations (2854–2858) across the Florida Straits. Methods of computation are identical with those used for

are summarized in Table 7. The variations in dynamic heights during this 24 hour period (last column, Table 7), at 100 and 200 decibars, were 4.3 and 2.3 times the maxima recorded for identical surfaces in the Sargasso Sea region, while time variations in isobaric sheet thicknesses were, on the average, 3.7 to 1.2 times those in the latter region. Perhaps noteworthy is the fact that positive and negative short period changes in dynamic thickness over the water column were not as irregular as at 2887, a result possibly of the internal wave mechanism acting on a shallower water column or perhaps of a difference in character of the mechanism itself.

TABLE 7

Pressure Decibars	A 4/18/37 $21^h 31^m$	B − A = Δ	B 4/19/37 $03^h 23^m$	C − B = Δ	C 4/19/37 $11^h 23^m$	D − C = Δ	D 4/19/37 $15^h 21^m$	Cms. Range
100	67.55	12.95	80.50	−6.60	73.90	−2.25	71.65	12.95
150	51.25	10.45	61.70	−7.40	54.30	−0.05	54.25	10.45
200	39.10	7.45	46.55	−7.20	39.35	1.95	41.30	7.45
250	30.55	4.65	35.20	−4.80	30.40	1.60	32.00	4.80
300	24.20	2.30	26.50	−2.75	23.75	1.35	25.10	2.75
350	19.05	0.90	19.95	−1.45	18.50	1.05	19.55	1.45
400	14.60	0.40	15.00	−0.75	14.25	0.80	15.05	0.80
450	10.60	0.35	10.95	−0.50	10.45	0.65	11.10	0.65
500	6.90	0.30	7.20	−0.35	6.85	0.45	7.30	0.45
550	3.40	0.15	3.55	−0.15	3.40	0.20	3.60	0.20
600	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00

Time variations in anomaly of dynamic height (from the 600 decibar surface) in the Gulf Stream at Station 2885 during the period, $21^h 31^m$, April 18 to $15^h 21^m$, April 19, 1937.

Effect of Short Period Variations of Oceanographic Properties on Computations of Dynamic Topography, Current Velocities, and Volume Transports

Application to the Western Sargasso Sea

Because of regional differences in horizontal distributions of oceanographic properties, the significance of their short period variations to dynamic computations is relative; comparative situations are brought out by brief discussions of two hydrographically

Station 2887, except that (because of shallower depth) dynamic heights of isobaric surfaces are referred to the 600 decibar surface, and because of unstable conditions close to the surface the water column is considered only below 100 meters depth. Hence, comparisons of water columns for the two regions from identical reference levels would, no doubt, show somewhat modified results (see papers in *Physical Oceanography and Meteorology*, Vol. VII, No. 3, 1939).

dissimilar parts of the North Atlantic; *e.g.*, the western Sargasso Sea and the Gulf Stream system off the American coast.

Computation of dynamic slopes of isobaric surfaces in the western Sargasso Sea (from the 2000 decibar reference level) as based on "Atlantis" Stations 2875 (35° 21′ N, 66° 46′ W), 2877 (34° 09′ N, 66° 02′ W) and 2879 (32° 50′ N, 65° 03′ W) show a decrease of 7.95 dynamic centimeters in dynamic height of the sea surface between 2875 and 2877 and of 2.88 dynamic centimeters between 2877 and 2879 (Fig. 11). Hence, a short period variation of 3.9 dynamic centimeters (as was computed to occur within a few hours at Station 2887; Table 6) would be sufficient to bring about resulting discrepancies between 49 and 139 per cent in dynamic slopes of the sea surface in this region. Variations within the water column are of the same order of magnitude; between Stations 2875 and 2877 the decrease in thickness of isobaric sheets was between 0.8 to 2.8 times, and between 2877 and 2879 it was 0.09 to 3.0 times the computed average time change in thickness of isobaric sheets at Station 2887.

Since computations of current velocities are based on dynamic slopes of isobaric surfaces, similar proportionate discrepancies are to be expected when applied to this region. Thus the current velocity, C, is given by

$$C = \frac{1}{2\omega \sin \varphi} \cdot \frac{H_A - H_B}{L},$$

where $H_A - H_B$ represents differences in dynamic heights of isobaric surfaces above an arbitrary deep reference surface, between two verticals (A and B), separated by a distance, L, φ the mean latitude of A and B, and ω the angular velocity of the earth's rotation.

The data of the present discussion also permit information to be brought out concerning the magnitude of possible discrepancies in computations of volume transports both in the western Sargasso Sea and the Gulf Stream system. Thus, it can be shown that the volume transport (V), above a surface of zero geopotential, between two verticals (A and B) in the sea is closely given by the formula

$$V = \frac{10}{2\omega \sin \varphi} (\Delta Q_A - \Delta Q_B) m^3 \sec^{-1},$$

where

$$\Delta Q = \int_z^0 \Delta H dz,$$

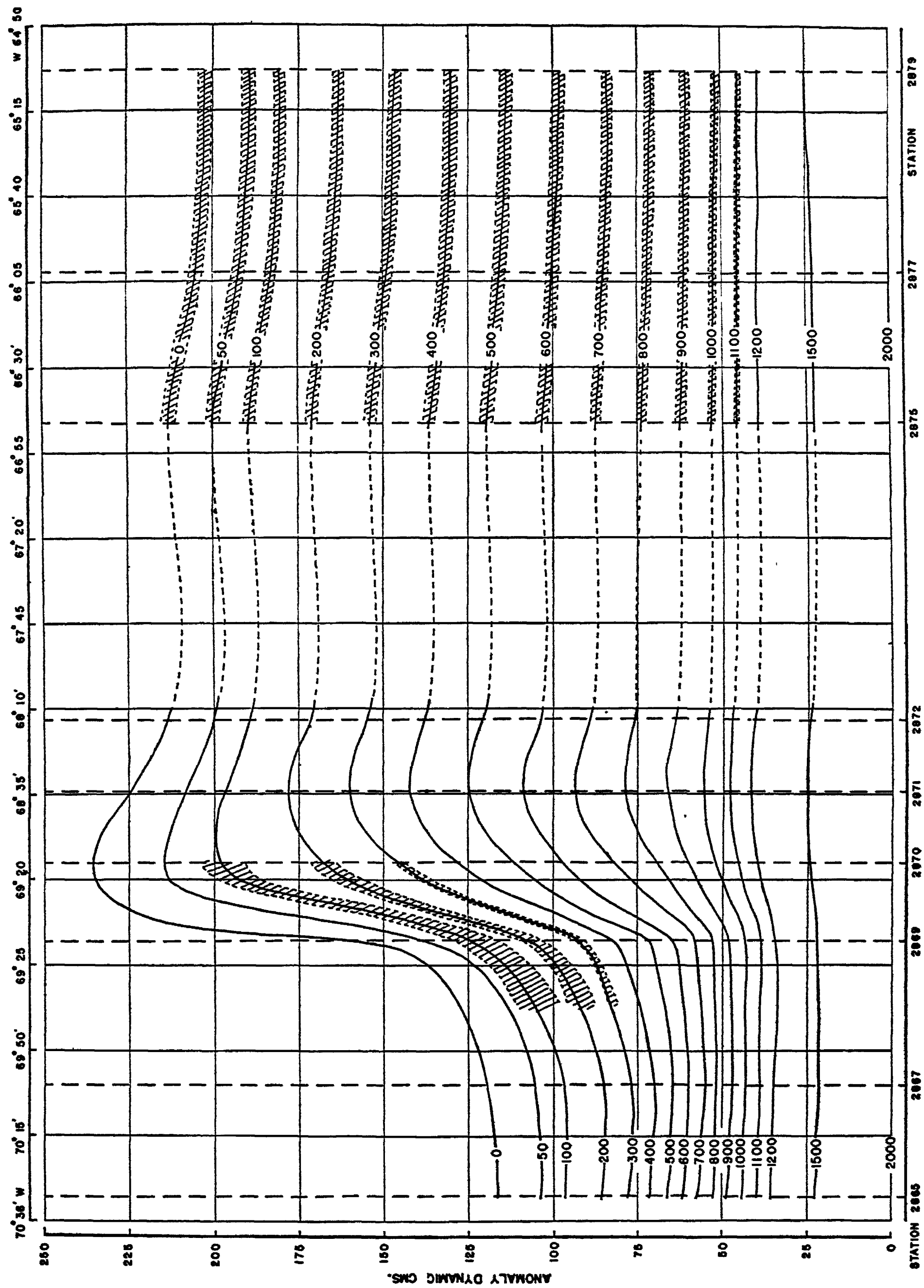

FIG. 11. Dynamic slopes of standard isobaric surfaces from 2000 decibar surface across the Gulf Stream as determined by "Atlantis" Stations 2865 (39° 24′ N, 70° 33′ W) to 2872 (36° 43′ N, 68° 13′ W) and over the Western Sargasso Sea as determined by Stations 2875 (35° 21′ N, 66° 46′ W) to 2879 (32° 50′ N, 65° 03′ W). Shaded areas represent range of short period fluctuations in dynamic height observed in the Gulf Stream at Station 2855 and in the Western Sargasso Sea at Station 2887.

ΔH being the anomaly of dynamic height. $\Delta Q_A - \Delta Q_B$ is designated as $\Delta' Q$.

In the western Sargasso Sea, the computed time variation of ΔQ (referred to 1200 decibars, equation 11) during a 24 hour period (beginning $07^h\ 57^m$, June 24) at Station 2887 had an average value

$$(\Delta Q)_M = 948.521 \text{ m}^3 \text{ sec}^{-2},$$

the average absolute change between series of observations was

$$(\Delta' Q) = 8.533 \text{ m}^3 \text{ sec}^{-2}$$

and the average absolute hourly change (over the 24 hour period) was

$$(\Delta' Q) = 2.905 \text{ m}^3 \text{ sec}^{-2},$$

and the maximum ΔQ difference $(\Delta' Q)$ between any two series was

$$(\Delta' Q)_{\max} = 30.000 \text{ m}^3 \text{ sec}^{-2}.$$

The consequence of these results to transport computations is relative (as for current computations), depending on $\Delta' Q$ variations in the area. The effect is variable and while the largest recorded variations of Q or ΔQ are not the usual rule, they occur with sufficient frequency so that even occasional large errors in transport computations from uncontrolled data [13] may vitiate, or at least seriously interfere with, conclusions of regional and seasonal differences in transport.

As an illustration of the horizontal changes in ΔQ $(\Delta' Q)$ to be expected in the Western Sargasso Sea, based on four groups of "Atlantis" Stations in this region, viz: 2875 (35° 21′ N, 66° 46′ W; June 7, 1937), 2877 (34° 09′ N, 66° 02′ W; June 8, 1937), 2879 (32° 50′ N, 65° 03′ W; June 8, 1937), 1220 (32° 40′ N, 65° 00′ W; April 17, 1932), 1221 (32° 51′ N, 66° 25′ W; April 18, 1937), 1360 (32° 43′ N, 65° 00′ W; August 28, 1932), and 1361 (33° 13′ N, 66° 20′ W; August 28, 1932), the computed range was from 19.120 to 107.687 m³ sec⁻², the average $\Delta' Q$ value being 53.614 m³ sec⁻². Comparison of these horizontal ΔQ variations with time ΔQ variations during a 24 hour period at Station 2887 shows that the maximum ΔQ difference between any two sets of observations at Station 2887 (30.00 m³ sec⁻²) was nearly 56 per cent of the average $\Delta' Q$ value in the Western Sargasso Sea, and between 28 and 157 per cent of individual horizontal ΔQ differences $(\Delta' Q)$. The aver-

[13] Data obtained without knowledge of time variation of temperature and salinity.

age change of ΔQ between observation series at Station 2887 ($8.533\ m^3\ sec^{-2}$) was 16 per cent of the average horizontal $\Delta' Q$ value and from 8 to 45 per cent of the individual values.

For this group of stations the values of V/L, or total volume transports (computed from equation 9) divided by the linear distances between stations, ranged from $13.80\ m^3\ sec^{-1}$ per m to $101.60\ m^3\ sec^{-1}$ per m, and the average was

$$\frac{V}{L} = -\ 47.98\ m^3\ sec^{-1} \text{ per m},$$

which may be the best value of transport per linear meter between latitudes 35° 21′ N–32° 40′ N and longitude 65° 00′ W–66° 46′ W. Variations in ΔQ, V, and V/L result from local, annual, seasonal, and short period variations of oceanographic elements, the partial effects of long period and local components being overshadowed by the superimposed short period variations.

Application to the Gulf Stream System off the American Coast

Short period time variations of dynamic heights at Station 2855 (previously discussed) when transposed to the dynamic pattern of this current system (based on a profile of six "Atlantis" Stations, transverse to the current, extending south 33° east from Montauk Point: 2865, 39° 23.5′ N, 70° 33′ W; 2867, 38° 41′ N, 70° 00′ W; 2869, 38° 02′ N, 69° 18′ W; 2870, 37° 34′ N, 68° 55′ W; 2871, 37° 08′ N, 68° 34′ W; 2872, 36° 43′ N, 68° 13′ W; June 4–6, 1937) may be shown to be of less relative significance than in the Western Sargasso Sea. Anomalies of dynamic height, computed from 2000 decibar surface, are illustrated by Fig. 11; shaded areas over the 100, 200, and 300 decibar surfaces represent 24 hour ranges in elevation deduced from continuous measurements at Station 2855. The main part of the Gulf Stream is well marked by the strong dynamic slope between Stations 2867 and 2870 (131 kilometers), the difference in dynamic height of the sea surface being 115.83 dynamic centimeters, and that at the 100 decibar surface being 100.30 dynamic centimeters. Thus, the average slope of the 100 decibar surface, over a horizontal distance of 131 kilometers, of 0.76 dynamic centimeters per kilometer, would (based on information from Station 2855) be subject to short period variation of about 13 per cent, whereas this same surface in the Western Sargasso Sea, with an average slope from 0.044 to 0.016 dynamic centimeters per kilometer (over horizontal distances of 149 to 172

kilometers), may be subjected to variations ranging from 50 to more than 100 per cent.

· Computed current velocities for the Gulf Stream are subject to proportionate variations. However, it seems likely that such variations will affect velocity computations for the Gulf Stream most where horizontal velocities are least, so that largest errors may be expected in computations applying near the boundaries of the Gulf Stream and to its weaker counter currents.

Horizontal variation of ΔQ ($\Delta' Q$) per unit distance in the Gulf Stream section off Montauk Point ($\Delta' Q$ = 640.160 m^3 sec^{-2} over 213.9 kilometers) was more than eight times that in the Western Sargasso Sea (average $\Delta' Q$ = 53.614 m^3 sec^{-2} over 148 kilometers); and its maximum value ($\Delta' Q$ = 444.010 m^3 sec^{-2} over 62 kilometers) was nearly nine times the maximum recorded for the latter region ($\Delta' Q$ = 107.687 m^3 sec^{-2} over 134.4 kilometers), and after correcting for differences in latitude, the average volume transport per meter of the Gulf Stream (V/L = 370.54) was 7.7 times the average for the Western Sargasso Sea (V/L = 47.98), and its maximum (V/L = 801.37), approximately eight times the maximum (V/L = 101.60) for the latter region. The total volume transport of the Gulf Stream between Stations 2867 and 2871 is calculated, on the basis of the data under discussion, to be 71.62 m^3 sec^{-1}, and, it is likely that discrepancies in volume transport computations for the Gulf Stream system, will, like current computations, be less than in the Western Sargasso Sea or dynamically similar regions; within the former, larger percentage departures are to be expected in computations involving transports of the counter currents. In this respect the maximum time variation of ΔQ at Station 2887 (30.000 m^3 sec^{-2}) represents between 7 and 31 per cent of the ΔQ change across the Gulf Stream proper, but approximately two and one-half times the ΔQ change involving the western boundary between Stations 2865 and 2867.

VASCULARITY IN THE BRAINS OF TAILED AMPHIBIANS. II. NECTURUS MACULOSUS RAFINESQUE

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(Communicated by Edwin G. Conklin)

ABSTRACT

The caliber of the cerebral capillaries in *Necturus* is larger than in any other animal where it has been measured, averaging 13.6 μ. The two limbs of a capillary loop are frequently unequal in caliber. The hypoglossal nucleus has relatively rather wide capillaries while those in the neuropil average narrower than in most other regions.

The total length of the capillaries in a unit volume of tissue is the least yet found in any animal. It is only about one-third as great as in Ambystoma, which is intermediate between *Necturus* and the frog. The differences in the vascularity of various parts of the brain also are less than in other animals.

The two areas of neuropil studied are notably vascular and the molecular layer of the cerebellum is also well supplied with capillaries while the corresponding cellular regions are remarkably poor. Speculations arise regarding diffusion of substances through the latter, the trophic functions of dendrites, and possible bearings on phylogeny.

The hypothalamus is very poorly vascularized throughout, with no foreshadowing of the remarkable richness of certain parts in mammals.

The volume of the capillaries in a unit volume of tissue is greater in most parts than in any animal hitherto studied but a cubic millimeter of blood is exposed to a much smaller area of capillary wall.

The area of the capillary walls in a unit volume of brain is similar in *Necturus* to that in *Ambystoma* and not much larger than that in the frog. It is less than in the rat but greater than in the dogfish in most regions.

No distinct quantitative difference distinguishes amphibians with loop and network types of capillary bed, and amphibians and elasmobranchs, as classes, do not differ widely in cerebral vascularity.

THE fact that, for better understanding of the nervous system in practically all its aspects, a much more complete knowledge of the intricate relations between that system and its blood supply is of prime importance has come to be recognized with increasing clearness during recent years and more and more investigators in various parts of the world have devoted their attention to elucidating these relations. Very fundamental are quantitative relationships and as a background for the important researches of neuropathologists on changes in the blood supply a thorough knowledge of such features in normal conditions is requisite and is gradually being built up. The contribu-

tion of the comparative anatomist towards the appreciation of the significance of the findings of his colleagues in other fields is properly the supplying of information regarding conditions in various types of lower animals and their relationships to each other and to that which has been discovered by these colleagues.

With this point of view in mind, the writer has for some years been carrying on a series of directly comparable quantitative measurements of vascularity in the central nervous systems of normal vertebrates representing the different classes. The most widely known amphibian being the frog, the brain of this animal was naturally studied first in its class ('38 *d*). However, attention had meanwhile been drawn to the existence in the Amphibia of two remarkably different types of vascular mechanism for the central nervous system, namely a capillary network, on the one hand and a system of independent capillary loops on the other ('38 *a*). It was believed that these two mechanisms were so different that quantitative studies on that of the frog (which has a spongy vascular network in the substance of the central nervous tissue) could not be considered representative for all amphibians, but only for its own subclass, while separate measurements would be required to reveal conditions in the brains of tailed amphibians, which have a less highly differentiated nervous structure and are vascularized by simple capillary loops.

Further, within the subclass Caudata it has been shown that the genus *Ambystoma* differs from all others hitherto examined and is somewhat intermediate in that it has a very simple capillary network interspersed among its loops. Hence it seemed desirable to have the tailed amphibians represented by measurements in two different genera, namely *Ambystoma* and one other. The other chosen was *Necturus,* the adult brain structure of which is probably the simplest among living amphibians and has been found by Herrick to correspond in many respects with that in larval *Ambystoma*. *Ambystoma* undergoes a complete metamorphosis during development, while *Necturus* is a perennibranchiate animal, usually regarded as a permanent larva. The results of the studies on *Ambystoma* have already been published ('39) and those on *Necturus* are reported here. Photomicrographs of sections of the brain of *Necturus* with injected capillaries were published in the 1938 *a* and the 1938 *e* papers.

MATERIAL

The material consisted of brains of adult *Necturus maculosus* caught in the vicinity of Toronto. These were prepared in the same way as the specimens used in studies by the writer previously reported, being injected with carmine gelatine, fixed in formalin, imbedded in celloidin, and mounted as complete series, which were then lightly stained with picric acid. The measurements were all made in series which were cut transversely, the sections being 20 microns thick. Each eleventh section was 200 microns thick, these thick sections serving to some extent to assist a check on the completeness of injection. Of course, as has been pointed out in previous cases, such a check does not prove that the injection is complete in adjacent thin sections but merely increases the probability that such is the case. Furthermore, it is obvious that such a check is of much less value when applied to a system of independent capillary loops than when applied to a continuous spongy network.

An additional source of possible error against which a special watch was necessary and which is peculiar to a system of independent loops is brought about by the not infrequent withdrawal of the loops from their perivascular sheaths as a result of rough handling or of the shrinkage during preparation. As each simple loop as a whole lies in a single sheath with no brain tissue between the limbs of the loop, it can very easily be pulled right out. Incidentally it may be pointed out that this inclusion of both the closely approximated limbs of the loop in a single simple perivascular sheath lends added support to the theory outlined in a former note ('38 *b*), according to which there may be an exchange of gases between the two limbs of the loop such that the oxygen and carbon dioxide contents are fairly uniform throughout its length and equal approximately to the average of the contents of the blood which enters the loop and of that which leaves it.

Though the injection appeared to be satisfactory, the variability of the results was found to be very great. This is doubtless to be expected where vascularization is by very widely spaced independent loops. It may be influenced by unconscious selection of areas which contain loops or of areas which do not, though every effort was made to avoid any such selection. To compensate to a small extent for these difficulties eleven speci-

TABLE 1

TOTAL LENGTH IN MICRONS OF THE CAPILLARIES IN 1/2 × 189^2 × 200 c. μ OF TISSUE IN THE BRAIN OF THE MUDPUPPY (NOT CORRECTED FOR SHRINKAGE EXCEPT THOSE IN THE LAST COLUMN)

	Necturus											Average	% P.E. of Average	Corrected Average Length in 100^3 c. μ of Fresh Tissue
	1 ♂	2 ♀	4 ♂	5 ♂	7 ♀	9 ♂	10 ♂	11 ♂	12 ♂	15 ♂	16 ♂			
Fasc. long. medialis	144	248	236	113	116	32	70	274	205	143	54	149	11.2	26
Nuc. motorius dors. X	449	360	410	472	194	260	231	373	294	482	217	340	6.3	59
Nucleus XII	294	332	161	258	205	321	17	25	138	335	399	226	11.3	39
Nuc. motorius VII	416	280	242	410	315	188	396	311	315	259	83	292	7.0	51
Nuc. motorius V	230	79	270	132	103	0	62	353	320	193	251	181	12.6	31
Nuc. fasc. solitarii	160	260	149	88	138	129	0	0	27	230	17	108	17.1	19
Nuc. spinalis V	50	283	247	119	40	95	246	32	211	71	0	127	16.2	22
Cerebellum molecular layer	210	322	110	218	382	147	303	201	108	311	244	232	7.9	40
Nuc. sens. principalis V	387	391	334	394	208	448	391	224	292	505	94	333	7.3	58
Cerebellum: granular layer	29	35	83	0	17	0	114	0	0	5	35	29	24.9	5
Eminentia cerebelli ventr.	35	174	258	42	91	173	0	67	31	175	262	118	16.1	20
Nucleus VIII	439	383	457	644	574	415	409	605	553	702	529	519	4.2	90
Primordium hippocampi	474	493	430	333	305	233	416	310	418	215	241	352	5.7	61
Prim. pallii dorsalis	182	145	111	175	227	195	0	0	45	152	21	114	14.9	20
Prim. piriforme	135	168	83	0	0	105	49	0	15	6	45	55	22.0	10
Neuropil of p. piriforme	672	648	495	456	307	276	417	512	505	628	581	500	5.3	87
Nuc. medialis septi	340	385	251	298	239	200	175	231	94	205	213	239	6.8	41
Corp. striatum; pars ventralis	91	151	25	5	37	0	0	87	0	56	35	44	22.4	8
Neuropil of corpus striatum; pars ventralis	595	503	440	311	389	228	317	333	316	495	328	387	5.7	67
Nuc. dorsolateralis amygdalae	134	330	206	0	0	27	70	87	215	8	0	98	23.0	17
Nuc. preopticus magnocellularis	95	39	142	38	79	0	0	0	78	26	115	56	17.9	10
Pars dorsalis hypothalami, lobus ventralis	0	—	172	0	0	90	25	27	0	22	127	46	28.6	8
Pars ventralis hypothalami, lobus anterior	100	—	0	0	67	148	0	18	23	0	0	36	30.9	6

mens were used instead of the usual ten. These ranged in total length from 24 cm. to 30 cm. and their sexes are shown in Table 1.

The instruments used were the same employed in the writer's previous quantitative studies.

Shrinkage was measured in the three dimensions of two specimens. The average linear shrinkage was found to be 20 per cent and the correction to be applied to the measurements of the capillaries in order to determine the total length of the vessels in a unit volume of fresh tissue is 0.62. The observation that shrinkage is considerably greater than in the brain of the frog leads to the suspicion that the corrected averages given for *Ambystoma* ('39), which were obtained by use of a coefficient borrowed from the frog, may be a little too high.

Caliber of the Capillaries

The caliber of the capillaries was measured in seven areas in each of five specimens, the average of fifty measurements being taken where possible. In the corpus striatum only one or, at most, two loops usually penetrated through its substance, a few others ending between the outermost cells. In this region it was therefore possible to make only twenty measurements a number of these being on different parts of the same loop.

The diameter of a single capillary varies somewhat along its course, and there seemed to be a tendency for both limbs of a loop to expand slightly towards their junction at its tip, though this may be an artifact. Moreover the diameter of one limb frequently tended to be greater than that of the other. Hence every group of fifty measurements was made on the two limbs of twenty-five pairs of capillaries and besides the average of all, the average of the thinner limbs and that of the thicker limbs was noted.

The results of combining all these records appear in the first four columns of Table 2. All the capillaries are extraordinarily large, larger than in any other animal for which such measurements have been published, though not quite so wide as was previously recorded ('38 *e*). This is no doubt primarily an adaptation to the unusual size of the erythrocytes, which are very large in all perennibranchs and, as in other animals, are even considerably larger than the average diameter of the capillaries. Smith ('25) found the width of the red blood cell to be 25–30 μ and the length over 50 μ.

TABLE 2

VARIOUS DIMENSIONS OF THE CAPILLARIES IN PARTS OF THE BRAIN OF THE MUDPUPPY

	Average Diam. of Thinner Limb	Average Diam of Thicker Limb	Average Diam in μ not Corrected	Average Diam. in μ Corrected for Shrinkage	Vol. in cu. μ of Capillaries in 100^3 cu μ of Fresh Tissue	Area in sq. μ of Cap. Walls in 100^3 cu. μ of Fresh Tissue	Area in sq. mm. to Which 1 cu mm. of Blood is Exposed
Fasc. longitudinalis medialis........	10.2	12.5	11.6	14.5	4,234	1,184	278
Nucleus XII...... .	11.5	13.9	12.7	15.9	7,647	1,948	253
Cerebellum, molecular layer...... .	9.8	11.6	10.7	13.4	5641	1,684	299
Cerebellum, granular layer...........	9.5	11.7	10.6	13.2	684	207	303
Corpus striatum, pars ventralis........	9.2	10.6	9.9	11.1	760	279	364
Neuropil of corpus striatum	9.3	10.8	10.1	12.4	8,091	2,610	323
Primordium hippocampi........ .	10.3	13.0	11.7	14.6	10,213	2,798	274
Average........			11.0	13.6			299

As in the frog and in *Ambystoma,* the vessels in the hypoglossal nucleus average wider than those elsewhere among the parts examined, so that this condition would appear to be one of general significance in amphibians, whatever that significance may be. The capillaries in the primordium hippocampi and in the medial longitudinal bundle are not significantly smaller, yet the three amphibians agree in having these vessels average a little narrower than those of nucleus XII. The vessels in the neuropil of the corpus striatum are relatively narrow, as in the other amphibians, but in this case those among the cells of the corpus striatum are equally narrow.

RICHNESS OF THE CAPILLARY SUPPLY

The total length of the capillaries in a unit volume of tissue was measured in the same regions previously studied in *Ambystoma* and in three additional regions representing the hypothalamus. These parts were identified on the basis of the descriptions of Herrick ('27, '30, '33, '34). The results are presented in Table 1 and are shown graphically in Fig. 1. In the study of these, it must be borne in mind that only the larger differences are statistically significant, the probable errors being

so large. For instance, the values for the granular layer of the cerebellum, the cellular parts of the primordium piriforme and the corpus striatum, and the three regions of the hypothalamus do not differ significantly. All differences of over 100 (and some of less) in the uncorrected averages—the third column from the right in Table 1—are statistically significant.

If the averages are compared with those for *Ambystoma* ('39, p. 23), it becomes obvious at once that the vascular supply is much poorer than in the latter, being in many cases roughly about half as great or even only a third (even allowing for the fact that the corrected averages for *Ambystoma* may be a little too high, as indicated above). The neuropil of the primordium piriforme approaches nearest to being equally well vascularized in the two animals. This implies, of course, that there is a still greater difference in the same direction from the frog in all except the neuropil areas. Figure 1 shows clearly the intermediate position of *Ambystoma* between the other two amphibian species. In the plotting of the values, the hindbrain, the hypothalamus, and the cerebral hemispheres were treated separately and the parts in each were arranged in order of increasing capillary richness in *Necturus*. A glance at the figure makes it clear that in the hindbrain the relations between the parts are nearly comparable in *Ambystoma* and in the frog, but differ somewhat in *Necturus,* where, moreover, the differentiation among these parts is considerably less. On the other hand, the forebrains of *Ambystoma* and *Necturus* are very similar in respect of the quantitative vascular relationships, while the frog falls into line a little less closely, particularly in respect of the neuropil (*G* and *H* on the graph). In the paper on the frog ('38 *d*), the values for that animal are compared graphically with those for the dogfish and for newborn and adult albino rats. Comparison of the graph with that here produced will show how the tailed amphibians fit into this series, with the brain of *Necturus* ranking as the least vascular in all parts studied and *Ambystoma* standing very close to the dogfish—in most places being slightly richer. Further, the ratfish brain ('27) is as rich in many parts as the frog's. Thus elasmobranchs and amphibians, as classes, evidently do not differ widely in general cerebral vascularity.

The neuropil of the primordium piriforme is remarkable in *Necturus* in that it is as richly supplied with capillaries as the

nucleus of nerve VIII, which, in most animals studied, is the richest region found with the exception, in mammals, of the paraventricular and supraoptic nuclei of the hypothalamus

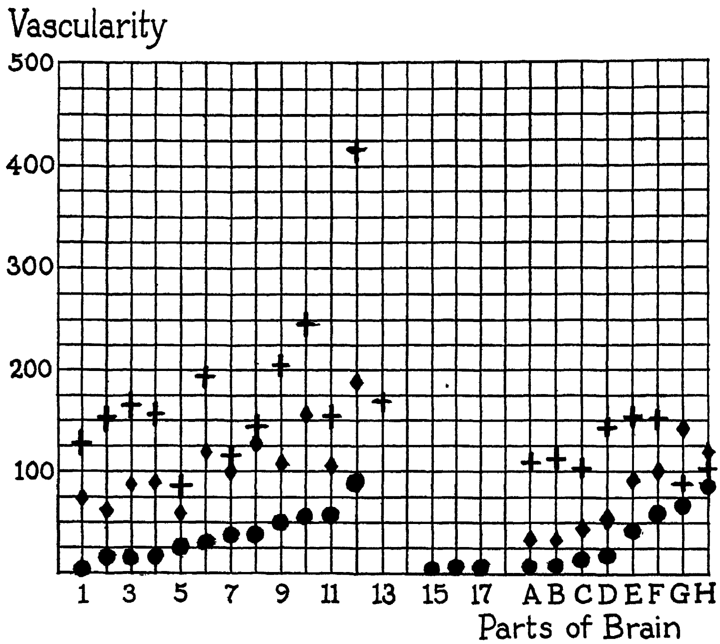

FIG. 1. The length in μ of the capillaries in 100^3 cu. μ of fresh tissue in various parts of the brains of amphibians.

● *Necturus* ◆ *Ambystoma tigrinum* + *Rana pipiens.*

1. Cerebellum: granular layer
2. Nucleus fasciculi solitarii
3. Eminentia cerebelli ventralis
4. Nucleus spinalis V
5. Fasciculus longitudinalis medialis
6. Nucleus motorius V
7. Nucleus XII
8. Cerebellum: molecular layer
9. Nucleus motorius VII
10. Nucleus sensorius principalis V
11. Nucleus motorius dorsalis X
12. Nucleus VIII (Cochlear nucleus of frog)
13. Vestibular nucleus
15. Pars ventralis hypothalami
16. Pars dorsalis hypothalami
17. Nucleus preopticus magnocellularis

A. Corpus striatum: pars ventralis
B. Primordium piriforme
C. Nucleus dorsolateralis amygdalae
D. Primordium pallii dorsalis
E. Nucleus medialis septi
F. Primordium hippocampi
G. Neuropil of corpus striatum
H. Neuropil of primordium piriforme.

(Finley, '39; Craigie, '40). The neuropil of the corpus striatum, while exceeding the cellular portion of this body in vascularity even more markedly than in *Ambystoma,* is not so rich in proportion to the other parts, being about on a par with the primordium hippocampi and only about three-quarters as rich

as the neuropil of the primordium piriforme. Whether or not the differences in vascular richness between the areas of neuropil correspond with differences in the number of synapses occurring, there does not appear at present to be any satisfactory way of determining. The marked relative poverty of the corresponding neuropil in the frog, however, in contrast with its relative richness in *Necturus,* makes it appear improbable that any such simple relationship exists, as was pointed out in the discussion of the frog.

It was noted previously ('39, p. 25) that in both *Ambystoma* and the frog the molecular layer of the cerebellum is richer than the graular layer and that this seems to parallel the richness of the forebrain neuropil, while contrasting with conditions in other classes of vertebrates. This situation is even more marked in *Necturus,* where the granular layer of the cerebellum is one of the least vascular parts found in the whole brain.

In the case of *Necturus,* a striking feature of these relationships is not merely the vascular richness of the neuropil but the simultaneous extreme poverty of the corresponding cell masses, all of which are considerably less well supplied with capillaries than even the white matter of the medial longitudinal bundle. The almost complete absence of loops running among the cells of the corpus striatum was described above and was illustrated in a photograph of a section from *Ambystoma* in the former paper ('39). This nearly avascular condition is of interest as supporting the theory enunciated by Herrick in 1921, according to which the first terrestrial vertebrates (one of the closest approximations to the brain of which among living animals is probably presented by *Necturus*) were of the type having thin-walled evaginated cerebral hemispheres because in times of critical oxygen deficiency such could survive by the diffusion of the gas from the cerebrospinal fluid.

These reflections raise the questions as to how far an effective diffusion of oxygen and carbon dioxide through the tissue substance can take place and to what extent the cerebrospinal fluid functions normally in the aeration of the brain tissue in amphibians. Krogh ('29) found that the longest distance an oxygen molecule has to go in the muscle of the frog is 28 μ from the center of a capillary, and the longest distance in the muscle of the dog 11 μ. In the corpus striatum of *Necturus* many cell

bodies are three hundred microns or more from the nearest capillaries and many are at least half that distance from either a capillary or the ventricular surface. One is tempted to wonder whether the cells are enabled to survive by the trophic function of their dendrites as suggested by Golgi ('06), by Kappers ('22), and by Kappers, Huber, and Crosby ('36), as these ramify extensively in the white matter where capillaries are more numerous, though any close association of dendrites with capillaries such as would be expected on this supposition was strongly denied by Cajal ('29).

The palaeostriatum, which is the part of the corpus striatum complex of higher vertebrates represented by the region studied in *Necturus,* is apparently more or less poorly vascularized throughout the phylogenetic series, Campbell ('39) having found it to be one of the poorest parts among those which he studied in the cat and a comparable condition existing in the dogfish (Craigie, '28). The poverty, however, is more marked in the *Caudata* than in the frog. If the cells of the striatum of the salamander were nourished largely through their dendrites, which ramify in the relatively richly vascular neuropil, the extension of a capillary net among the cell bodies would make such function of the dendrites less important and might account for the relative vascular poverty of the neuropil in the frog in contrast with its richness in *Ambystoma* and *Necturus.*

The poor capillary supply of the hypothalamus in all its parts is another feature worthy of note. In the rat ('40) the hypothalamus contains centers of extremely wide difference in vascular richness, the paraventricular and supraoptic nuclei, as in the cat (Finley, '39), being very much richer than any other parts of the brain, while the mamillary nuclei rank with the richest parts elsewhere and the dorsomedial and ventromedial nuclei are among the poorest areas of gray matter. These facts contrast with the conditions in the hypothalamus of *Necturus* (Fig. 2) the vascularization of which is uniformly poor. While the large-celled paraventricular and supraoptic nuclei are phylogenetically old, as several writers on the hypothalamus have emphasized, being represented by the nucleus preopticus magnocellularis of fishes and amphibians, they are not only structurally undifferentiated in the latter, but are evidently also much less metabolically active. The length of the capillaries in a unit

volume of these nuclei is two hundred times as great in the rat as in *Necturus*. In view of the probable connections, structural and functional, between these nuclei and the pars nervosa of the hypophysis, this poor development is interesting when considered in association with the weakly developed pars nervosa of *Necturus* and its purely superficial vascular net ('38 *c*).

FIG. 2. Photomicrograph of part of transverse section 200 μ thick passing through the nucleus preopticus magnocellularis (*pm*) of *Necturus*. × 50.

Similar backwardness in functional differentiation appears to be indicated by the vascular poverty of the pars dorsalis hypothalami, which is the forerunner of the richly vascular mamillary nuclei of the mammal.

Volumes and Areas of the Capillaries

The volumes of the capillaries in a unit volume of tissue and the surface areas of the same capillaries in the seven centers where diameters were measured are listed in Table 2 and are represented graphically in Figs. 3 and 4. The relative vascularity of the various parts when expressed in these terms does not show important divergences from the relations found in terms of capillary length, though the differences between the parts are, of course, greater.

However, comparison (Fig. 3) of the volume of the vessels in *Necturus* with the corresponding volume in the other animals does present a picture considerably different from that afforded by comparison of the lengths in these species, as a result of the

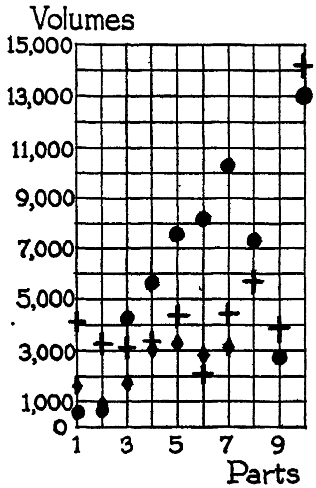

FIG. 3

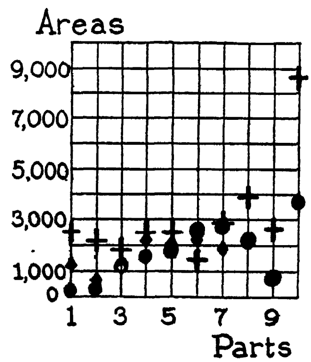

FIG. 4

FIG. 3. Volumes in cu. μ of capillaries in 100^3 cu. μ of fresh tissue in parts of the brains of amphibians.

FIG. 4. Areas in sq. μ of capillaries in 100^3 cu μ of fresh tissue in the same parts.

● *Necturus* ◆ *Ambystoma tigrinum* + *Rana pipiens.*

1. Cerebellum: granular layer
2. Corpus striatum
3. Fasc. long. medialis
4. Cerebellum: molecular layer
5. Nucleus XII
6. Neuropil of corpus striatum
7. Primordium hippocampi
8. Nucleus motorius VII
9. Primordium pallii dorsalis
10. Nucleus VIII (Cochlearis of frog).

The values for *Necturus* in the last three centers were calculated by use of the average capillary diameter (13.6 μ).

great caliber of the capillaries in the mud puppy. No longer do we find *Necturus* to be the most poorly supplied with blood of the three amphibians, except in the very poorest regions. Thus the granular layer of the cerebellum and the cellular corpus striatum still occupy this low rank. Other parts, however, contain twice or even three times as much blood as they do in *Ambystoma*—much more, even, than in the frog. The neuropil of the corpus striatum, the richness of which has already been discussed, has even four times as great a volume of capillaries as has that of the frog. The VIII nerve nucleus approaches its differentiated

and richly vascular derivative, the cochlear nucleus of the frog, and much surpasses the vestibular nucleus of the latter.

On the other hand, since *Ambystoma* has capillaries which average slightly narrower than those of the frog, their volumes in a unit of tissue, like their lengths, are somewhat less in the former animal than in the latter.

If the areas of the walls of the capillaries are considered (Fig. 4), the three species are found to be more nearly alike, the frog having the largest area in most parts and the two tailed amphibians being rather similar. Thus in *Necturus* a much larger volume of blood is exposed to a more or less similar area of wall through which the physiological exchange of substances may take place, so that if other factors were equal the oxygen:carbon dioxide ratio in the blood of *Necturus* would change much less rapidly than in the other species. The average area to which one cu. mm. of blood is exposed in the brain of *Necturus* (299 sq. mm.) is less than half as great as in the frog (654 sq. mm.), while that in *Ambystoma* (696 sq. mm) is greater still. It may be noted that Smith ('25) found the carbon dioxide production of *Rana pipiens* to be twice as active as that of *Necturus maculosus,* but observations on the rate of metabolism in the brains of these species are not available.

The area covered by one cu. mm. of blood in the brain of the frog is about the same as in that of the dogfish and only a little more than half as great as in that of the rat. The volumes in a unit volume of tissue in the frog are in most cases somewhat less than in the rat (greater in the medial longitudinal bundle, practically equal in the nucleus XII, two-thirds as great in the granular layer of the cerebellum). In *Necturus,* except in the specially poor regions already noted, the volumes are greater than those found anywhere else, considerably greater than in even the rat.

The areas in the frog are much less than in the rat, but greater than in the dogfish (except in the cerebellum, where they are about equal). Even in *Necturus,* with the exceptions indicated, the areas are larger than in the dogfish.

Loop and Network Mechanisms

The studies on amphibian brains have failed to reveal any evident quantitative difference in vascularization associated with the two morphological types of capillary mechanism. In all

classes where the loop arrangement has been observed (Cyclostomata, Amphibia, Reptilia, Mammalia), it occurs in more generalized (whether primitive or retrogressive) members of each. While the relation is not yet established as an absolute rule, the indications are that a generalized condition of the brain is associated with a relatively poor capillary supply and relatively little differentiation of the parts in respect of vascularity, whether the generalized condition is the result of the phylogenetic or of the ontogenetic status of the animal considered. Hence it is clear that the loop type of capillary mechanism has been observed so far only in members of their respective classes which have relatively poorly vascular brains. As exact quantitative measurements in loop mechanisms have not yet been made except in amphibians, it remains to be seen whether or not similar studies elsewhere will bear out this deduction.

The present investigation has not thrown any new light on the puzzling question of the phylogenetic relationship between the two types of capillary system.

Summary

1. The caliber of the cerebral capillaries in *Necturus* is larger than in any other animal where it has so far been measured, averaging 13.6 μ.

2. The two limbs of a capillary loop are frequently unequal in caliber and the tip of the loop often appears somewhat swollen.

3. The hypoglossal nucleus has rather wide capillaries and the neuropil rather narrow ones.

4. The length of the capillaries in a unit volume of tissue is the least yet found in any animal, being only about one-third to one-half as great as in *Ambystoma,* which is intermediate between *Necturus* and the frog. Also *Necturus* shows less differentiation of the parts in respect of this character.

5. The neuropil of the primordium piriforme and that of the corpus striatum are remarkably rich, the former being one of the richest parts found in the brain and being comparable with nucleus VIII. The molecular layer of the cerebellum is also much richer than the granular layer.

6. The cellular part of the corpus striatum and the granular layer of the cerebellum are almost avascular, a condition giving rise to speculations regarding transport of essential substances

to and from them. Herrick's theory regarding the reason that terrestrial vertebrates originated among animals with thin-walled, evaginated cerebral hemispheres is recalled, as are discussions regarding trophic functions of dendrites.

7. The hypothalamus is exceedingly poorly vascularized throughout and the magnocellular preoptic nucleus shows no foreshadowing of the remarkable capillary richness of its mammalian derivatives.

8. The volume of the capillaries in a unit volume of tissue is greater in most parts than in the other amphibians, or in any animal hitherto studied, but a cubic millimeter of blood is exposed to a much smaller area of capillary wall than in the other cases.

9. The area of the walls of the capillaries in a unit volume of tissue is similar in *Necturus* and *Ambystoma* and not a great deal larger in the frog. It is less than in the rat but greater than in the dogfish in most regions.

10. No definite quantitative difference in vascularization appears to correspond with the morphological difference between loop and network types of capillary mechanism but the former type usually occurs in somewhat generalized brains, which tend to be relatively poor in capillaries.

11. Amphibians and elasmobranchs, as classes, do not differ widely in general cerebral vascularity.

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STUDIES ON DESTRUCTION OF RED BLOOD CELLS

Relation of Increased Hypotonic Fragility and of Erythro-stasis to the Mechanism of Hemolysis in Certain Anemias

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ABSTRACT

The more nearly spherical are erythrocytes, the greater is their susceptibility to hemolysis by swelling in hypotonic solutions. There has been no satisfactory explanation, however, for the increased destruction of red blood cells of such increased "fragility" *in vitro* (*e.g.*, congenital hemolytic jaundice) under presumably isotonic conditions *in vivo*.

It has now been demonstrated that sterile incubation *in vitro* and intravascular stasis *in vivo* produce progressive swelling of erythrocytes, spheroidicity and increasing osmotic fragility, such that hemolysis eventually occurs in isotonic solutions. The swelling *in vitro* of abnormally fragile erythrocytes progressed at a rate similar to that of normal cells but hemolysis occurred sooner, presumably because the spherical form was reached earlier. These effects apparently were related to metabolic changes. No hemolytic agents were demonstrated, nor was increase in osmotic fragility associated with the hemolysis produced by lytic agents, such as saponin and anti-human red blood cell immune serum.

It is believed that intravascular stasis, normally occurring in the spleen and other organs, is the immediate mechanism resulting in increased blood destruction in hemolytic anemias characterized by increased fragility of the erythrocytes, namely, congenital hemolytic jaundice, icterus neonatorum, and the acute hemolytic anemias caused by arsine and sulfanilamide. Histologic and physiologic studies of the spleen indicate its function as a stasis organ, and so account for the beneficial effect of its removal in congenital hemolytic jaundice despite the continued abnormal fragility of the erythrocytes.

Theoretically, therefore, increased intravascular stasis *in vivo* should be associated with increased destruction of blood. Stasis in the spleen induced by prolonged nembutal anesthesia in animals caused the osmotic fragility of red blood cells in the spleen to increase to the point that hemolysis occurred. Generalized intravascular hemagglutination produced in animals by concanavalin A (Sumner) caused similar changes in splenic blood and in the circulating blood with obvious hemoglobinemia and hemoglobinuria. It is believed that increased intravascular

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stasis accounts for certain hemolytic anemias in which hemolysins are not demonstrable but which are associated with: (*a*) intravascular agglutination from transfusion reactions, from type XIV anti-pneumococcus horse serum, in certain cases of Lederer's anemia and in cases associated with rouleau formation due to increased plasma globulins (hemolytic crises with infections); (*b*) increased blood viscosity in sickle cell anemia and possibly in acute infections; (*c*) increased local stasis in "hypersplenic hemolytic anemia" and in passive congestion of cardiac decompensation.

THE more nearly spherical are erythrocytes, the greater is their susceptibility to hemolysis, or their "fragility," in hypotonic solutions of sodium chloride (Gänsslen (1922), Haden (1934), Castle and Daland (1937)). In certain hemolytic anemias occurring in human beings, the increased rate of destruction of red blood cells is associated with such increased spheroidicity and abnormal fragility of the erythrocytes *in vitro,* when compared with normal human erythrocytes. Red blood cells, however, are not exposed to hypotonic solutions *in vivo* and, therefore, the erythrocyte abnormality, detected *in vitro* by the "fragility test," has never been satisfactorily related to the increased destruction of blood in these anemias. Furthermore, in congenital hemolytic jaundice removal of the spleen may abolish the anemia without changing materially the abnormal fragility of the red blood cells. The spleen in this disease, therefore, is apparently essential to the hemolytic process *in vivo.* The purpose of this preliminary communication is to demonstrate the existence of a mechanism whereby erythrocytes are hemolyzed by osmotic forces *in vivo,* and to establish the causal relation of intravascular stasis in the spleen and other tissues, and of increased fragility of erythrocytes, to the increased rate of destruction of red blood cells in certain hemolytic anemias.

A normal function of the spleen, that of storage of blood, is known to produce intravascular stasis (erythro-stasis) and concentration of the erythrocytes by removal of plasma (erythro-concentration), Knisely (1936). When an attempt to imitate intravascular stasis was made *in vitro* by the sterile incubation at body temperature of whole defibrinated mammalian blood, the red blood cells showed progressive increase in volume, spheroidal erythrocytes, and an increase in fragility to such a degree that hemolysis eventually occurred in the serum. A portion of the erythrocytes removed at the termination of such an experiment showed hemolysis in isotonic salt solution in the fragility test. When blood from a patient with hemolytic jaundice, show-

ing an initial increase in fragility of the erythrocytes, was similarly incubated, the same progressive changes occurred except that less swelling was required by these cells to reach sphericity and consequently hemolysis began at an earlier time (12 hours) than with human erythrocytes of normal fragility (32 hours). When, in an attempt to imitate both the erythro-stasis and the erythro-concentration occurring in the spleen, normal red blood cells were initially separated from the serum by centrifugation and then incubated, the above changes progressed at a strikingly more rapid rate than in samples of normal blood in which the red blood cells were suspended or mixed continuously in the serum. It appeared reasonable, therefore, to infer that these observations *in vitro* might indicate a mechanism by which red blood cells are destroyed in such organs as the spleen.

If the above changes in fragility observed *in vitro* were to have validity, however, when applied to the living subject, it was essential to demonstrate *in vivo* that normal red blood cells would develop increased fragility when subjected to an abnormally increased degree of intravascular stasis. Accordingly, prolonged nembutal anesthesia (6 to 7 hours) was employed in dogs to produce splenomegaly and presumably intravascular stasis in the spleen. At the end of such experiments the spleen showed variable degrees of enlargement and blood obtained from the spleen showed a marked increase in the volume percentage of erythrocytes (erythro-concentration). Red blood cells obtained from the splenic vein or from the splenic pulp showed an increase in fragility which varied from slight to moderate in degree, when compared with the fragility of a sample obtained simultaneously from the vena cava. Slight hemoglobinemia, observed in the plasma of blood from the splenic vein, indicated hemolysis of a small percentage of the erythrocytes contained in the spleen. Similar observations have been made on the cat's spleen by Mellgren (1939). Complete obstruction for two hours of the inferior vena cava and of the splenic veins (arteries patent) produced a significant increase in the fragility of blood samples obtained from both sites.

Theoretically, an agent producing agglutination of erythrocytes should cause intravascular stasis by retarding blood flow in certain regions of the body. Such an agglutinating agent was found in concanavalin A, a crystalline protein derived from the

jack-bean (Sumner and Howell (1936)). Concanavalin A, *in vitro,* did not cause significant hemolysis or change in fragility of the erythrocytes. In a series of experiments on dogs and rabbits the injection of concanavalin A produced intravascular agglutination and erythro-concentration in certain organs as indicated by a significant initial fall of the percentage volume of red blood cells remaining in the peripheral circulation. This was followed by a progressive increase in the number of spheroidal erythrocytes and in the fragility of the red blood cells in the spleen, and even in the peripheral blood. Red blood cells from the spleen were significantly more fragile than those from the peripheral blood. When hemoglobinemia appeared in the animal, a portion of the red blood cells was shown to hemolyze in physiological salt solution in the fragility test. By appropriately varying the amount of concanavalin A it was possible to produce either an acute and fatal or a subacute hemolytic anemia with recovery. Experimental hemolytic anemias similarly characterized by increased fragility of the erythrocytes have been produced by Dameshek and Schwartz (1938) in guinea pigs by the injection of immune serum containing both agglutinating and hemolytic antibodies specific for the red blood cells of the guinea pig. Naturally, the resulting destruction of erythrocytes and the increase in their fragility were considered to be caused by the hemolysins in the immune serum. However, in experiments reported below, it was demonstrated that hemolysins derived from immune serum may hemolyze erythrocytes *in vitro* without significantly altering their fragility. Moreover, all the features of the experimental anemias caused by immune serums have now been reproduced by concanavalin A, an agglutinating agent which is non-hemolytic *in vitro.* Therefore, it is probable that the hemolytic anemia produced by immune serums was mainly due to intravascular stasis as a result of the agglutinins and was not caused by hemolysins in the immune serums.

These animal experiments thus suggest that the phenomena observed upon the sterile incubation of blood *in vitro* occur *in vivo* when intravascular erythro-stasis is induced.

Because of the following observations, the progressive swelling and increase in fragility are considered to be related to metabolic processes. Such effects were absent during 24 hours at ice box temperatures. When erythrocytes were resuspended

in fresh serum every two hours for 22 hours during incubation at body temperature, the increase in fragility was slight. The considerable increase in fragility, following 24 hours of incubation of erythrocytes in the original serum, was partly reversed by subsequent equilibration of samples with oxygen and was further reversed by resuspension of the erythrocytes in fresh serum together with such equilibration. The alteration in pH of a blood sample during 24 hours of incubation was from 7.5 to 6.91, and was accompanied by an increase in erythrocyte volume of 14 per cent. Such a shift in pH usually produces an erythrocyte volume increase of only approximately 4 per cent (Dill et al (1937)) and consequently is insufficient to account for the increase in volume observed. Accordingly, it is inferred that the increase during incubation of erythrocyte volume and of fragility are due to metabolic processes which produce an increase in the osmotically active constituents of the erythrocytes.

The progressive increase in fragility, resulting eventually in hemolysis, could not be shown to be related to hemolytic agents present in or derived from serum. In fact, these changes occurred upon incubation for 17 hours at body temperature of human red blood cells which were washed, resuspended, and mixed continuously in physiological salt solution. Similar effects were observed with washed erythrocytes resuspended and mixed continuously in plasma (heparin), serum (fresh), and in serum (heated 10 minutes at 56° C.). These facts specifically exclude the plasma or serum lysolecithin of Bergenhem and Fåhraeus (1936) and of Fåhraeus (1939) as the essential agent in the hemolytic mechanism described here. Furthermore it was demonstrated that hemolysins, such as saponin (see also Ponder (1937)) or immune serum, did not cause an increase in erythrocyte volume or in fragility when used *in vitro* at concentrations producing definite hemolysis.

When, as a result of osmotic forces, the spherical form is reached, erythrocytes have obtained the maximum volume possible without extension of their surface. Hemolysis is caused by any further increase in volume and consequently by extension of the surface (Castle and Daland (1937)). Obviously, the more nearly spherical is an erythrocyte the smaller is the increment of volume increase possible before the spherical form is reached. Accordingly, when such abnormal erythrocytes increase in vol-

ume during incubation at body temperature their critical point for hemolysis is reached with less volume increase and consequently at an earlier time than in similar experiments with normal erythrocytes. Theoretically, therefore, with similar degrees of intravascular stasis, abnormally fragile erythrocytes would be more readily hemolyzed *in vivo* than would normal erythrocytes. It has been observed by others (Heilmeyer (1936), Gripwall (1938) and Mellgren (1939)) as well as here that erythrocytes in the splenic blood of normal animals and of man are more fragile and more nearly spherical than those entering the spleen. The experiments of Knisely (1936) indicate that under normal circumstances erythrocytes may remain as long as ten hours in the spleen of the cat. Consequently, it is believed that the increased destruction of red blood cells in hemolytic anemias which are characterized by increased fragility of the erythrocytes is caused by a "normal" degree of intravascular stasis occurring in the spleen and other organs. Thus, a mechanism is now proposed to account for the increased blood destruction in hemolytic anemias, such as congenital hemolytic jaundice, icterus neonatorum (Goldbloom and Gottlieb (1929)), and the acute hemolytic anemia caused by arsine (Kiese (1937)) in which the erythrocytes are abnormally susceptible to hypotonic solutions *in vitro*. This mechanism presumably was operative as well in five cases of severe acute hemolytic anemia observed to follow the administration of sulfanilamide. In these patients the erythrocytes showed a transient but striking increase in fragility and the maximum blood destruction occurred when a portion of the red blood cells in venous samples were susceptible to hemolysis *in vitro* in isotonic or slightly hypotonic solutions of sodium chloride.

The above diseases are characterized by erythrocytes of abnormally increased fragility and by intravascular stasis which is presumably of "normal" degree. Conversely, as was experimentally demonstrated above, abnormally increased intravascular stasis of normal erythrocytes in certain organs may induce increased destruction of blood. Likewise, in human subjects, an unusual degree of erythro-stasis might account for certain hemolytic anemias in which an abnormal increase in fragility of the erythrocytes is absent or only secondary. Thus, increased intravascular stasis in congestive heart failure may account for

the increased fragility and destruction of the red blood cells noted by others (Waller (1939)). The effect of splenectomy in congenital hemolytic jaundice is certainly to remove effective intravascular stasis in a large organ. As a result, blood destruction is materially reduced although the abnormal erythrocyte fragility persists. The explanation for certain cases of so-called "hypersplenic anemia" (Heilmeyer and Albus (1935)) is possibly an increase of the normal function of the spleen with respect to erythro-stasis. Analogous to the experiment with concanavalin A derived from the jack-bean would appear to be clinical instances of hemolysis following the injection of agglutinating agents for red blood cells. Thus, it may be more than a coincidence that an agglutinating agent for red blood cells has been obtained from the bean *Vicia fava,* the ingestion of which is responsible for the hemolytic anemia of favism. In severe hemolytic transfusion reactions following the administration of incompatible blood, in which only a low concentration of isohemolysins is demonstrable *in vitro,* a manifestly high titer of iso-agglutinins is suggested as the responsible factor (Davidsohn (1939)). Similarly, in a few instances, the injection of type XIV antipneumococcus horse serum containing only small amounts of hemolysins, has produced severe hemolytic crises presumably because of a demonstrably high concentration of agglutinins for human red blood cells (Finland and Curnen (1938)). Certain cases of acute hemolytic anemia (Lederer's?) have been reported (Greenwald (1938)) in which spontaneous agglutination of the erythrocytes was observed at room temperature due to cold agglutinins. In the present investigation such a patient showed etreme auto-agglutination of the erythrocytes *in vitro* at body temperature and a high concentration (1:1025 dilution) of cold agglutinins in the serum. As in the experiment with concanavalin A, during the acute period hemoglobinemia and hemoglobinuria the erythrocytes in circulation showed a striking transitory increase in fragility. No hemolysins were demonstrated.

Theoretically, an increase in blood viscosity should cause an increase in erythro-stasis in the capillary bed. With the onset of an acute infection the rise in plasma globulins measurably increases the plasma viscosity and, as reflected by an increased sedimentation rate, causes an increased rouleaux formation or pseudo-agglutination. These phenomena are tentatively sug-

gested as an explanation for the well known fact that crises of blood destruction are often initiated by infections in congenital hemolytic jaundice and in other hemolytic anemias including sickle cell anemia. In sickle cell anemia, the present experiments disclose a mechanism which probably induces a vicious cycle of erythro-stasis and which, accordingly, may well explain the crisis of blood destruction and the frequency of thrombotic phenomena and of total infarction of the spleen (Diggs and Ching (1934)). When erythro-stasis takes place, reduction of the oxygen content of the erythrocytes is inevitable. In sickle cell anemia this is the condition essential for inducing sickling (Hahn and Gillespie (1927)). Experiments with the blood of patients with this condition showed that complete reduction with a mixture of nitrogen and carbon dioxide produced extensive sickling and an increase of viscosity of the blood by 120 per cent compared to an increase of four per cent for a normal blood sample, as measured in an Ostwald viscosimeter. This striking observation suggests a mechanism whereby erythro-stasis and consequently hemolysis may account for the increased blood destruction in these patients. Further studies of the experimental and clinical applications of the observations included in this preliminary report are in progress.

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THE NEUROMOTOR SYSTEM OF *OPISTHONECTA HENNEGUYI* (FAURÉ-FREMIET)

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ABSTRACT

Opisthonecta henneguyi is an unattached free-swimming vorticellid ciliate protozoan which swims with adoral zone and mouth at the rear. It has no known attached stage. Its neuromotor system is an integrated fibrillar apparatus concerned with ciliary activities, changes in the shape of the body, and retraction and contraction of the epistomal and aboral regions. The body has an adoral spiral of two undulating membranes composed of fused cilia in 1¼ spiral turns around the epistome in a counterclockwise spiral, with only the inner membrane continuing down the pharynx. The aboral membranelles form a circle around the body near the aboral end.

The neuromotor system consists of a motorium on the wall of the pharynx joined by fibers from the outer and inner undulating membranes of the adoral zones of cilia, with connections to sphincter and collar fibers around the oral end and to epistomal retractor fibers uniting in an apical epistomal ring with connections aborally to the aboral ring of membranelles. From this ring radial fibers converge over the aboral end in an antapical aboral ring. In the pharyngeal region a spiral pharyngeal membrane runs aborally into the endoplasm. From the motorium a longitudinal pharyngeal fiber spirals down the pharynx opposite the pharyngeal membranes and through the endoplasm terminating on the aboral radial fibers. All cilia and contractile fibers are integrated in one system. The silver line structures of the surface seem only to be related directly to the pellicle.

INTRODUCTION

THE Vorticellidae were among the first Protozoa recognized as possessing fibrils but to date no neurofibrils have been assigned to this group. The fibrillar elements have been considered as myonemes, functioning in the active contractions of the body and stalk. In the light of recent investigations, the existence of a neuromotor system is indicated. Coordination of feeding and locomotor activities is well established and one would expect a coordinating mechanism for the regulation of reactions to stimuli maintaining this ordered activity, especially since this is the case in other groups of Protozoa.

In searching for an organism suitable for investigating this hypothesis, the free-swimming peritrich, *Opisthonecta henneguyi,* was found to be available and promising. This form lends itself readily to such an investigation since it is easily cultured and stains well. It is a fairly good-sized organism and is nearly transparent.

Thanks are due to Dr. A. E. Noble for loaning slides of *O. henneguyi* for comparison in determination of the specific status of our organism. We are especially indebted to the American Philosophical Society for a grant-in-aid which made this investigation possible. Assistance rendered by the personnel of Works Projects Administration O. P. No. 65–1–08–113, Unit "C," is acknowledged.

Materials and Methods

The form used by the authors was isolated from Strawberry Creek on the campus of the University of California at Berkeley in May, 1935 and has been kept continuously in culture since. It maintains itself and reproduces vigorously in a balanced salt solution containing a little hay infusion and wheat grains for the organic matter necessary for bacterial growth.

Our form differs slightly from *O. henneguyi* as described originally by Fauré-Fremiet (1906, 1923) from Cette and Woods Hole and again by Lynch and Noble (1931) from Berkeley, but comparison of our form with that of Lynch and Noble fails to reveal differences sufficient to give it separate specific standing. The variance is chiefly in size and number of contractile vacuoles. Fauré-Fremiet states that there are many contractile vacuoles disposed around the pharynx into which they empty. Lynch and Noble find three and occasionally four. Our material shows two and only occasionally three.

The best preparations have been those fixed in Schaudinn's fluid with 5 per cent acetic acid, followed by Heidenhain's long method of staining with iron hematoxylin. In our case the organisms have been left in the mordant for 1½ hours and in the hematoxylin for 3 hours. This has been effective for both sectioned material and whole mounts. A modified Dobell's alcoholic hematoxylin method used after Schaudinn's fluid is also good. Zenker's fixation followed by Mallory's triple stain as modified by Sharp (1914) has not contributed materially to demonstrating the neuromotor system, although the mitochon-

drial-like bodies in the surface are clearly shown by this technique. Euparal was found to be the ideal mounting medium. The silver methods of Klein (1926) and Gelei and Horvath (1931) have manifested an argentophilic surface pattern. The living organism was best studied after vital staining with Brilliant Cresyl Blue. Osmic acid fixation was used to determine the structure of the adoral ciliation and of the locomotor band. Nigrosin was used as a relief stain.

Habits and Activities

Opisthonecta is constantly in motion. It is extremely difficult to watch a living specimen because of this fact. Evèn if a specimen is caught in the meshes of lens paper or cotton, the beating of the membranelles and the constant rotation of the body make vital observations a real task. The organism swims with its aboral end foremost, just as do the swarmer stages of the stalked vorticellids, rotating in a clockwise fashion as it progresses. Counterclockwise rotation may occur for short periods. The actual reversal of ciliary motion has not been observed.

The organisms tend to inhabit the lower part of the culture dishes, occasionally settling to the bottom either on the oral or aboral end and continuing to rotate. Frequently they come to the top of the fluid and repeat this process there. At the initiation of encystment they generally come to the surface. When encystment is complete the cysts sink to the bottom.

Ciliary waves in the adoral zone are initiated at the outer or terminal end of the adoral undulating membranes, and continue to the inner end of the cytopharynx. The waves of the locomotor band seem to have no localized point of origin, but continuously circle the body. At any one moment there are usually ten waves in progress around the band as shown after osmic acid fixation in which the membranelles retain their wave formation. The waves progress around the body in a clockwise fashion (observed from oral end). The epistomal membrane is independent of the adoral membranes. It is not immobile as was stated by Lynch and Noble but is constantly flickering back and forth as though it helped to shunt food particles into the pharynx.

The body of *Opisthonecta* is capable of some longitudinal contraction. The epistome can be entirely withdrawn within the

body by means of the longitudinal fibrils, at which time the collarette closes over it by the contraction of a set of sphincter fibrils. When the epistome is contracted, the adoral membranes cease to move, but the spiral movement of the pharyngeal membrane continues. The contraction of the epistome is so sudden that it is impossible to determine whether or not the ciliary activity ceases before contraction occurs. However, at relaxation the epistome is entirely protruded before the ciliary action is resumed. The ciliary action within the pharynx is never stopped except in well advanced cysts.

The food of this ciliate consists principally of bacteria which are swept into the pharynx. At the inner end of the pharynx there is an ampulla, or enlargement. It is here that the food particles are rounded into a ball, preparatory to cyclosis in the cytoplasm. Greeff (1870–71) states that in *Epistylis flavicans* and *E. plicatilis* the food is sent from the ampulla into the gullet which is a prolongation from the inner end of the pharynx, thus liberating vacuoles near the aboral end of the body. The authors have not been able to confirm this procedure in *Opisthonecta henneguyi* although a gullet-like prolongation is clearly present (Fauré-Fremiet, 1923; Lynch and Noble). The food vacuoles are seemingly set free from the ampulla and follow along the path of this prolongation which acts as a guide. The gullet-like prolongation is a fibrillar structure and its terminations are seen only in stained specimens.

The food vacuoles circulate in a definite path, as was shown for *Epistylis flavicans* and *E. plicatilis* by Greeff, and finally empty into the cytopyge which is a temporary structure near the oral opening of the cytopharynx.

During division the epistome and collarette remain contracted with apparently no feeding occurring at this time. The dividing organism spirals clockwise with occasional counterclockwise movement. Details of division have not been studied.

Morphology

General

The description of *Opisthonecta henneguyi* which follows is that based wholly on the study of our material. We designate the ends as oral and aboral rather than anterior and posterior, respectively, but orient our figures with the oral end up or an-

terior for comparison with published figures of vorticellids, although in locomotion by swimming the oral end is posterior, as generally in free-swimming stages of other vorticellids. At no time has any attached stage of this species been observed by us or reported by others.

Opisthonecta henneguyi is a relatively large vorticellid, a distinct advantage in studying the neuromotor system. Measurements of length and greatest diameter of 27 specimens fixed in Schaudinn's fluid show averages and ranges of 97 μ (81 to 114) and 76 μ (69 to 81), respectively. Fauré-Fremiet (1923) reports the length of this species as 150 to 170 μ. This and our failure to verify Fauré-Fremiet's (1923) finding of an undulating membrane above the aboral zone of membranelles are the only notable differences between his account of the species and ours. They seem inadequate without further investigation to justify specific separation. The unfixed specimen is only slightly longer due to the relaxed condition of the myonemes.

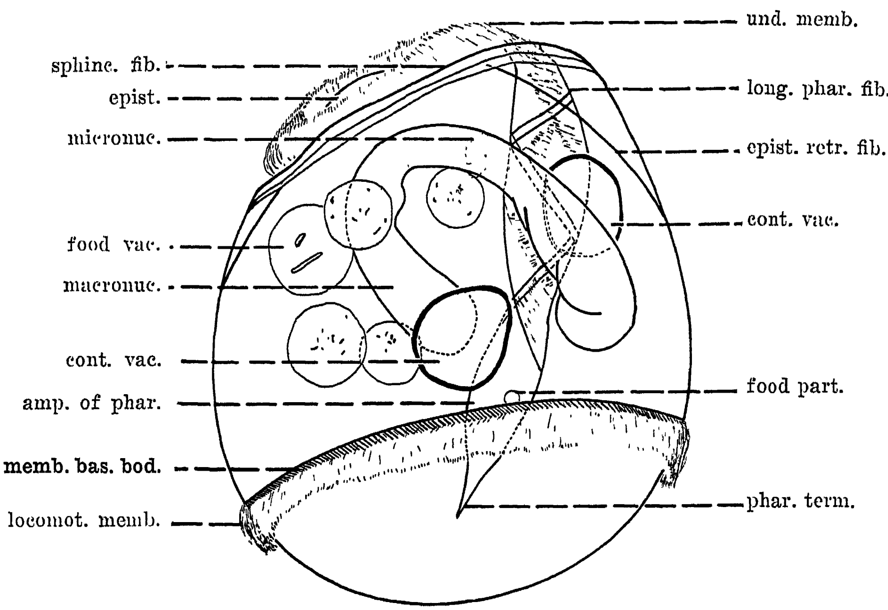

FIG. *A*. Camera lucida outline of *Opisthonecta henneguyi*. Schaudinn's fixative and Heidenhain's iron hematoxylin. × 900. Abbreviations: *amp. of phar.*, ampulla of pharynx; *cont. vac.*, contractile vacuole; *epist.*, epistome; *epist. retr. fib.*, epistomal retractor fibril; *food part.*, food particle; *food vac.*, food vacuole; *locomot. memb.*, locomotor membranelles; *long. phar. fib.*, longitudinal pharyngeal fibril; *macronuc.*, macronucleus; *memb. bas. bod.*, membranelle basal bodies and circular connective; *micronuc.*, micronucleus; *phar. term.*, pharyngeal terminus; *sphinc. fib.*, sphincter fibrils; *und. memb.*, undulating membranes.

The shape is that of a cylinder, somewhat truncate at the oral end, the aboral end being a bit larger and dome-shaped. Adoral cilia 10 μ long are fused into 2 undulating membranes running side by side and encircling the epistomal disc or cone, one and one-quarter times in a counterclockwise spiral before entering the cytopharynx. There is only a single undulating membrane within the pharynx formed by a continuation of the inner membrane of the epistomal disc. It spirals down the pharynx making two turns before reaching its inner end. There is an epistomal membrane about 20 μ in length, twice as long as the adoral membranes, at the opening of the pharynx borne on a short ridge on the ventral edge of the cytostome with its long cilia standing off at an angle of 45°. This epistomal membrane is wholly independent of the adoral membranes.

The aboral membranelles form a single zone of locomotor organs encircling the body at about 20 μ, or one-fourth the total length from the aboral pole. They range from 15 μ to 22 μ in length. A separate undulating membrane on the aboral border of the aboral membranelles was not found as reported by Fauré-Fremiet (1923) in his form of *O. henneguyi*. Lynch and Noble (1931) make no mention of this membrane in the material they investigated. However, we find that the basal bodies of the membranelles are in two groups, the set on the adoral side composed of five rows and that on the aboral of but a single one, which is suggestive of an undulating membrane, or some form of separate activity of this row.

The cytopharynx arises at the cytostome and lies obliquely in the cytoplasm at about 45° from the axis; its inner end is near the central axis of the body. From the inner end of the pharynx there is a gullet-like continuation in the endoplasm which can be traced for a short distance in the living specimen.

The functions of the several ciliary regions are as follows: the double row of membranes of the adoral zone picks food particles from the surrounding water and moves them along to the cytostome. The pharyngeal spiral membrane zone whirls the particles down the cytopharynx and with the help of the peniculus forms a bolus of them at its inner terminal end in the endoplasm. The epistomal membrane forms a deflector which turns the particles carried in the current created by the adoral membrane zones into the cytostome and down the pharynx. The

adoral membranes give rise to the spiral rotation of the body. The aboral zone of membranelles is the locomotor mechanism which propels the body forward, *i.e.*, in the aboral direction. Owing to the oblique set of the individual membranelles in the zone they probably also assist in spiral rotation.

Two large contractile vacuoles lie near the pharynx, and both empty into it. They are permanent in location and function independently of one another, each pulsating on an average of once every 48 seconds. The cytopyge is an obscure temporary exit near the cytostome visible only at the moment when wastes are being discharged.

The macronucleus is typical of the Vorticellidae. It is about 125 μ long, roughly of a horseshoe or open-ring shape, and circular in cross section. It lies in the oral half of the body and curves in the transverse plane. It varies considerably in both size and shape. The cytopharynx passes through the middle of the horseshoe. A small vesicular micronucleus lies free in the cytoplasm near the cytostome, and migrates from this position only at times of division, when it is to be found at the aboral end of the body.

The surface of the body is annulated throughout, except on the epistomal disk, by delicate, uniform, seemingly transverse but in reality spiral surface ridges, about 40 from end to end. The ridges contain numerous papillary prominences. Living cysts of *O. henneguyi* average 57 μ in diameter (Rosenberg, 1938), and are spherical except for the elevated area wherein is located the depression which is the point at which the cyst breaks at the time of excystment.

Neuromotor System

The term "neuromotor apparatus" was first used by Sharp (1914) to designate a coordinating system of fibrils in *Diplodinium ecaudatum.* It has since been applied to numerous other Protozoa, chiefly the Ciliophora and Flagellata. For historical résumés and bibliographies on the subject consult Ten Kate (1927), Lund (1933), Bush (1934), Kidder (1934) and others.

The neuromotor apparatus of *Opisthonecta henneguyi* is an integrated fibrillar system for the coordination of ciliary activities and certain other largely contractile changes in the shape of the body and retraction and contraction of the epistomal and aboral regions.

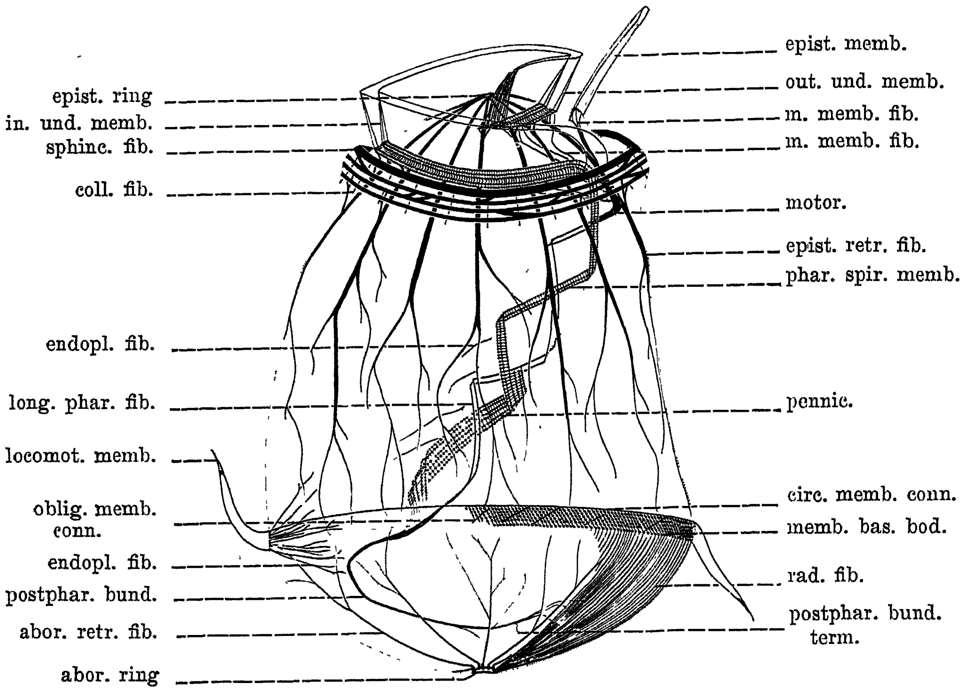

FIG. *B*. Diagram of the neuromotor system of *Opisthonecta henneguyi*. The outlines were drawn from a specimen fixed in osmic acid. The radial fibrils and the basal bodies of the membranelles are omitted on one side of the organism for the sake of clarity. × 1050. Abbreviations: *abor. retr. fib.*, aboral retractor fibril; *abor. ring*, aboral ring; *circ. memb. conn.*, circular membranelle connective; *coll. fib.*, collarette fibril; *endopl. fib.*, endoplasmic fibrils; *epist. memb.*, epistomal membrane; *epist. retr. fib.*, epistome-retractor fibril; *epist. ring.*, epistomal ring; *in. und. memb.*, inner undulating membrane; *locomot. memb.*, locomotor membranelles; *long. phar. fib.*, longitudinal pharyngeal fibril; *memb. bas. bod.*, membranelle basal bodies; *m. memb. fibs.*, moto-membrano fibrils; *motor.*, motorium; *obliq. memb. conn.*, oblique membranelle connective; *out. und. memb.*, outer undulating membrane; *pennic.*, peniculus; *phar. spir. memb.*, pharyngeal spiral membrane; *postphar. bund.*, postpharyngeal bundle; *postphar. bund. term.*, postpharyngeal bundle terminal; *rad. fib.*, radial fibril; *sphinc. fib.*, sphincter fibril.

The neuromotor system is divisible structurally and presumably functionally into three major regions with interconnecting fibrils, corresponding to the three major ciliary regions. The fibrillar regions are adoral, pharyngeal and aboral. Their constituent fibrillar elements are tabulated below:

The adoral region

a. Inner undulating membrane
b. Outer undulating membrane
c. Epistomal membrane

d. Epistomal ring
e. Epistome-retractor fibrils
f. Sphincter fibrils
g. Collarette fibrils.

The pharyngeal region

a. Motorium
b. Moto-membrano fibrils
c. Longitudinal pharyngeal fibrils
d. Postpharyngeal bundle
e. Pharyngeal spiral membrane
f. Penniculus
g. Endoplasmic fibrils.

The aboral region

a. Aboral ring
b. Aboral retractor fibrils
c. Locomotor membranelles
d. Radial fibrils
e. Circular membranelle connective
f. Oblique membranelle connectives.

The Adoral Region

The *inner undulating membrane* is composed of three rows of cilia, the *outer undulating membrane* of but two. These two membranes have a common origin on the epistome, arising as a group of cilia which soon separate into the two component groups. Both membranes have a common course. Fine fibrils connect the two transversely at each ciliary basal body junction. Schröder (1906a) shows three rows of basal bodies for each undulating membrane in *Campanella umbellaria.* He figures fine fibrils connecting the basal bodies as in *Opisthonecta.* The basal bodies of each membrane in *Opisthonecta* lie just beneath the pellicle and are connected by cross and longitudinal fibrils. The distance traversed by the membranes is about 125 μ to the cytostome. Twelve rows of cilia occur in a space of 5 microns, thus the outer membrane consists of about 600 cilia, the inner of 900.

The *epistomal ring* is at the apex of the epistome. From this the epistome-retractor fibrils radiate. Schröder (1906c) shows a solid body in *Vorticella moniliata* at the oral pole, from which the retractor fibrils radiate. In *Campanella* and *Epistylis*

Schröder (1906a, 1906b) figures no central body or ring from which they radiate but shows them ending abruptly and singly.

The *epistome-retractor* fibrils are heavy and radiate from the ring. They give off fine fibrils to the bases of the inner and outer undulating membranes. They also give off fine fibrils to the sphincter fibrils of the collarette. As they proceed aborally they become closely applied to the inner side of the pellicle and here they branch considerably. The fibrils become smaller as they approach the locomotor membranelle ring where some of them insert. Some insert on the pellicle before reaching this point.

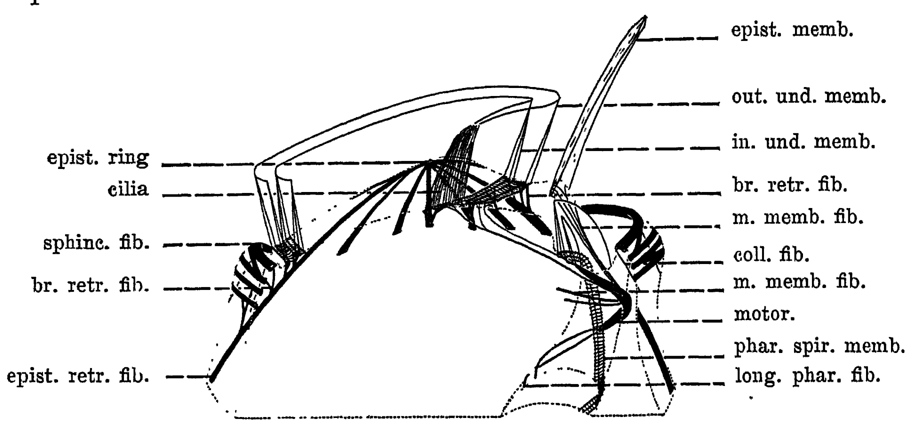

FIG. *C*. Diagrammatic section of anterior third of same organism as Fig. *B*. × 1400. Abbreviations: *br. retr. fib.*, branches of retractor fibrils; *coll. fib.*, collarette fibril; *epist. memb.*, epistomal membrane; *epist. retr. fib.*, epistome-retractor fibril; *epist. ring*, epistomal ring; *in. und. memb.*, inner undulating membrane; *long. phar. fib.*, longitudinal pharyngeal fibril; *m. memb. fibs.*, moto-membrano fibrils; *motor.*, motorium; *out. und. memb.*, outer undulating membrane; *phar. spir. memb.*, pharyngeal spiral membrane; *sphinc. fib.*, sphincter fibrils with connectives.

The *sphincter fibrils* are usually four in number although one often encounters specimens with only three. The one nearest the border of the collarette is nearly twice the size of the others. There are heavy oblique branches which interconnect the sphincters at frequent intervals. These connecting fibrils were not seen in living specimens. When contracted, the sphincter fibrils are much larger in size. They are not round but more nearly ribbon-shaped or oval on cross section. The *collarette* fibrils run longitudinally from the heavy border sphincter fibril to the pellicle just aboral to the last sphincter fibril. At contraction these fibrils run obliquely from the border to the pellicle, showing that they do not interconnect the separate sphincter fibrils.

The Pharyngeal Region

The *motorium* is a crescent-shaped body which stains black with hematoxylin stain. It is found below the pellicle of the cytostome, curving around the side of it nearest the border of the body, and at the termination of the outer undulating membrane. From the motorium fine fibrils run to the base of the epistomal membrane and to the base of the outer and inner undulating membranes in the region of their origin. These we have termed the *moto-membrano* fibrils.

Two *longitudinal pharyngeal* fibrils arise from the motorium at the end opposite the termination of the outer undulating membrane. These two fibrils run a counterclockwise spiral course, making two complete circuits before reaching the ampulla of the pharynx. From the ampulla they continue along the gullet and then into the endoplasm along with other fibrils from the pharynx to form the *postpharyngeal bundle.* As the bundle approaches the aboral end of the body it breaks up into four to eight fine fibrils which terminate in the radial fibrils.

The *pharyngeal spiral membrane* is a continuation of the inner undulating membrane, thus consisting of three rows of cilia. It, too, runs a counterclockwise course making almost two circuits of the pharynx. As it completes its course it breaks up into individual cilia.

At the point at which the pharyngeal spiral membrane breaks up into its component cilia, there arise three more rows of cilia which run parallel to them, thus giving six rows of cilia which beat heavily into the ampulla. This set of six rows of cilia we have termed the *penniculus* as it suggests a structure similar to that found in Paramecium (Gelei, 1932; Lund, 1933). The penniculus makes about a half circuit of the pharynx and terminates in the ampulla. The longitudinal fibrils which connect the ciliary basal bodies of the pharyngeal spiral membrane continue into the penniculus. The longitudinal fibrils of the penniculus continue past the ampulla and gullet and unite with the longitudinal pharyngeal fibrils to form the postpharyngeal bundle, but because of their small and decreasing diameter probably do not accompany it to its termination.

There are numerous endoplasmic fibrils, one group arising from the endoplasmic margins of the inner end of the pharynx and ampulla and terminating free in the endoplasm after a short

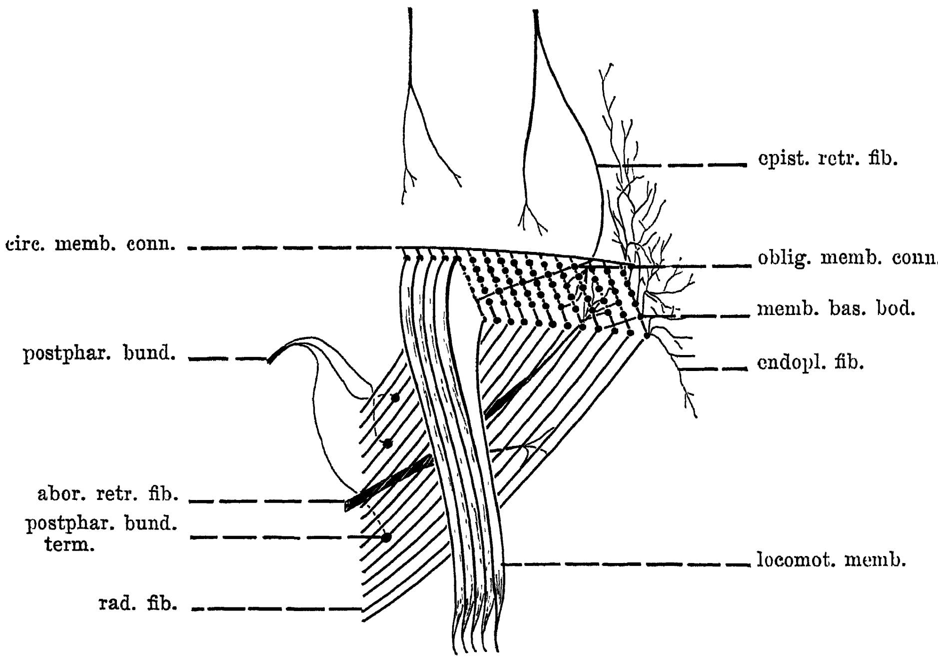

FIG. *D*. Diagrammatic section of a portion of the aboral membranelle region. × approx. 2900. Abbreviations: *abor. retr. fib.*, aboral retractor fibril; *circ. memb. conn.*, circular membranelle connective; *endopl. fib.*, endoplasmic fibrils; *epist. retr. fib.*, epistome-retractor fibril; *locomot. memb.*, locomotor membranelles; *memb. bas. bod.*, membranelle basal body; *obliq. memb. conn.*, oblique membranelle connective; *post-phar. bund.*, postpharyngeal bundle; *postphar. bund. term.*, postpharyngeal bundle terminal; *rad. fib.*, radial fibril.

course. The second group consists of numerous fibrils from the bases of the locomotor membranelles.

The Aboral Region

At the aboral pole there is a ring about 2 μ in diameter. It is comparable to the point of origin of the stalk in the stalked Vorticellidae but no stalk has ever been observed in this species.

Eight to ten *aboral retractor fibrils* arise at this ring and radiate towards the locomotor band. They branch and become smaller. Some insert on the inner border of the pellicle but most of them insert at the bases of the locomotor membranelles. In *Vorticella moniliata* and *V. putrinum*, Schröder (1906c) shows only one set of retractor fibrils which extend from the aboral pole to the epistome. In *V. putrinum* branches are given off orally and aborally from them to the membranelle ring.

The *locomotor membranelles* consist each of six fused cilia. Sixteen or seventeen membranelles occur in a space of 10 μ. The basal bodies of the membranelles lie obliquely on the surface of the organism. The six basal bodies of each membranelle are interconnected by a fibril which continues aborally from the membranelle as the *radial fibrils.* These diminish in diameter as they approach the aboral end of the body. All the radial fibrils approach the aboral ring and appear as radii from it. The diameter of the radial fibrils decreases so materially that actual connections of them with the aboral ring are not seen probably because they are beyond the resolving power of the microscope.

On the oral border of the locomotor band there is a circular fibril which connects all the membranelles. This we term the *circular membranelle connective.* The membranelles are further interconnected by *oblique membranelle connectives,* which are not evenly distributed and in most cases are not connected with the *circular membranelle connective.*

The Silverline System

Opisthonecta is apparently without a pellicular silverline system. Preparations made according to the Klein technique show a system of lines around the body which upon careful analysis reveal themselves as several spirals running side by side. Along these lines there occur at regular intervals, large argentophilic dots one-half micron in diameter. These lines and dots occur over all of the body except on the epistomal disc. At the aboral pole there is considerable branching and anastomosis of the lines. New lines arise here and there in the course of the spirals. These lines agree with the surface annulations of the body, even to the branching and anastomoses as demonstrated by the nigrosin technique. The large dots correspond to the papillary sculpturing on the surface. In Gelei-Horvath preparations the lines show up faintly in a few cases; the dots, however, remain as dark and distinct as in the Klein preparations. In the Gelei-Horvath preparations there are no distinct connections between these dots.

Since ridges and prominences are apt to impregnate with silver nitrate (Lund) it appears that the same is the case here. Lund has said that dry silver preparations tend to show silver

impregnated ridges, while those made according to the wet technique show the ridges less prominently if at all. Thus it appears that the silverline system of *Opisthonecta* is only an indication of the surface patterning. Since no connections are found between the silver impregnated dots on the surface and the internal fibrillar system, these dots are probably only argentophilic sculpturings, and are not concerned with any conductile mechanism of the body.

Discussion

The entire fibrillar system of *Opisthonecta* is related to the motorium directly or indirectly and thus provides a coordinating mechanism. The three undulating membranes are most intimately associated with it. Motor impulses apparently reach the basal bodies of the membranes by way of the moto-membrano fibrils. Since membrane waves are initiated at the oral end of the body, such a mechanism seems plausible. The fine fibrils connecting the basal bodies of each membrane and those connecting the two adoral membranes further aid in making a unified effective mechanism of the membranes. The epistomal membrane beats coordinately with the other two in such a manner that food is effectively shunted into the pharynx, and prevented from passing the mouth.

The epistome-retractor fibrils may have a dual function, that of contraction and conduction. They are associated with the motorium through the basal bodies of the adoral membranes, at the one end of the body and through the basal bodies of the membranelles at the other end. Thus stimuli may travel by either of two paths, the shorter of which is from the motorium to the moto-membrano fibrils, to the basal bodies of the membranes and from there directly to the retractor fibrils. The second path is an indirect one. In this case the stimuli may pass from the motorium to the longitudinal pharyngeal fibrils, to the postpharyngeal bundle to the radial fibrils, and back to the membranelle basal bodies. From the membranelle basal bodies they may be picked up by some of the terminal branches of the retractors. The assertion that the retractors must possess some powers of conduction is based on the fact that they all respond together. The epistomal ring may be a further coordinating device for them.

The sphincter fibrils receive small branches from the epistome retractors and are thus stimulated simultaneously with them. The sphincters are integrated by oblique connectors. The collarette fibrils are intimately associated with the primary sphincter and are thus stimulated simultaneously with it.

The aboral retractors are stimulated by impulses picked up from the basal bodies of the membranelles and/or the aboral ring. The aboral ring, radial fibrils and the membranelle connectives appear to be the chief coordinating mechanisms of the membranelles.

The penniculus receives its stimuli from the motorium through the path of the undulating membranes as it is a direct continuation of the pharyngeal spiral membrane which in turn is a direct continuation of the inner adoral membrane. The penniculus works in conjunction with the membranes to take care of the food. As fast as the membranes send food to the pharynx, the penniculus rounds it compactly into a bolus preparatory to formation of a food vacuole, and sends it on into the endoplasm.

The endoplasmic fibrils may be non-conductile, acting as supporting ramifications in the viscous protoplasm to support the membranelles and the pharynx, or they may have some, as yet obscure, endoplasmic relationship with the neuromotor system.

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EXPLANATION OF PLATE I

All figures are of *Opisthonecta henneguyi* and are drawn with camera lucida to same scale. × 780. The specimens here figured with the exception of Fig. 2 were fixed in Schaudinn's fluid, stained in Heidenhain's iron hematoxylin, and mounted in balsam.

FIG. 1. Whole mount showing cytoplasmic detail. For labels of parts see text figure *A*.

FIG. 2. A cyst vitally stained with Brilliant Cresyl Blue showing the persistence of organelles, the surface annulations, and systolic condition of the two contractile vacuoles. The micronucleus does not show in Brilliant Cresyl Blue preparations.

FIG. 3. Late fission showing the contracted condition of the body, especially the sphincter fibrils. Note that division is at this stage longitudinal. The micronuclei assume an aboral position during fission.

FIG. 4. A 10μ longitudinal section showing undulating membrane, sphincters with their connectives, retractors, surface annulations, a portion of the macronucleus, and details of the locomotor band.

PLATE I

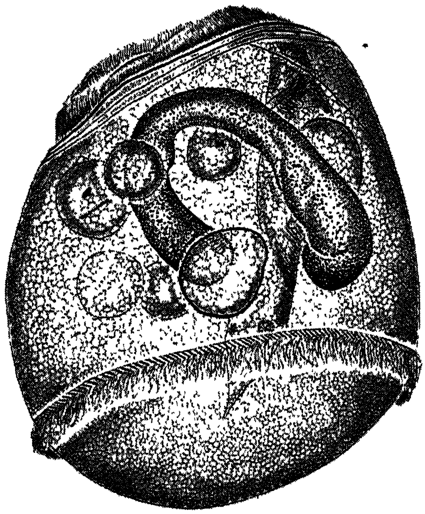

FIG. 1

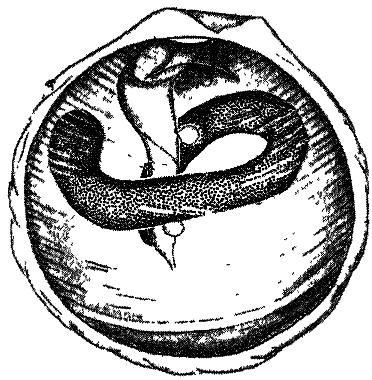

FIG. 2

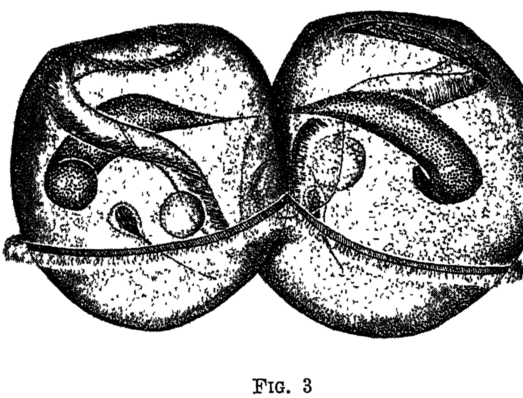

FIG. 3

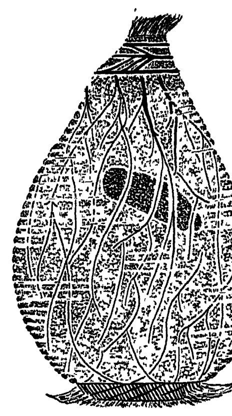

FIG. 4

CONJUGATION IN *OPISTHONECTA HENNEGUYI* A FREE SWIMMING VORTICELLID

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(Communicated by C. A. Kofoid)

ABSTRACT

Opisthonecta henneguyi undergoes a conjugation cycle similar in its general aspects to other peritrichs, but because of its free-swimming, unstalked existence certain variations occur. The preconjugation fission results in a large macroconjugant, indistinguishable from the vegetative individuals, and a typical microconjugant. In stalked peritrichs the microconjugant makes contact with a macroconjugant at one side of the stalk and remains there until maturation is completed at which time only the cytoplasm of the microconjugant flows into its conjugant mate, leaving the microconjugant hull on the outside to drop off. In *Opisthonecta* the entire microconjugant gains access to the interior of the macroconjugant. The point of entry is always at the aboral tip of the latter, where a stalk would occur were it a stalked form. The micronucleus of the microconjugant undergoes three maturation divisions to form eight nuclei of which seven degenerate. Two maturations in the macroconjugant result in four nuclei of which three degenerate. The single nuclei in each conjugant form the pronuclear spindles; the body wall of the microconjugant disappears and the pronuclei unite to form the zygote nucleus. The macronuclei of both conjugants degenerate and the fragments become intermingled. Three postzygotic nuclear divisions result in eight nuclei of which one becomes a micronucleus and seven form macronuclear anlagen. Subsequent fissions of the postconjugant and its daughters parcel the macronuclear anlagen to seven individuals; the micronucleus divides at each fission.

INTRODUCTION

ALTHOUGH there are numerous papers in the protozoan literature pertaining to the phenomena accompanying conjugation in the Infusoria, none has dealt with the process in an unstalked, free-swimming vorticellid, such as *Opisthonecta henneguyi* described herein.

The author has had *Opisthonecta* in culture since May 1935, and during this time has been able to induce conjugations at will (Rosenberg, 1938). Material for this study has all been derived from specimens fixed in Bouin's or Schaudinn's fixatives followed by various hematoxylin techniques.

CONJUGATION

The vegetative individuals of this strain of *O. henneguyi* in normal cultures have been described previously (Rosenberg,

1938). Upon transferring old cysts to fresh culture, vigorous division begins immediately, and if not subcultured at the end of twenty-four hours, a cycle of conjugation begins. Preconjugation fission stages (Pl. I, fig. 1) are the first indication that an epidemic of conjugation is about to occur. In this fission individuals elongate laterally in an asymmetric fashion, one side being smaller than the other, resulting in a pair of individuals of unequal size. Stained specimens at this stage show the macronucleus enlarged and elongated, about a third of it being in the smaller individual. The micronucleus has moved from its original position in the oral end, toward the aboral pole and there undergoes a seemingly normal mitosis. From the fission there results one large individual, the macroconjugant (Fig. *A*, 3*a*), which is identical with vegetative individuals (as far as the writer can ascertain), and a small microconjugant (Fig. *A*, 3*b*), which is a miniature of the vegetative form. It is assumed that the larger individual is physiologically different from the normal vegetative form. This assumption is based on the carefully controlled experiments of Finley (1939) who found that the macroconjugant of *Vorticella microstoma* is physiologically different from the vegetative form since the macroconjugant creates an attraction for the free-swimming microconjugants; the vegetative *V. microstoma,* identical in form to the macroconjugant, does not exert an attracting influence. As yet it has been impossible to prove this fact for *Opisthonecta* since both conjugants are free swimming and distinction between, and separation of macroconjugants and vegetative individuals is more difficult.

The microconjugant possesses the same locomotor organelles (undulating membranes and aboral membranelles) as the vegetative individual. A cytopharynx is present, although apparently not used for feeding since no food vacuoles have been seen. The microconjugants average $30\,\mu \times 40\,\mu$ with little variation.

All the larger individuals (vegetative forms and macroconjugants) appear identical in their activities of feeding and locomotion, since no large individual has been observed without food vacuoles or with a different locomotor pattern.

Vegetative individuals of preconjugation cultures average only $62\,\mu \times 80\,\mu$ as compared to $86\,\mu \times 97\,\mu$ in stabilized cultures probably due to the rapid and repeated divisions of the former with insufficient time to grow to the characteristic size.

The onset of a conjugation cycle always appears as an epidemic phenomenon; preconjugation fission stages rapidly become abundant, and a few hours thereafter, practically all large individuals are found to be macroconjugants, each with a microconjugant within its body.

Opisthonecta in all its various forms swims with the aboral end foremost. The microconjugant is extremely active; it swims at several times the speed of the larger forms, and in a jerky, erratic fashion. For this reason, a culture in the conjugation phase is easily recognized.

When a microconjugant is attracted to a macroconjugant, it approaches the surface of the latter and makes several attempts to affix itself, bouncing on and off the surface in its efforts. The bouncing may occur for some time with the microconjugant making contact at points all over the surface of its conjugant mate until finally an attachment is made at the aboral tips of the two individuals (Fig. *A*, 4). This point of union corresponds to the point of stalk attachment in such vorticellids as *Epistylis, Carchesium, Vorticella,* etc.

Other microconjugants may affix themselves at various places on the surface of a macroconjugant, but a permanent union is only rarely effected, as indicated by the absence of such supernumerary microconjugants on fixed specimens. This phenomenon is analogous to that of sperm entrance into eggs of metazoa wherein the successful entry of one sperm commonly induces the formation of a fertilization membrane which precludes entry of other additional sperms. The rare instances of penetration by more than one microconjugant may likewise be compared to cases of polyspermy as occur in some eggs.

In stalked vorticellids the microconjugant does not enter the macroconjugant, but after making contact remains attached at that point until the maturation processes have been completed, after which cytoplasmic continuity is effected at this point between the two individuals (*Vorticella* and *Epistylis,* Engelmann, 1876; *Vorticella,* Greeff, 1870; *Carchesium,* Popoff, 1908; *Ophrydium,* Kaltenbach, 1916). The same is true of *Trichodina,* an urceolarid peritrich (Padnos, 1939).

In *Opisthonecta,* the microconjugant gradually enters the macroconjugant within which it remains intact during the maturation divisions (Fig. A, 5–9; Pl. I, figs. 3–5), and until the

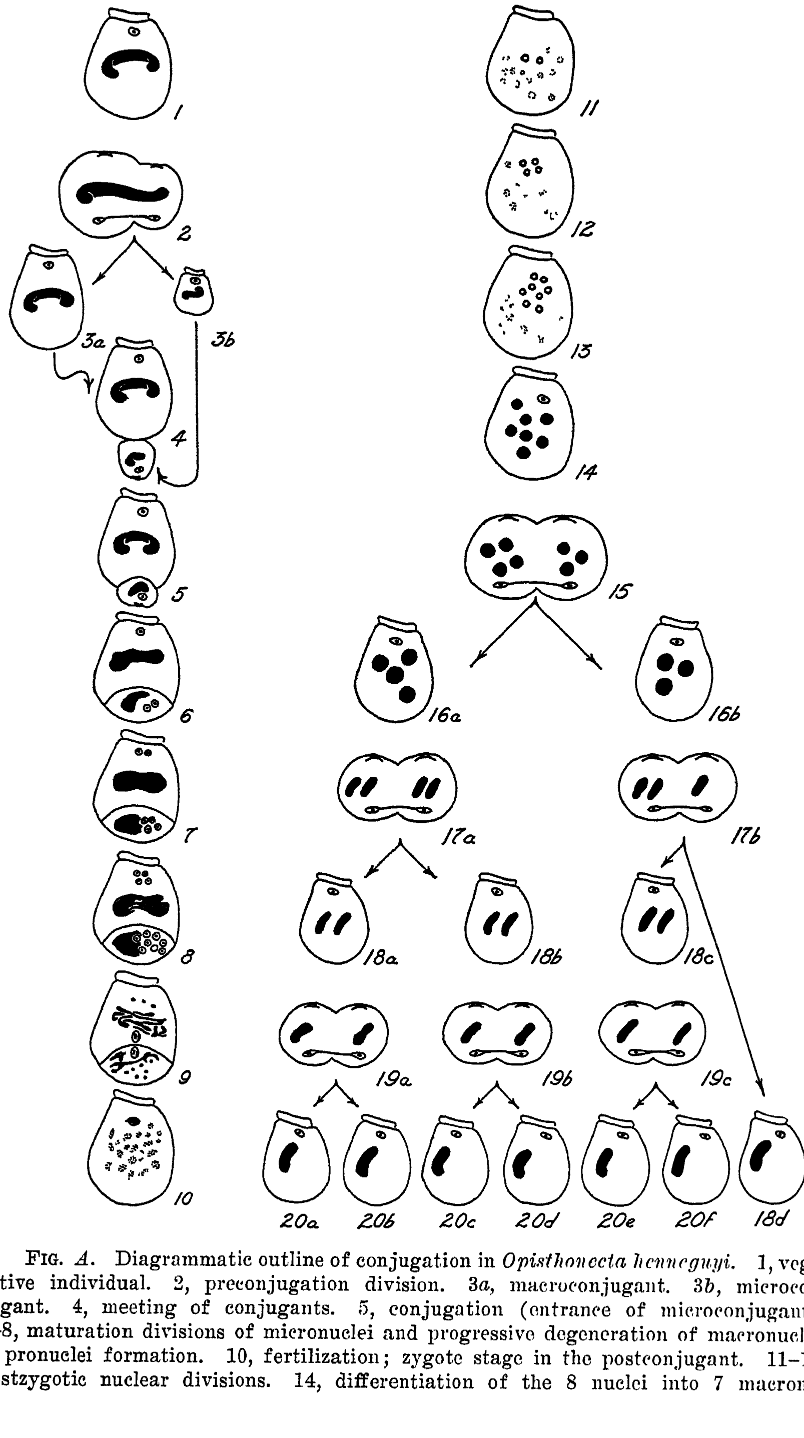

FIG. *A*. Diagrammatic outline of conjugation in *Opisthonecta henneguyi*. 1, vegetative individual. 2, preconjugation division. 3*a*, macroconjugant. 3*b*, microconjugant. 4, meeting of conjugants. 5, conjugation (entrance of microconjugant). 6–8, maturation divisions of micronuclei and progressive degeneration of macronuclei. 9, pronuclei formation. 10, fertilization; zygote stage in the postconjugant. 11–13, postzygotic nuclear divisions. 14, differentiation of the 8 nuclei into 7 macronu-

pronuclei of the two conjugants are ready to meet one another; the body wall of the microconjugant then disappears and cytoplasmic continuity of the two is established (Fig. *A*, 10; Pl. I, fig. 6).

The micronuclei of both conjugants undergo their respective maturation, each in its own oral region. The maturation of the microconjugant precedes shortly that of the macroconjugant; the first maturation division of the latter not occurring until the second one of the microconjugant is under way. The macroconjugant undergoes two maturation divisions, forming four nuclei. The microconjugant undergoes a third maturation division while the second one of the macroconjugant is occurring. The result is that eight nuclei are formed in the micro- and only four in the macro-conjugant. The maturing nuclei in both undergo short resting stages between divisions. The macronucleus of each of the two conjugants begins to show indications of degeneration during the maturation divisions. Upon completion of the micronuclear maturation, the macronucleus in each begins to thread out and subsequently fragments (Pl. I, figs. 4–7). Whether the definitive pronucleus of each of the two conjugants is formed by an additional division of one of its matured nuclei, or whether it is merely transformed from one of them cannot be said definitely for *Opisthonecta*. The pronuclear stage has only been seen in three instances (all similar to Fig. *A*, 9 and Pl. I, fig. 5); in each of them it appeared that one of the nuclei of the macroconjugant was undergoing a third maturation division, and one in the microconjugant was undergoing a fourth. The "spindles" seen, however, may either be division figures or the actual pronuclei. In 200 slides of conjugating forms, each with many hundreds of individuals, no further development of these pronuclear figures has been observed. Popoff (1908) shows that such divisions of one matured nucleus in each conjugant do occur in *Carchesium polypinum*. In *Opisthonecta*, at any rate, three nuclei of the macro- and seven of the

clei and 1 micronucleus. 15, first somatic division; 4 macronuclei parcelled to one individual (16*a*) and 3 to the other (16*b*). The micronucleus divides. 17*a*, *b*, second somatic division resulting in three individuals with 2 macronuclei each (18*a*, *b*, *c*), and one with a single macronucleus (18*d*). 19*a*, *b*, *c*, third somatic division. 20*a*, *b*, *c*, *d*, *e*, *f* and 18*d*, final forms resulting from the reorganization stages, which will grow to become normal vegetative individuals as of stage 1 above.

Note: The degenerating macronuclei of the original conjugants are not shown here after stage 13, but are evident in life in all stages through the final forms (20*a*, etc.).

micro-conjugant are seen to degenerate at the appearance of the pronuclear "spindles" which now move aborally in each conjugant; the next stage observed has been that of cytoplasmic continuity and the union of the two gametic nuclei to form a zygote nucleus. The macronuclear fragments of the two individuals now become intermingled. The zygote nucleus (Fig. *A,* 10; Pl. I, fig. 6) moves orally in the body which has been formed from the united conjugants, a body which for convenience may now be called the postconjugant.

The zygote nucleus then undergoes three successive divisions without the interposition of resting stages (Fig. *A,* 11–13). The result is eight granular nuclei of which one soon differentiates into a typical micronucleus and the other seven develop into identical homogeneous-staining bodies which later become macronuclei (Fig. *A,* 14; Pl. I, figs. 7, 8). The micronucleus stains as a typical one, but the new macronuclei are only slightly chromophilic, and appear as seven gray bodies after moderate staining with iron hematoxylin. The new macronuclei increase considerably in size before the first somatic division. As soon as the micronucleus differentiates, it moves to its normal position in the oral region of the postconjugant from which it again migrates aborally at the onset of each fission, to return orally during each growth phase.

At the first fission of the postconjugant (Fig. *A,* 15; Pl. II, fig. 4) the body elongates laterally and the micronucleus divides, as in a normal mitotic fission. The seven macronuclei, however, do not undergo division; they are parcelled unevenly into the two prospective daughter cells, four into one individual and three into the other (Fig. *A,* 16*a,* 16*b*; Pl. II, fig. 1). Deviations from this distribution occur rarely; instances of six and one and of five and two have been observed. The F_1 daughter cells do not attain the full size of normal vegetative forms before the second somatic fission is indicated. From the daughter cell possessing four macronuclei, two F_2 individuals result, to each one of which are parcelled two macronuclei (Fig. *A,* 18*a,* 18*b*; Pl. II, fig. 2). The F_1 individual with three macronuclei divides into two F_2 individuals, one with two macronuclei and one contains a single macronucleus. The F_2 individuals undergo a short growth period. Each of the three F_2 individuals (Fig. *A,* 18*a,* 18*b,* 18*c*; Pl. II, fig. 2) with two macronuclei undergoes a third

somatic division, with the result that six F_3 individuals are formed, each with a single macronucleus and a single micronucleus. The end result of the reorganization of the postconjugant consists thus of seven individuals, six of which are F_3, and one of the F_2 generation (Fig. *A*, 20*a–f*, 18*d*). The resulting seven individuals are quite small but grow rapidly upon the completion of the reorganization process to become normal vegetative individuals.

During the reorganization period the macronuclei gradually become more chromophilic and change in shape from spherical to an elongate, sausage form (Pl. II, figs. 2, 5, 6).

Fragments of the degenerating macronuclei of the conjugants are carried through the entire reorganization process but disappear soon after the last somatic division.

During the maturation divisions of the micronuclei and during the three post-zygotic nuclear divisions the chromatin appears as tiny chromophilic chromosomes, numbering about 55 to 60. They are extremely small and difficult to count. As low as 28 have been counted on an early first maturation spindle, and up to 58 on a later first maturation spindle. From 50 to 58 have been seen on the second maturation spindle of both micro- and macro-conjugants, and a like number in the eight nuclei resulting from divisions of the zygote. At all other divisions of the micronuclei the chromosomes are long and stain lightly.

The entire process from the preconjugation division to the last somatic division of the reorganization individuals is completed within seventy-two hours.

At the onset of every fission, whether it be vegetative, preconjugational or postconjugational, the epistome with its adoral undulating membranes is contracted (Fig. *A*, 2, 15, 17, 19; Pl. I, fig. 1; Pl. II, figs. 4, 5, 6), and does not relax until the daughter individuals are released from the dividing figure. Feeding is therefore impossible during fission.

Discussion and Summary

Opisthonecta henneguyi presents certain variations from the established picture of conjugation as seen in other peritrichs, yet it agrees in general trend to such an extent that a fairly definite pattern for peritrich conjugation may be assumed.

In the stalked vorticellids the small, free-swimming micro-

conjugant unites with the stalked macroconjugant. *Opisthonecta* is an unstalked form, retaining the aboral ring of locomotor organelles throughout life. When the preconjugation division occurs the two daughter cells are the free-swimming macro- and micro-conjugants, which superficially are identical save for their size and locomotor activities. The microconjugant, as in other peritrichs, is a non-feeding form that swims rapidly in an erratic fashion, while the macroconjugant is indistinguishable from normal vegetative individuals. In all other peritrichs the microconjugant unites with its larger conjugant mate at a place near the aboral pole and to one side of the stalk. There it remains partly fused until the pronuclei of both conjugants unite to form a zygote, at which time cytoplasmic continuity is established between the two conjugants. In *Opisthonecta* the microconjugant seeks out the aboral tip of the macroconjugant, where the stalk would appear in stalked forms. Here it makes its entry into the conjugant host, and is entirely inside by the time the first maturation division occurs. The body wall of the microconjugant disappears just prior to the formation of the zygote nucleus.

It is well to complete the comparison of *Opisthonecta* with other peritrichs at this point. In all peritrichs it appears that maturation is completed in the body of the microconjugant. In *Opisthonecta* that microconjugant, although within the body of the macroconjugant, is still complete unto itself; after maturation it breaks down and its entire body becomes a part of the macroconjugant. In all other forms the completion of maturation sees the establishment of cytoplasmic continuity and the contents of the microconjugant flow into the macroconjugant; the "shell" of the microconjugant remains on the surface and is later dropped off (Greeff, 1870).

In the Peritricha, conjugation is a one way phenomenon in which two individuals of different sizes meet and subsequently fuse to form a single individual, the postconjugant. This is in distinct contrast to the process in most orders of the Infusoria, where reciprocal fertilization with subsequent separation of the conjugants is the rule rather than the exception. No conjugation of the reciprocal type has been reported for the peritrichs.

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EXPLANATION OF PLATES I AND II

All drawings are from specimens of *Opisthonecta henneguyi* and have been drawn with the aid of a camera lucida.

Figures 1 to 7 of Plate I and figures 1 and 7 of Plate II were fixed in Schaudinn's fluid; figures 2–6 of Plate II were fixed in Bouin's fluid. All were stained with iron hematoxylin. The figures are produced here at a magnification of 465 diameters.

PLATE I

FIG. 1. Preconjugation fission.

FIG. 2. A microconjugant.

FIG. 3. The microconjugant making its way into the macroconjugant is preparing for the first maturation of its micronucleus.

FIG. 4. The microconjugant with its body wall still intact is entirely within the body of its conjugant mate. Maturation has been completed in both individuals and the first signs of macronuclear degeneration are indicated.

FIG. 5. Pronuclear ''spindles.'' Seven degenerating matured nuclei are present in the micro- and three in the macro-conjugant. Macronuclear threading is pronounced.

FIG. 6. The postconjugant with its zygote nucleus in the oral region. Cytoplasmic continuity has been established and the macronuclei of both conjugants are intermingled.

FIG. 7. One micronucleus and seven new macronuclei have resulted from postzygotic divisions of the zygote nucleus.

FIG. 8. A later development of the same stage.

PLATE I

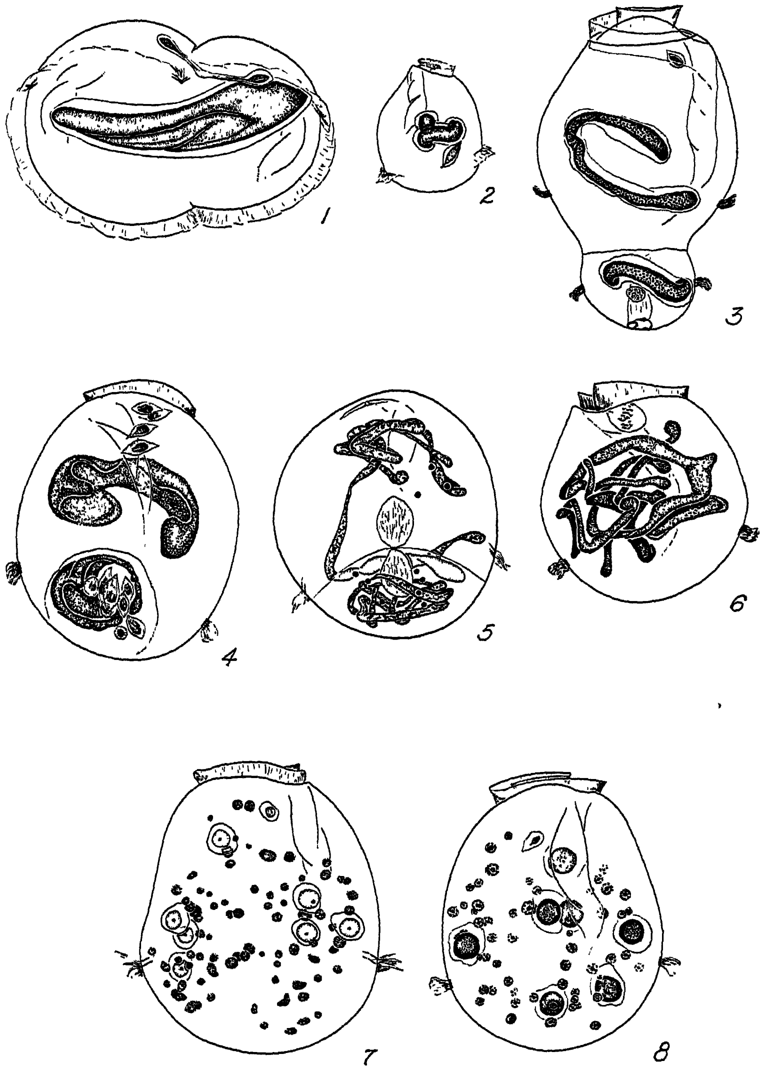


PLATE II

FIG. 1. An F_1 individual with four macronuclei and one micronucleus.

FIG. 2. An early F_2 individual (before growth) with two macronuclei and one micronucleus.

FIG. 3. One of the seven endproducts of reorganization with one macronucleus and one micronucleus. This individual is in the growth phase. Shortly the degenerating particles of the old macronuclei will be completely resorbed and the individual will be a normal vegetative form.

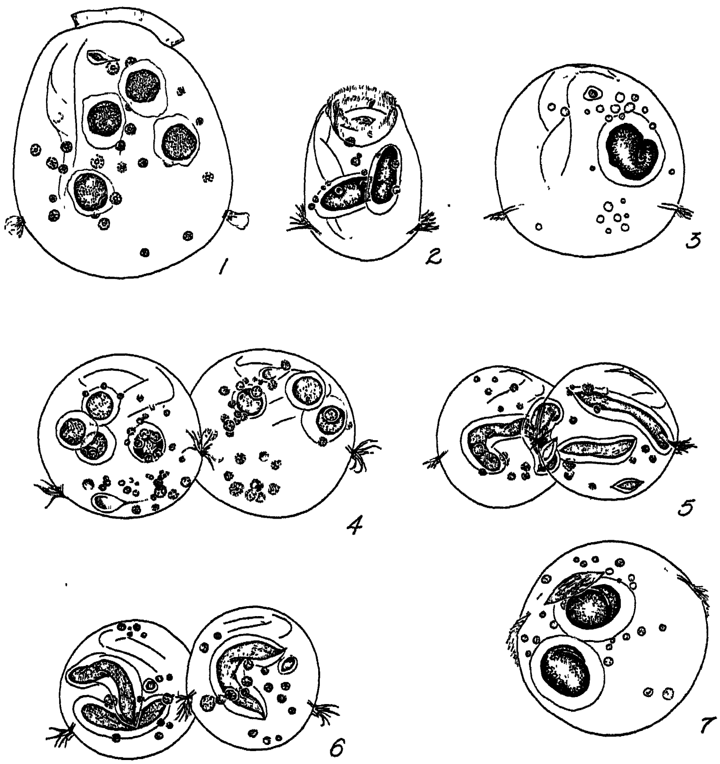
FIG. 4. Fission of a postconjugant, parcelling four macronuclei to one daughter and three to the other.

FIG. 5. Fission of a four macronucleate F_1, parcelling two macronuclei to each daughter.

FIG. 6. Fission of a three macronucleate F_1, parcelling two macronuclei to one daughter and one to the other.

FIG. 7. Early pre-fission stage of a two macronucleate F_2 individual.

PLATE II



THE DISTRIBUTION OF SELF-STERILITY IN THE FLOWERING PLANTS *

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ABSTRACT

The incidence of self-sterility has been investigated in more than eight hundred species of flowering plants. The occurrence of self-sterility in various orders is based on the author's investigations and such information as is available in the scattered literature on the subject. Self-sterility is widespread among the various orders, but is more prevalent in the more specialized groups and especially in the orders which include only or primarily herbaceous plants. The greater incidence of self-sterility among the herbaceous plants is to be expected since these types are derived from the more primitive woody species and self-sterility must be a relatively recent inovation in sexual reproduction. Self-sterility in promoting variation and perpetuating heterozygosity appears to have played an important part in the evolutionary development of the flowering plants.—KARL SAX.

INTRODUCTION

SELF-STERILITY, self-incompatibility, or Parasterilität (*cf.* Brieger, 1930), is a phenomenon found in various orders of the Angiosperms. Comparable, and perhaps homologous, phenomena have been described in some of the lower plants, particularly the fungi, and in certain animals. Brieger has analyzed the genetical bearings of self-sterility, as he found the subject in 1930, with clarity and thoroughness. He confined himself to a discussion of the intensive post-Mendelian work on a relatively small number of species which had cleared away most of the genetical difficulties, and to a general comparison of the situations found there with those in less well known instances in other plants and animals.

Since this time, a number of valuable papers have been published, which add to our knowledge of the distribution of self-sterility in the Angiosperms, and which make intelligible the

* This paper was nearly completed by Dr. East at the time of his death, but he had not written his interpretation of the data, nor had he discussed the work in detail with his colleagues. He had observed the correlation between growth habit and incidence of self-sterility. I have attempted to correlate the incidence of self-sterility with the phylogenetic development of the various orders, and Dr. Edgar Anderson has provided the tabular summary of the data presented in Table 1.—KARL SAX.

MONOCOTYLEDONEAE[1]

1. Pandanales

No self-incompatibility is known. The Typhaceae are protandrous, but *Typha angustifolia* L. and *T. latifolia* L. have been proved to be self-fertile. There is little definite information on the Pandanaceae and the Sparganiaceae, though general indications point to self-fertility throughout the families. *Pandanus utilis* Bory, and *Sparganium americanum* Nutt. and *S. angustifolium* Michx. are known to be self-fertile. There is some protogyny.

2. Helobiae (Fluviales)

No experimental observations have been made on the seven families included in this order. In the Potamogetonaceae, Zostera, Posidonia and Potamogeton are protogynous, while Ruppia is protandrous (E. & P.); yet the structure of the pollen and of the female flowers is such that self-fertility throughout is highly probable. The same may be said for the Najadaceae, where not diœcious, and the Aponogetonaceae, particularly as fertilization is said sometimes to occur under water. The Scheuchzeriaceae are generally protandrous, but are self-fertile, so far as known. In the Alismataceae, the common monœcious Sagittarias, and *Alisma Plantago-aquatica* L. are self-fertile. Relationships in the Butomaceae are not known. The Hydrocharitaceae, where not diœcious, are presumably self-fertile, as judged by structure. The Halophilas are pollinated under water (E. & P.). There is some entomophily.

3. Triuridales

The fertility relationships of the Triuridaceae are unknown.

4. Glumiflorae

There is a considerable amount of self-incompatibility in the Gramineae, but it is confined to the hermaphroditic tribes, and to genera where there is little tendency to dichogamy. It is much

[1] Certain references are abbreviated: D., A. & P. is Darwin, Animals and Plants under Domestication, Ed. 2; D., C. & S. is Darwin, Cross- and Self-fertilisation in the Vegetable Kingdom, Ed. 2; E. & P. is Engler & Prantl, Die Pflanzen-Familien; K. is Knuth's Handbook of Flower Pollination (Davis translation); M. is Müller, Fertilisation of Flowers (Thompson translation). Where no references are given, the observations are those of the writer.

more frequent in perennial than in annual grasses. The most reliable information concerns species having an economic value. See Körnicke (1890), and especially the extended investigations of Beddows and his colleagues at the Welsh Plant Breeding Station (1931, Series H, No. 12). Other references are found here.

Maydeae. Self-sterility is unknown. *Euchlaena mexicana* Schrad., all forms of *Zea mays* L., *Coix lacryma* L., and *Tripsacum dactyloides* L. are self-fertile.

Andropogoneae. Self-sterility is unknown, but positive specific information on self-fertility is available only on the cultivated species of Saccharum and Andropogon. Focke (1893) gives *Saccharum officinarum* L. as self-sterile, but modern plant breeders have found that it is self-fertile, though the pollen is often bad.

Zoysieae. Tristegineae. The fertility relations are unknown, though it is thought that there is frequent autogamy.

Paniceae. No self-sterility is known. The commoner species of the larger genera Paspalum, Panicum, and Pennisetum, are self-fertile. Many species are autogamous, and *Amphicarpum Purshii* Knuth is cleistogamous.

Oryzeae. No self-sterility is known. *Zizania aquatica* L. and the cultivated species and varieties of *Oryza sativa* L. are self-fertile. *Leersia oryzoides* Sw. is cleistogamous (D., C. & S.), as is also *Oryza clandestina* A. Br. (M.).

Phalarideae. The members of this tribe are mostly annual and are self-fertile. *Anthoxanthum odoratum* L. and *Phalaris canariensis* L. are exceptional in being weakly self-sterile (Beddows, 1931). Autogamy is prevalent.

Agrostideae. Known self-steriles are *Apera spica-venti* P. Beauv. and *Alopecurus agrestis* L. (*A. myosuroides* Huds.) among annuals, and *Phleum pratense* L. and possibly *Alopecurus pratensis* L. and *Agrostis tenuis* Sibth. among perennials (Beddows, 1931). The last two species, if self-sterile, are weakly so. Some twenty species or so are known to be self-fertile, but the tribe has not been well sampled.

Aveneae. Known self-steriles are the perennials *Holcus lanatus* L., *Deschampsia flexuosa* Trin., and *Arrhenatherum elatius* Mert. & Koch (Beddows, 1931). *Aira caryophyllea* L. (Beddows, 1931) and all the Avena species studied are self-fertile. *Danthonia spicata* R. & Sch. is well known as a cleistogamous species.

Chlorideae. No self-steriles are known, but little controlled work has been done on this group. *Spartina glabra* Muhl., *Eleusine indica* Gaertn., and *Buchloë dactyloides* (Nutt.) Engelm. (when mon.), appear to be self-fertile; while *Cynodon dactylon* (L.) Pers. appears to be self-sterile from rather too casual observations.

Festuceae. Diplachne serotina Link and *Triodia decumbens* Beauv., *Festuca Myuros* L., *F. sciuroides,* and *F. ciliata* Danth., and probably several other species (see Hackel, 1902, 1906) are known to be cleistogamous (Hackel, 1906; Beddows, 1931). Hildebrand (1872), Körnicke (1890), and Beddows (1931) each found that the perennial species of this group are highly self-sterile, while practically all the annual species tested (between 30 and 40) were strongly self-fertile. A possibly self-sterile exception among the annuals is *Bromus secalinus* L. Fertile exceptions among the perennials are: *Brachypodium sylvaticum* R. & Sch., *Cynosurus cristatus* L., *Bromus giganteus* L., *B. ramosus* Huds., and *B. carinatus* Hook. & Arn. More or less strongly self-sterile perennials are: *Festuca rubra* L., *F. pratensis* Huds., *F. arundinaceae* Schreb., *Bromus inermis* Layss., *Briza media* L., *Dactylis glomerata* L., and *Molinia caerulea* Moench.

Hordeae. Self-sterility in *Secale cereale* L. was discovered by Rimpau in 1877. Beddows (1931) has added *Lolium multiflorum* Lam. var. *westerwoldicum* from among the annual species. Most of the annual species, however, are self-fertile, including the genera Hordeum and Triticum, both of which are well known. The perennial self-sterile species listed by Beddows include *Hordeum bulbosum* L., *Agropyrum repens* Beauv., and *Lolium perenne* L. (some male sterile plants found). The remaining perennials tested were self-fertile, including *Agropyrum tenerum* Vasey, *A. Richardsonii* Schrad., *A. biflorum* (Brignoli) R. E. S., and *A. caninum* L. (Beauv.), quoted by Beddows from Molte, *A. scabrum* Beauv., and *A. multiflorum* T. Kirk.

Bambuseae. Little is known about the bamboos, since they flower rarely after many years, and then usually die. *Bambusa tulda* Roxb. and *B. vulgaris* Schrad. are self-fertile, if one may judge the behavior of single clumps apparently from one clone.

Cyperaceae. Self-incompatibility has not been found in this wind-pollinated family. My own observations on a few species

of the genus Cyperus, including *C. papyrus* L., and on the genus Carex indicate complete self-fertility. It is true that natural self-fertilization is probably not the rule in the monœcious species of the Caricoideae; nevertheless, since the plants are usually protandrous and the pollen is tenacious of life, self-pollination is possible and is probably effective.

5. Principes

Palmae. Self-sterility is unknown in this family. Though mostly monœcious through abortion, with a tendency toward dichogamy, I believe that all palms are self-fertile. My observations on individual isolated fruiting specimens include the following genera; Acanthorhiza H. Wendl., Actinophloeus Becc., Areca L., Arenga Labill., Butia Becc., Caryota L., Elaeis Jacq., Guilielma Mart., Latania Comm., Livingstona R. Br., Phoenix L., Rhaphis L. f., Roystonea O. F. Cook, Sabal Adans, Salacca Reinw., Thrinax L. f. apud Sw., and Washingtonia H. Wendl.

6. Synanthae

Cyclanthaceae. The monœcious species of this small family are so highly protogynous that self-fertilization is usually impossible. Mr. R. M. Grey, Superintendent of the Atkins Institution of the Arnold Arboretum, informs me that self-fertilized flowers of *Carludovica palmata* Ruiz & Pav. ripen a few seeds. One suspects that the family is physiologically self-compatible.

7. Spathiflorae

Araceae. This family is largely monœcious, and the plants are so protogynous that self-pollination is difficult to test. Isolated specimens of *Symplocarpus foetidus* (L.) Nutt., *Cyrtesperma senegalense* (Schott) Engl., and *Anthurium hybridum* Hort. apud Engl. var. *sanguinea* Hort. occasionally set a few selfed seed. Numerous species are thought to be autogamous. Self-sterility has been reported in *Pinellia tuberifera* Tenore (Hunger, K.) and in *Acorus Calamus* L. (Ludwig, 1900), but the thing is questionable. Ludwig's evidence is simply that the European forms set no seed.

Lemnaceae. This little family is thought to be largely autogamous and often cleistogamous (Engler, E. & P.).

8. Farinosae

Flagellariineae. The general indications are for complete self-fertility in the Flagellariaceae, but experimental observations are wanting.

Enantioblastae. Of the five small families in this sub-order, the Restionaceae are practically all diœcious. The remaining four, the Centrolepidaceae, Mayacaceae, Xyridaceae, and Eriocaulaceae, are monœcious or hermaphroditic, and, from the flower structure, are probably self-fertile; but nothing definite is known.

Bromeliineae. The little outlying groups, Thurniaceae and Rapateaceae, are unknown as to their fertility relationships, but again the collateral indications are for self-fertility. Two cases of self-sterility have been reported in the Bromeliaceae. F. Müller (K.) found that *Billbergia speciosa* Riv. was self-sterile, although *Billbergia zebrina* Lindl. was self-fertile. Webber (*J. R. Hort. Soc.*, **24**: 138–139, 1900) obtained no seed when selfing *Ananas sativus* Lindl., though some 500 seedlings were obtained after cross pollinations. The flowers of the species in this family are protandrous and ordinarily very ephemeral. Self-pollination may not be the rule, but Wittmack (E. & P.) says that artificial selfings are fruitful. Cleistogamy is prevalent. F. Müller (K.) states that he found cleistogamy in 20 species of the single genus Nidularium.

Commelinineae. Self-sterility is fairly widespread in the Commelinaceae. The Tradescantias of the virginiana group (species 1–18, Anderson and Woodson, 1935) are all self-sterile; and *Callisia repens* L., *Commelina virginica* L., and *Zebrina pendula* Schnizl., have variable self-sterility. On the other hand, *Commelina coelistis* Willd. is autogamous (Kerner, K.), and *C. benghalensis* L. has subterranean cleistogamous flowers (Weinmann, K.). *Rhoeo discolor* Hance, and several species of Tradescantia and Commelina are known to be self-fertile.

Pontederiineae. The Pontederiaceae is the only family among the monocotyledons known to have heterostyled flowers (F. Müller, 1871. K.). Cleistogamy is present in Monocharia, in four species of Heterantha, and probably in Hydrothrix. In *Eichhornia cranipes* (Mart.) Solms, "longs" and "mids" are known. All three forms are known in the trimorphic species *Eichhornia azurea* (Sw.) Knuth, *Pontederia cordata* L., *P.*

rotundifolia L., *Reussia subovata* Solms, and *R. triloba* Seub. (Schönland, E. & P.). *Heteranthera reniformis* Ruiz & Pavon and *H. limosa* (Sw.) Willd. are dimorphic and heterostyled, while *H. zosteraefolia* Mart. is dimorphic but not heterostyled and is self-fertile (K.). The self-sterility in the heterostyled species is weak.

Nothing is known about the fertility relationships of the Cyanastraceae or of the Philydraceae.

9. Liliflorae

Juncineae. Self-sterility is unknown in the Juncaceae; and the reproductive biology is so well known from the work of Müller, Buchenau, and others, that it should have been discovered if present in many species. F. Müller (K.) ascertained that cleistogamy occurs in five species of Juncus and one species of Luzula. Autogamy in chasmogamous species is thought to be rare on account of the prevalence of protogyny. Three genera are diœcious.

Liliineae. The few species found in the Stemonaceae are probably self-fertile. *Stemona javanica* (Kunth) Engl. is said to be autogamous. Of the three small families, Haemodoraceae, Velloziaceae, and Taccaceae, practically nothing is known about fertility relationships, though *Lachnanthes tinctoria* (Walt.) Ell. and *Lophiola aurea* Ker. appear to be self-fertile. Many of the Dioscoreaceae are diœcious. The monœcious and hermaphroditic species are dichogamous. No observations appear to have been made on the effects of artificial self-pollination. In the remainder of the Liliineae, self-sterility is common.

In spite of the fact that so many species in the Liliaceae are cultivated, the sexual physiology within the species is not very well known. The same species is frequently described as protandrous and as protogynous by different authors. If insects are found busying themselves about the flowers, the plants are asserted to be cross-pollinated to a greater extent than the evidence warrants. And if plants are homogamous, and stigmas are found covered with pollen, autogamy is marked down without waiting to find whether any seeds set.

Recently a few groups have been sampled fairly well and observed carefully, however, and the frequency with which self-sterility has been found is astonishing. One should not be sur-

prised, therefore, to find self-sterility in any hermaphroditic genus where there is no great tendency toward dichogamy.

No incompatibility has been reported in the Melanthioideae and the Herrerioideae. Nevertheless, I suspect that it exists and will be found in such genera as Tofieldia, Schoenocaulon, Melanthium, Uvularia, and Colchicum. My own observations on *Uvularia perfoliata* L. and *Colchicum autumnale* L. indicate self-incompatibility rather strongly, though this is opposed to the usual view.

The Asphodeloideae show a great amount of self-sterility. It is suspected in *Asphodelus fistulosus* L. and in species of Anthericum. *Hemerocallis fulva* L. is self-sterile (Sprengel Entd. Geh.), as well as *H. flava* L., *H. Dumortieri* Morr. and "*H. serotina*" (Focke, 1879). Berger (Das Pflanzenreich, 1908) apparently feels that the whole genus Aloë is self-incompatible, although only "*A. aethiopica* (Schwein.) Berger, *A. pluridens* Haw., *A. cassia* Solm., etc." are mentioned as having been observed experimentally; but the "etc." may cover other data. I have found *Aloë vera* L. to be self-sterile. Marshak (1934) proved that *Gasteria brevifolia* Haw., *G. nigricans* Duval, *G. planifolia* Baker, *G. pulchra* Haw., and *G. verrucosa* Duval are self-sterile. *G. lingua* Link., *G. disticha* Haw., and *G. sulcata* Haw. were weakly self-sterile. The same was true of *Haworthia attenuata* Haw., *H. cymbiformis* Duval (= *H. planifolia* Haw.), and *H. tortuosa* Haw. I have corroborated these observations. It seems, therefore, that the entire group of Aloëae may be expected to show a greater or lesser degree of self-sterility, even among plants that set a fair amount of seed after self-pollination.

Only a few members of the Allioideae have been investigated. Knuth says that *Gagea fascicularis* Salisb., *G. bracteolaris* Salisb., *G. saxatilis* Schult., and *G. Liotardi* Schult., are autogamous or pseudo-cleistogamous; and it is well known that the common onion, *Allium Cepa* L. is autogamous. But Levan (1936), who has worked with *Allium nutans* L. and *A. Schoenoprasum* L., found that these species were decidedly self-sterile. Both vary greatly in chromosome number, the diploids being more self-sterile than the polyploids, and Levan suggests that the self-sterile condition is a factor in the production of polyploidy.

In the Lilioideae, the best known genus is Lilium, owing largely to the intensive work of Stout (1922, 1933). Self-

sterility was first reported here by Wilder (D., C. & S.) in *L. auratum* Lindl. Later, Focke (1878, 1890) tried for fifteen years, without success, to fertilize various classes of *L. bulbiferum* L. Stout's list of self-sterile species includes *L. auratum* Lindl., *L. canadense* L., *L. candidum* L., *L. chalcedonicum* L., *L. croceum* Chaix, *L. dauricum* Ker. Gawl., *L. elegans* Thunb., *L. Hansoni* Leichtl., *L. Henryi* Baker, *L. Humboldtii* Roezl. & Leichtl., *L. japonicum* Thunb., *L. Kelloggii* Purdy, *L. Leichtlinii* Hook. (= *L. Maximowiczii* Regel), *L. longiflorum* Thunb., *L. Martagon* L., *L. pardalinum* Kellogg (= *L. Roezlii* Regel), *L. Parryi* S. Wats., *L. parvum* Kellogg, *L. philadelphicum* L., *L. regale* E. H. Wilson (some plants self-fertile), *L. Sargentiae* E. H. Wilson, *L. speciosum* Thunb., *L. sulphureum* Baker, *L. superbum* L., *L. tenuifolium* Fisch., *L. tigrinum* Ker. Gawl., and *L. warleyense* Willm. (= *L. Willmottiae* E. H. Wilson). It is hardly proper, at present, to say that all the numerous species of this genus are self-incompatible, but the above records form a pretty good sample, and not a single species is wholly and persistently self-compatible. *L. longiflorum* and *L. regale* both produce self-compatible seedlings, yet they are outnumbered by those which are self-incompatible. I have worked only with *L. regale* and found, as did Stout, that the incompatibility reaction occurs in the tissue at the base of the stigma. One should expect to find a slight change in the trend of the growth-curve of the pollen-tubes, therefore, even in the self-fertile clones; but if the reaction is present, it is indetectable in the most fertile plants.

Queerly enough, related genera are largely, though perhaps not wholly, self-fertile. Fritillaria species are practically all pseudo-cleistogamous (K.), and Tulipa, Scilla, and Hyacinthus appear to be self-fertile, since the Dutch horticulturists make no mention, so far as I am aware, of incompatibility. I have found some apparent self-sterility in *Erythronium americanum* Ker., *E. albidum* Nutt., *Ornithogalum umbellatum* L., and *Muscari botryoides* (L.) Mill., but further work should be done to establish the facts.

The Dracaenoidae are supposed to be self-fertile, though cross-pollination is promoted by marked protogyny. Artificial self-pollination is certainly effective in *Cordyline terminalis* (L.) Kunth, *Dracaena fragrans* (L.) Ker-Gawl, and *Sansevieria*

thyrsiflora Thunb. There is a question, however, about the Yuccas. *Yucca filamentosa* L. and *Y. aloifolia* L. have set no seed in a few self-pollinations that I have tried, and *Y. Treculeana* Carr., and some late-blooming varieties of *Y. filamentosa* L. are reported as self-sterile by Kunth. These plants should be investigated further, for it would be interesting, if true, to find that the Yucca moth must, of necessity, afford the opportunity for a cross-pollination if her larvae are to be fed.

The Asparagoideae are self-fertile where known, as in Asparagus, Smilacina, Convallaria, Paris, and Trillium. In *Trillium grandiflorum* (Michx.) Salisb. and *Polygonatum commutatum* (R. & S.) Dietr., I suspect weak self-sterility from observations on a limited number of pollinations.

The remaining sub-families having perfect flowers afford us no information.

The Amaryllidaceae, when not homogamous, tend to be protandrous; and one might expect a certain amount of self-sterility. Unfortunately, but a few careful observations have been made on the family, and these are largely upon cultivated forms. Nevertheless, self-sterility was reported in certain horticultural varieties (hybrids) of Hippeastrum by Herbert (1837, D., A. & P.), and in certain species and cultivated hybrids of Amaryllis and Cystanthus by Darwin (A. & P.), and by Ludwig (1900). The only additional record that I have found is on *Hymenocallis lacera* Salisb. (K., Eggers, 1881).

My own investigations show self-sterility in *Hymenocallis occidentalis* (Le Conte) Kunth., *Agave americana* L., *A. ferox* C. Koch, *Curculigo recurvata* Dryand., and *Hypoxis hirsuta* (L.) Coville. It is indicated in *Narcissus Pseudo-Narcissus* L. and *N. poeticus* L. and *Zephyranthis Atamasco* (L.) Herb. It is my opinion that many other self-sterile species will be found when trouble is taken to test them.

There are no experimental observations available on the Taccaceae and the Dioscoreaceae, but I have been told, by growers, that there are monœcious species in Dioscorea that are self-fertile when good pollen is obtainable at the time that the female flowers are ready.

Iridineae. In the Iridaceae, Darwin (A. & P.) reported that Rawson found self-sterility in certain hybrid forms of Gladiolus; and one of my friends, who is a distinguished Gladiolus breeder,

had such difficulty in obtaining selfed seed from various pure species that there is no doubt as to the presence of self-sterility in the genus. *Romulea Bulbocodium* Seb. Maur. is self-sterile (K., Battandier), as is also *Marica Northiana* Herb. (K., F. Müller).

On the other hand, the cultivated species of Crocus, and certain species of Marica, Trimezia, and Cypella (K., F. Müller) are self-fertile, and isolated plants of the commoner species of Sisyrinchium of eastern North America set seed. The large amount of work done by Cover, Swabey, and Stout (1926) on the cultivated varieties of Iris indicates complete self-fertility in the bulbous Dutch irises and the beardless irises when the pollen is good, though the crested irises are less self-fruitful. Since the bearded irises only occasionally set seed to their own pollen, it is possible that they are weakly self-sterile, but the matter requires further investigation.

10. Scitamineae

The Musaceae are self-fertile, so far as I can learn. Most of them are monœcious, and the fruits of the cultivated varieties are seedless. For reproductive cytology, see White (1928).

In the Zingiberaceae, self-sterility has been reported in *Zingiber officinale* Rosc. (Focke, 1893), *Hedychium coccineum* Buch.-Harn. (F. Müller, Ludwig), and *Alpinia nutans* Rosc. and an unnamed species of the same genus (F. Müller). Müller reported self-fertility in other species of Hedychium and Alpinia. I suspect self-sterility in *Kaempferia rotunda* L., but the evidence is hardly adequate for decision.

The cultivated varieties of Canna are found to be self-fertile by horticulturists, yet it would not be surprising if some self-sterility were discovered, considering the close relationship of the Cannaceae to the Zingiberaceae.

The few species of the Marantaceae, studied by Hildebrand and by F. Müller, proved to be self-fertile.

11. Microspermae

Nothing is known of the reproductive physiology of the Burmanniaceae, which are sometimes one-sexed through abortion.

In the huge family Orchidaceae, with its 15,000 or 20,000 species, almost anything is rather more than likely to happen. So

one finds cleistogamous species, autogamous species, self-sterile species, and species with almost every imaginable mechanism for cross-fertilization by insects. Unfortunately, since the time of Darwin and Hermann Müller, no particular interest has been taken in the situations under discussion here. Taxonomy, interspecific crossing, and the technique of seedling production rightly have been considered of more importance; nevertheless, I was somewhat shocked that practically nothing was to be found on self-sterility, beyond what was reported by Darwin, in the large collection of books on orchids owned by my colleague, Professor Oakes Ames.

Cleistogamy is listed by Darwin or by Müller in species of Cattleya, Dendrobium, Epidendron, and Schomburgkia; autogamy in Epidendrum, Epipactis, Gymnadenia, Listera (occasionally), Neotinea, Neottia, Ophrys, and Platanthera; self-fertility, but no autogamy, in Cypripedium, Epipactis, Orchis, etc. Veitch's Manual of Orchidaceous Plants (1887–94) has the statement that one or more species of Acanthephippium, Cypripedium, Chysis, Dendrobium, and Laelia are either cleistogamous or autogamous. They (J. Veitch & Sons) believe that self-fertilization has been insufficiently emphasized in the orchids, and that it is more common in terrestrial than in epiphytic species.

The self-sterile species listed by Darwin are: *Epidendrum cinnabarium* Salzm. (Müller), *Epipactis atrorubens* Schult. (= *E. viridiflora* Reichb.), *Maxillaria atrorubens* [?] (Scott), *Oncidium Cavendishianum* Batem. (Rivière), *O. cimiciferum* Munro & Sims (= *O. flexuosum* Lindl.) (Müller), *O. crispum* Lodd. (Müller), *O. divaricatum* Lindl. (Munro), *O. microchilum* Batem. ex Lindl. (Scott), *O. sphacelatum* Lindl. (Scott), and *O. unicorne* Lindl. In addition, Darwin says that self-sterility is common among the Vandeae, and that F. Müller found self-sterility in unamed species of Burlingtonia, Gomeza, Notylia (two species), Oncidium (four other species), Rodrigueza, and Stigmatostalix (own pollen poisonous to stigmas). Knuth (Davis) names as self-sterile *Cypripedium calceolus* L. (Ludwig, 1900) and *Oncidium Lemonianum* Lindl. (Eggers).

One gathers from these data that at least 5 per cent of the orchids are self-sterile, or perhaps 750 species, in all, if the samples are at random.

Since no modern work has been done on incompatibility in the Orchidaceae, we can only speculate as to the mechanism involved. We know, however, that from six months to a year sometimes elapses between pollination and maturity of the seed. Under these circumstances, it is unlikely that differential pollen-tube growth is the cause. Presumably the situation is like that which Sears found for Gasteria.

DICOTYLEDONEAE

ARCHICHLAMYDEAE

1. Verticillatae

The monogeneric, monœcious family Casuarinaceae, comprising the single genus Casuarina, is self-fertile, according to several investigators who have found isolated fruiting individuals. I have checked on *C. equisetifolia* Forst. and *C. glauca* Sieb.

2. Piperales

Nothing is known about the reproductive physiology of the three small families belonging to this order, and not very much about the Piperaceae. *Peperomia maculosa* (L.) Hook. and *Piper auritum* H. B. K. are self-fertile. I have been told by good observers that other species are self-fertile. So far as I can learn, few, if any, accurate observations have been made on hermaphroditic forms.

There is apogamy in the genus Houttuynia of the Saururaceae (Ernst, 1918).

3. Salicales

The exceptionally monœcious plants of the Salicaceae appear to be self-fertile.

4. Garryales

Of the Garryaceae, nothing is known.

5. Myricales

I suspect that the monœcious species of Myrica (usually diœcious) are all self-fertile, but I know this to be the case only in *M. asplenifolia* L.

6. Balanopsidales. 7. Leitneriales

There is no hermaphroditism in these two small groups.

8. Juglandales

All species of the monœcious Juglandaceae are presumably self-fertile, though tending to be protandrous. I have investigated all the common species of Juglans and Carya.

9. Batidales. 10. Julianiales

There is no hermaphroditism in these small isolated groups.

11. Fagales

The members of the monœcious families Betulaceae and Fagaceae are frequently so protandrous that isolated trees bear almost no fruit; yet several graduate students of Harvard University, who have worked with these families, have found no difficulty in getting selfed seed by artificial pollination. In one or the other of these two ways the common representatives in eastern North America—Carpinus, Ostrya, Corylus, Betula, Alnus, Fagus, Castanea, and Quercus—have been found to be self-fertile.

12. Urticales

There is no reason for suspecting self-incompatibility in the generally monœcious family Ulmaceae. There is marked protogyny, which makes testing difficult, but it has been possible to show self-fertility in several species of Ulmus and Celtis.

The same remarks apply to the Moraceae, a larger and more varied family, where there is a definitely greater tendency toward the diœcious condition. In reality, less is known about reproductive physiology than one would expect, owing to the involved sexual arrangements and the existing proclivity for the maturation of the male and the female gametes at different times. I have satisfied myself that *Artocarpus communis* Forst, *A. integer* (Thun.) Merr., *Castilloa elastica* Cerv., *Morus alba* L., and *M. nigra* L. are self-fertile. I have also examined rather carefully, in Cuba, five imported species of fig—*Ficus Benjamina* L., *F. crassinervis* Desf., *F. elastica* Roxb., *F. glomerata* Roxb.,

and *F. religiosa* L. The evidence is in favor of the idea that all these species produce viable seeds after self-fertilization. Owing to the immense literature on caprification of the commercial fig, *Ficus Carica* L., by means of the wasp *Blastophaga psenes* L., and to the fact that other wasps of the same genus (and other genera) were found by F. Müller to be acting as pollinating agents in other species of Ficus, the feeling has grown that all figs require cross-pollination for seed production. This idea I believe to be erroneous. It is correct in one sense only, even for *F. Carica* L. The old inflorescences, from which the mature wasps emerge, bear only male flowers, and the young inflorescences which they enter for egg-laying bear only female flowers. Thus there is a functional dichogamy so great that it simulates diœciousness. But this appears not to be true of other species; and even in the wild forerunner of the commercial fig, evidence gathered by workers in the U. S. Dept. of Agriculture indicates that dichogamy is imperfect. The supposed "self-sterility" in the figs, therefore, is merely a matter of lack of synchronism in the development of the male and the female flowers within a single inflorescence, combined with lack of the freedom for geitonogamy (pollination by a neighboring flower) found in most plants.

The frequency of explosive mechanisms, whereby clouds of pollen are carried into the air, indicates self-fertility in the Urticaceae (where not diœcious). There is no experimental evidence available, except that certain species of Elatostema are apogamous (Ernst, 1918).

13. Proteales

The Proteaceae are usually hermaphroditic, rarely monœcious, polygamous, or diœcious. The perfect-flowered species tend to be protandrous; and from this fact Delpino (K.) concluded that cross-pollination is the rule. This appears to me to be doubtful. Several species that I have examined seem to be self-pollinated as a general rule, but I have experimented only with *Grevillea robusta* A. Cunn. and *Macadamia ternifolia* F. v. Muell.; they were self-fertile. If any self-sterility exists, it must be rare, or it would have been discovered before now in a family having 1200 species or so.

14. Santalales

The group of families found here exhibit every imaginable sort of sexual specialization—hermaphroditism, monœcism, diœcism, and apogamy (Balanophoraceae, Ernst, 1918). There are parasites (Myzodendraceae), herbs and trees. Unfortunately, however, very little experimental work on them has appeared. So far as I can learn, where there is homogamy, as in Thesium, Comandra, and certain genera of the Loranthaceae, there is also self-fertility. No self-sterility has been reported in any of the families; and, if one may judge from the ease with which morphological specializations have persisted, none will be found.

15. Aristolochiales

The interesting floral mechanism in the Aristolochiaceae was discovered by Sprangel. He overlooked the decided protogyny of the flowers which makes them commonly cross-pollinated. This fact was noted by Hildebrand. The most extended work on their pollination, however, is that of Ule (1898, 1899), who was unable to ascertain, with certainty, that the plants are self-fertile, as maintained by Burck (Ann. Jard. Bot., Buitenzorg, 1890), in spite of the fact that *A. ornithocephala* Hook. was sterile in cultivated gardens. The probability is that the protogyny is the only deterrent to self-fertilization.

The parasitic plants of the little family Hydnoraceae are apparently self-fertile. So also, so far as is known, are the Rafflesiaceae. The Rafflesias are pollinated by carrion flies, according to Delpino (K.); but since the huge flowers of these root parasites are always found singly, the pollination must be self-pollination, and must occur earlier or before the odor develops.

16. Polygonales

The Polygonaceae have numerous variations in their reproductive mechanisms. Usually hermaphroditic, they may be protandrous, homogamous, or protogynous. Within the species, the condition varies from polygamy to monœcism to diœcism, as in *Rumex Acetosella* L. Several species are dimorphic and heterostyled. The best known is *Fagopyrum esculentum* Gaertn., on which there are numerous observations because of its agricultural importance (Althausen, 1908; Dahlgren, 1922).

Like forms are rather weakly self-sterile. Other species are cleistogamous, as *Polygonum perfoliatum* L. and *Koenigia islandica* L. (K.). I have found self-fertility in *Coccoloba uvifera* L. and *Triplaris americana* L. Several species of Rumex are apogamous (Ernst, 1918), including *R. Acetosa* L., *R. arifolius* L. f., and *R. nivalis* Hagetschw.

17. Centrospermae

The Chenopodiaceae are small-flowered, usually hermaphroditic, occasionally diclinous. There is protogyny in most Chenopodiums, but protandry in the Salsoleae. All species investigated, a relatively small number, have proved to be self-fertile.

No self-sterility is known in the Amarantaceae. F. Müller (K.) observed dimorphism on a species of Chamissoa and on several species of Celosia, but they are probably not heterostyled in Darwin's sense.

In the Nyctaginaceae, autogamy and cleistogamy have been observed (K.). I have found self-incompatibility in *Bougainvillea spectabilis* Willd., where there was only 2 per cent of bad pollen. The styles were 12 mm. long, and the pollen-tubes grew only 3 mm. in 48 hours. Other varieties investigated had 50 per cent bad pollen.

The Cynŏcrambaceae, being monœcious, are probably self-fertile; but little is known of this family or of the Aizoaceae and Basellaceae. The last two families, having some homogamous species, may have developed self-incŏmpatibility.

In the Phytolaccaceae, *Phytolacca decandra* L., *Rivina humilis* L., and *R. octandrum* (L.) H. Walter, are self-fertile.

In the Portulacaceae, the Montias and several of the Portulacas are said to be cleistogamous (E. & P., K.). The Claytonias have a tendency to protandry, but may be self-sterile. Willis (J. Linn. Soc., Bot., London, 1895) selfed *Claytonia alsinoides* Sims. and *C. siberica* Pall., with doubtful effect. I have done the same on *Claytonia virginica* L.

The only record of self-sterility in the Caryophyllaceae is Darwin's notation (C. & S.) for *Dianthus caryophyllus* L. This record may be an error. In any event, self-sterility is a rarity. Possibilities for cross-fertilization are provided in protandry and less commonly in protogyny, which goes so far that the plants become andromonœcious, androdiœcious, gynomonœcious,

or gynodiœcious. Autogamy, and even cleistogamy, is known in many genera, and in the immense amount of work done on the group by horticulturists, there is never any difficulty in obtaining selfed seeds, so far as can be learned after a rather extensive inquiry.

18. Ranales

Darwin (C. & S.) stated that in the Nymphaeaceae several Nymphaea species and *Victoria regia* Lindl. were self-sterile. This may very well be the case, though the situation seems not to have been reexamined. Caspary (E. & P.) says that the blossoms of Victoria and Nuphar cannot fertilize themselves, but this notation does not appear to mean that self-sterility is exhibited. On the other hand, he also states definitely that those species of Nymphaea where the inner stamens open before (Section Castalia) or at the same time (Section Symphytopleura) that the stigma is receptive, are self-fertile. In Euryale, the closed flowers are fertilized under water. Nymphaeas of the Section Hydrocallis are also autogamous or cleistogamous. I am satisfied that our own commoner species, *Nymphaea advena* Ait., *Castalia odorata* (Ait.) Woodville & Wood., and *Nelumbo lutea* (Willd.) Pers., are self-fertile; nevertheless I cannot be certain that some of the Nymphaeas that I had the opportunity of examining at the Atkins Institution of the Arnold Arboretum in Cuba, are not self-sterile. The evidence on the point is conflicting.

The monœcious Ceratophyllums are presumably self-fertile, though the reproduction does not appear to have been studied in detail.

The Cercidiphyllaceae are diclinous, and reproduction in the Trochodendraceae has not been studied experimentally.

Of the four families placed in the sub-order Ranunculineae, the Ranunculaceae are the best known. They are often protandrous, usually homogamous, and more rarely protogynous (Helleborus, certain sp. Ranunculus, Prantl, E. & P.). Self-sterility has been noted in *Delphinium Consolida* L. (D., A. & P., Müller, K.), *D. elatum* L. (Müller), *Helleborus foetidus* L. (Ludwig, 1900), *Nigella damascena* L. (Hoffman, K.; self-fertile, Müller), *Ranunculus acris* L. (Focke, 1893), *R. bulbosus* L. (Focke, 1893), and *R. verna* L. (Ludwig, 1900). Numerous spe-

cies, however, many from these same genera, have been found to be self-fertile. There is apogamy in Thalictrum (Ernst, 1918).

The Berberidaceae are probably all self-fertile. This is true of *Hydrastis canadensis* L., *Podophyllum peltatum* L., and a number of Berberis and Mahonia species tested. Halstead (1889, K.) reported *Berberis vulgaris* L. as self-sterile, because covered plants set no fruit. This was an erroneous conclusion.

It has been reported to me that *Akebia quinata* (Thun.) Decaisne, of the usually monœcious little family Lardizabalaceae, is self-fertile when not too protogynous. No other information is available.

Although the Menispermaceae are practically all diclinous, they may be noticed here because in the 56 genera into which Prantl divided the family, 2 are known by female flowers only, and 10 by male flowers only. The fruit is unknown in 14 genera.

Within the 10 families that Engler and Gilg place in the suborder Magnoliineae, there is no record of self-sterility. The Magnoliaceae are self-fertile wherever self-pollination is possible; the Calycanthaceae are protogynous and cannot be tested easily—though *Calycanthus fertilis* Walt. is said to be self-fertile; the monotypic family Lactoridaceae is polygamous-monœcious; and the reproductive physiology in the Eupomatiaceae, Gomortegaceae, Hernandiaceae, Monimiaceae, and Myristicaceae is practically unknown. The situation in the Anonaceae is clear only in the cultivated species of Anona and in *Asimina triloba* Dun., all of which are self-fertile. Self-fertility is also characteristic of the Lauraceae, to all appearances, when self-pollination is possible. Perhaps this is an overly broad statement, however, for I have tested only certain species of Cinnamomum, Nectandra, and Persea. On the other hand, information that I believe to be reliable has come to hand on several other genera.

19. Rhoeadales

Self-incompatibility is well distributed throughout the Rhoeadales, and is especially prevalent in the Rhoeadineae.

The Papaveraceae has produced self-incompatible species in all three subfamilies, Hypecooideae, Papaveroideae, and Fumarioideae. The list is as follows: *Hypecoum grandiflorum* L.

and *H. procumbens* L. (Hildebrand, D., A. & P.), *Eschscholtzia californica* Cham. (F. Müller, Hildebrand, 1869, D., C. & S.), *Papaver alpinum* L. (Hoffman, D., C. & S.), *P. radicatum* Rottb. (Focke, 1879), *P. Rhoeas* L. (Hoffman, 1878, Bot. Ztg. 36), *P. somniferum* L. (Hoffman, self-fertile with D.), *Corydalis cava* Schweigg. & Kort. (Hildebrand, D., C. & S.), *C. lutea* D. C. (D., C. & S.), and *C. digitata* Pers. (= *C. solida* Sm. Hildebrand). I have found self-incompatibility in certain plants of *Dicentra Cucullaria* (L.) Bernh.

Roughly, one-fourth of the species tested have been found to be self-sterile and three-fourths self-fertile, in the genera where self-sterility occurs. Several Papavers, in fact, are cleistogamous. Genera tested scatteringly, where no self-sterility has been found, thus far, are Adlumia, Argemone, Chelidonium, Fumaria, and Glaucium; but then, so many genera contain only one or two species. What is really needed is a better sampling of Corydalis and Fumaria.

The Capparidaceae has not been tested properly. There is some tendency to develop monœcism (Forchhamineria) and even diœcism (Apophyllum), yet there is a sufficient number of homogamous species to suppose that self-sterility may be found. Some of the larger genera, namely Capparis, Cleome, and Polanisia, were sampled by Delpino (K.), who found them to be cleistogamous.

The Cruciferae, although all hermaphroditic, have developed a sufficiently extreme protogyny (and also protandry, which is not so effective) in so many species, that natural self-fertilization is frequently impossible. Nevertheless, homogamy is widespread, and in some of these species self-sterility has been developed. Some fifteen or so species are known to be self-sterile out of less than one hundred and fifty species tested, so that presumably many more examples remain to be discovered. The interesting thing about self-sterility, here, is the great variability in the strength of the reaction. In the numerous varieties of *Brassica oleracea* L., *B. Rapa* L. (Lund and Kjaerskou, K.), and *Raphanus sativus* L., for example, self-sterile and apparently self-fertile (perhaps pseudo-sterile) plants are found in the same strains. Additional records of self-sterility include *Aethionaema grandiflorum* Boiss. & Hohen. (Hildebrand, 1896), *Capsella grandiflora* Boiss. (Shull, 1923, 1927, 1929), *Cardamine*

pratensis L. (Hildebrand, D.), *C. bulbifera* R. Br. ex Ait. (Ludwig), *Cochlearia Armoracia* L. (D., A. & P.; possibly an error because of bad pollen), *Crambe tatarica* Jacq. (Kerner, K.), *Hesperis tristis* L. (Hildebrand, 1896), *Hugueninia tanacetifolia* Reichb. (Hildebrand, 1896), *Iberis pinnata* L. (Hildebrand, 1896), *Lobularia maritima* Desv. (= *Alyssum maritimum* Lam.) (Hildebrand, 1896), and *Raphanus Raphanistrum* L. (Hoffman, K.).

Other species from each of these genera have been found to be self-fertile. Also self-fertile are species of Arabis, Bunias, Diplotaris, Droba, Erophila, Erysimum, Lepidium, Myagrum, Nasturtium, Radicula, and Sisymbrium (K. or the author). *Subularia aquatica* L. is cleistogamous and is pollinated under water, according to Knuth.

In the little family Resedaceae, Darwin and later workers found that *Reseda odorata* L. and *R. lutea* L. were self-sterile, though self-fertile strains were also found. *R. Luteola* L. and *R. glauca* L. are self-fertile. It is interesting that the first two species belong in the section Resedastrum DC., while the last two species belong in the sections Luteola DC. and Glaucoreseda DC. There are no records of tests of other species either in this or the five remaining genera, though it is known that there is a tendency to develop monœcism through abortion.

Little is known about the outlying genera Tovaria and Moringa. *Tovaria pendula* Ruiz & Pav. is homogamous and self-fertile; *Moringa oleifera* Lam. is self-fertile, though slightly protandrous.

20. Sarraceniales

In the Sarraceniaceae, *Sarracenia purpurea* L. (Hildebrand, 1883), *S. variolaris* Michx., *S. flava* L., and probably the others, are self-fertile. Heliamphora and Darlingtonia are also thought to be self-fertile, but this is not known with certainty.

The Nepenthaceae have developed the monœcious condition in all species, and are probably physiologically self-fertile.

The Droseraceae apparently are all self-fertile. *Dionaea muscipula* Ell., I am informed by an observing gardener, is self-fertile; *Aldrovanda vesiculosa* L. is cleistogamous (Drude, E. & P.), though most of the ovules remain unfertilized because of paucity of pollen (Knuth), while all the common Droseras are autogamous and frequently pseudo-cleistogamous; nevertheless,

the floral structure is such that it would not be surprising if a case or two of self-incompatibility were to be found.

21. Rosales

Podostemonineae. The Podostemonaceae have not been investigated experimentally. They are water plants, usually hermaphroditic, but occasionally monœcious, and in one genus diœcious (Hydrostachys). They are said to blossom most frequently at the end of the rainy season when the water begins to recede, and to tend to be cleistogamous (Warming, E. & P.). In at least some instances, however, the possibility of cross-pollination is provided by protogyny.

Saxifragineae. There is agreement among botanists that the Crassulaceae are ordinarily slightly protandrous and usually autogamous, but marked protandry and, occasionally, protogyny may make natural self-pollination difficult. Many species of Sedum, Sempervivum, Cotyledon, Bryophyllum, and Crassula have been tested, and no self-incompatibility has been reported (Knuth and others). General considerations lead one to expect it, however.

In the Saxifragaceae, where conditions are known, there is much homogamy, though there are certain highly protandrous and protogynous species. It is odd, therefore, that self-sterility has been reported in only one species, *Tolmiea Menziesii* (Hook.) Torr. Gray, where Correns (1928) found the universal cross-fertility that was mentioned earlier. This is probably because the reproductive physiology of the Saxifragoideae, including the huge genus Saxifraga, is not at all well known. The species investigated by Müller and Delpino usually tended to be cross-fertilized through highly developed protogyny or protandry. On the other hand, the remaining subfamilies—in particular the Hydrangeoideae and the Ribesioideae—appear to be self-fertile, wherever the sexual arrangements are such as to make this possible, and the pollen is viable. Here should be mentioned many tested species of Deutzia, Hydrangea, Philadelphus, and Ribes.

About the group of small families belonging to the Saxifragineae, no experimental information is available. One may note, nevertheless, that monœcism, and diœcism, have developed. Fertile seed can be obtained from well-isolated individuals of *Hamamelis virginica* L. and *Fothergilla Gardeni* Murr.

Rosineae. The monœcious Platanaceae, in spite of noticeable protogyny, are self-fertile, if one may judge from observations on isolated individuals of *Platanus orientalis* and *P. occidentalis* L. In the Rosaceae and the Leguminosae, however, self-sterility is fairly widely distributed.

Self-sterility has not been reported in the Spiraeoideae, though it may exist, for only a dozen or so species of Spirea and *Sorbaria sorbifolia* (L.) A. Br. have been investigated. Concerning the Pomoideae, one might have been able to make the same statement on a truly comparative basis, since numerous wild species in Cotoneaster, Cydonia, Pyrus, Eriobotrya, Amalanchier, and Mespilus have proved to be strictly self-fertile (about forty species being found in my own records), were it not that self-sterility has been found in such a host of the cultivated apples, pears, and quinces (see especially Waite, 1894; Crane, 1927; Crane & Lawrence, 1929). An observation of self-sterility in any genus of the Pomoideae need not be suspect, therefore, except in Crataegus, where almost all the forms are apogamous, if certain diploids are excluded (Sax, unpublished).

In the Rosoideae, Focke (1893) has reported self-sterility in *Kerria japonica* DC., *Neviusia alabamensis* A. Gray, *Rubus spectabilis* Pursh., and *R. odoratus* L., *Potentilla bifurca* L., *Rosa beggeriana* Schrenk., *R. rugosa* Thunb., and *R. setigera* Michx. No one among geneticists seems to have been interested in this point about the Rosoideae since Focke's time, though doubtless a search of horticultural literature would yield new cases. On the other hand, published instances of self-fertility (a number in Knuth) and my own observations include species in the genera Fragaria, Potentilla, Duchesnea, Geum, Ulmaria, Alchemilla and Rosa. In Rosa, for example, the Arnold Arboretum has a collection where nearly thirty species are represented each by a single example. Each is highly fruitful and can be assumed to be self-fertile, since artificial crossing has shown that the species do not hybridize easily enough to make it possible for a plant to be loaded with hybrid seed. Moreover, when samples of the seed are grown, hybrids appear with considerable rarity. One has the feeling, therefore, that the three species of Rosa, termed almost self-sterile by Focke, can possess the reaction in only a rather mild form.

In the Prunoideae, it is said (see Dorsey, 1919, for literature) that all cultivated varieties of the native American plums tested

are self-sterile, with the exception of "New Ulm" and "Robinson." Only about one-half of the cultivated varieties of *Prunus domestica* L. are self-sterile, however. Generalization is precluded, though, because forms having no economic value have not been tested. The extensive work at the John Innes Horticultural Institution (Sutton, 1918; Crane, 1923, 1927; Crane and Lawrence, 1929) is corroboratory on these points, in so far as they are touched. In addition, it was found that all the sweet cherry varieties, *P. avium* L., and about half of the sour cherry varieties, *P. Cerasus* L., were self-sterile; and many were tested. It should be noted that the sweet cherries have somatic chromosome numbers of 16 or 16 plus, while the sour cherries have somatic chromosome numbers of 32 or 32 plus. The varieties of *Prunus domestica* possess approximately 48 chromosomes as the somatic number.

The almonds and peaches, so far as the published records go, appear to be self-fertile. *P. prostrata* Labill. and *P. lusitanica* L. are listed as self-sterile by Focke (1893).

Concerning the Chrysobalanoideae, I have only one record—self-fertility in *Chrysobalanus Icaeo* L.

Practically nothing is known about the fertility relationships of the Connaraceae, but the Leguminosae have been sampled about as well as any family. There are over 400 genera and more than 12,000 species in this group, so naturally there are gaps in our knowledge of the fertility relationships; nevertheless, in an examination of the literature that was by no means exhaustive, I have become satisfied that there is self-fertility in from one to thirty species each in over one hundred and fifty genera. The small number of cases of self-sterility known, therefore, indicates that the phenomenon is relatively rare, and is probably confined to a few groups. This statement does not mean, of course, that there is any lack of arrangements whereby a certain proportion of flowers are cross-pollinated. Delpino (K.) described four such mechanisms: (*a*) a valvular type, which forces the stamens and pistil to project from the corolla when the weight of an insect acts; (*b*) an explosive spring arrangement that can be tripped but once; (*c*) a pump arrangement; and (*d*) a brush arrangement.

Knuth (Davis) lists as self-sterile according to Darwin, the following species: *Trifolium pratense* L., *T. repens* L., *T. incar-*

natum L., *Phaseolus multiflorus* Willd., *Lathyrus grandiflorus* S. & S., *Vicia Faba* L., species of Erythrina (communication of Sir W. MacArthur), *Sarothamnus scoparius* Koch (*Spartium scoparium* L.), *Melilotus officinalis* (L.) Lam., *Lotus corniculatus* L., and *Cytisus Laburnum* L. As a matter of fact, Darwin made the more accurate statement that these plants were more or less sterile without insect aid or artificial self-pollination.

The most satisfactory evidence of true self-sterility in the Leguminosae is that of Williams and of Silow (see Beddows et al., 1931). Williams tested only species of the Papilionatae-Trifolieae, except for one vetch, *Anthyllis Vulneraria* L. (self-fertile). Thirteen species of Trifolium and four species of Medicago were highly self-fertile. Two species of Trifolium were self-fertile only when selfed artificially. *Trifolium alexandrium* L., *T. alpestre, T. badium* Schreb., *T. hybridum* L., and *T. medium* L. were self-sterile, though not enough plants were tested to be able to say that self-sterility runs throughout the species. Perhaps all are like *T. pratense* L., where Silow found both self-fertile and self-sterile plants. Silow's work on pollen-tube growth, however, showed definitely that the self-sterility exhibited is the antigen-antibody reaction that changes markedly the trend of the curve at a definite point, and Williams found intra-sterile, interfertile groups. In another paper (l.c.), Williams found variable self-sterility in *Trifolium repens* L. It is a relatively weak reaction. It is to be noted that the self-sterile species of Trifolium, with the exception of *T. alexandrinum,* are perennial.

Thus it is seen that several species in the Trifolieae are self-sterile. It would not be an extraordinary thing, therefore, if self-sterility should be found in other genera than Trifolium, particularly Medicago and Melilotus, as Piper, Jørgensen, and others have maintained (see Beddows et al., 1931, for this literature). The evidence, at the moment, however, is not wholly satisfactory.

In the Papilionatae-Loteae, Silow (l.c.) has found self-sterility in *Lotus corniculatus* L. and *L. major* Smith (weakly self-sterile), thus corroborating the statements of Darwin, Müller, and others. Two other species were self-fertile.

In the other tribes, there is satisfactory evidence of self-sterility only for *Phaseolus multiflorus* Willd. (D., personally

corroborated), *Lathyrus grandiflorus* S. & S. (D.), and possibly some stocks of *Vicia Faba* L. (D.) and *Apios tuberosa* Mnch. (Chute, 1894). This does not mean that there are not other species that are truly self-sterile. It does not mean, even, that there is not perfectly good experimental evidence that has been missed. It merely means that many records are questionable. It is certainly not wrong, therefore, to conclude that self-sterility is rare and is confined to a few groups in the Leguminosae.

I have investigated a considerable number of species from among genera usually grown in temperate regions. Since no cases of self-sterility were found, it is not necessary to cumber this record with the details. Perhaps it is not out of place, however, to list genera that I have observed at the Atkins Institution of the Arnold Arboretum, in Cuba. They are mostly trees which could be recorded rather easily because only a single flowering specimen was available, although in a few instances other specimens were to be found a third of a mile or farther away. Judgment was made, in most cases, simply by the set of fruit, and the plentiful harvest that was the rule indicates that autogamy and geitonogamy are more frequent than the older observations reported by H. Müller and by Knuth led one to expect. A few species set no fruit naturally, but did after artificial self-pollination.

The record of genera in which self-fertile species were found is as follows. The order corresponds with Engler and Prantl, the numbers being those of the species investigated when more than one.

Mimosoideae. Inga, Enterolobium, Pithecellolobium, Albizzia—2, Acacia—6, Mimosa—2, Desmanthus, Adenanthera.

Caesalpinioideae. Trachylobium, Tamarindus, Lysidice, Cercis—2, Bauhinia—4, Dialium, Cassia—14, Gleditschia—2, Parkinsonia, Haematoxylum, Caesalpinia—5, Peltophorum—2, Poeppiga.

Papilionatae. Ateleia, Camoensia, Sophora, Cladrastis, Crotalaria—2, Laburnum—2, Indigofera, Psoralea, Gliricidia, Robinia—2, Caragana—4, Desmodium, Lespedeza, Lonchocarpus—2, Abrus, Clitoria, Erythrina—7, Canavalia—3, Rhynchosia, Dolichos.

There was an indication of self-sterility in three species not in the above list. The first was *Cassia siamea* Lam. As four-

teen other species of Cassia were highly self-fertile, and the flower mechanism is not particularly variable, this case is suspicious—the more so because 49 per cent of the pollen was empty.

The other two cases *Erythrina speciosa* Andr. and *E. senegalensis* DC. are definitely self-sterile. *E. speciosa* has styles somewhat longer than the stamens, and does not get pollinated properly under Cuban conditions, where it flowers during the early part of February. The tree investigated had less than 2 per cent of bad pollen. When selfed artificially, practically no pollen germinated, though it would germinate in sugar solutions. It seems probable, therefore, that much of the self-sterility reaction takes place on the stigma.

The tree of *E. senegalensis* under observation had stamens and stigmas of about the same length (20 mm.), and the stigmas were always covered with pollen. The pollen was fine, a count of 200 grains in aceto-carmine showing not a single bad one. Only about 5 per cent of the pollen grains germinated, and these produced short tubes—one-half the breadth of the pollen grains—which did not enter the pistil. Other trees, grouped together at a distant part of the garden, fruited well.

Though seven species of Erythrina were listed as self-fertile in the above list, the point is somewhat questionable. *E. Christagalli* L., *E. cubensis* Wright, and *E. monosperma* Gaudich., set only small amounts of seed. Probably it would be more accurate to say that they are weakly self-sterile; but as they were not investigated cytologically, I have let the list stand as originally noted.

Reasoning from the facts in hand, therefore, self-sterility is not to be found in the Mimosoideae and Caesalpinoideae, and is relatively rare in the Papilionatae. It is more frequently found in herbaceous species, but is not confined to them. There is no obvious generalization to be drawn, based upon floral development, as to why the distribution of self-sterility is what it is.

22. Pandales

Monotypic. Unisexual.

23. Geraniales

Geraniineae. According to Reiche (E. & P.), the sexual relationships of the Geraniaceae have been investigated only in

the Geranieae. From the floral structure, the general lack of anatomical specializations for promoting cross-pollination, and the great amount of homogamy with but slight protandry, one should expect to find self-sterility. As a matter of fact, most of the species of Geranium, Erodium, and Pelargonium observed by Darwin, Müller, Knuth, and others have proved to be self-fertile. Some species are cleistogamous, or pseudo-cleistogamous. A few instances have been noted where cross-fertilization is favored by extreme protandry. Darwin found that *Pelargonium zonale* Hort. was weakly self-sterile, and Ludwig (1900) reported similar conditions in *Erodium macrodenum* L. Herit. and *E. cicutarium* L. Herit. var. *pimpinellifolium.* I have found that all the cultivated forms of Pelargonium examined—perhaps a dozen—are weakly self-sterile.

Heterostyly in the Oxalidaceae was discovered in 1863 by Roland Trimen one of Darwin's numerous correspondents. From the intensive investigations of Hildebrand (1866, 1887, 1899), as well as those of Darwin himself, it was found that there are 20 trimorphic species, 51 dimorphic species, and 30 monomorphic species in the genus Oxalis. Dimorphism (Reiche, E. & P.) appears to be characteristic of all the 20 or more species of Biophytum. There is great variability in the self-sterility reactions of the heterostyled species, and in the different forms within the species. The monomorphic forms, so far as they have been investigated, appear to be self-fertile (*O. Acetosella* L., *A. stricta* L., and *A. corniculata* L.); the first-named species produces cleistogamous flowers.

Little is known of the Tropaeolaceae. The stocks of the Nasturtium, *Tropaeolum majus* L., are protandrous and generally self-fertile. I found one strain that was nearly homogamous and seemed to be self-sterile, though the pollen was good; but my observations were not extensive enough to enable me to be certain about the point.

Of the Linaceae, heterostyly is known only in Linum. There the species native to the Americas and to South Africa are monomorphic, but about half of the species (29 enumerated by Alefeld D.) native to Europe, Asia, and North Africa, are dimorphic. The self-sterility reaction is most effective in the long-styled forms. All the monomorphic types investigated are self-fertile, with one exception; Thompson (1850) says that *L. monogynum* Forst. is self-sterile.

The Erythroxylaceae, consisting of the genus Erythroxylon, with nearly two hundred species, plus the single species *Aneulophus africana* Benth. (monomorphic), are, in the main, dimorphic and heterostyled. It may be noted, in view of the situation in Linum, that nearly all the Erythroxylons are indigenous to South America.

Very little information is available concerning the sexual physiology of the remaining families of the Geraniineae. Self-fertility is possible, in spite of protogyny in certain Zygophyllums and in *Guajacum officinale* L. The specializations in the Rutaceae, Simarubaceae, and Burseraceae tend toward monœcism through abortion. Polyembryony is frequent in Citrus. The Meliaceae are often polygamous. Many genera are so extremely protandrous or protogynous (more rarely) that natural self-pollination is impossible.

Besides Citrus, I have records of self-fertility in the Rutaceae of *Aegle Marmelos* (L.) Corr., *Amyris elemifera* L., *Casimiroa edulis* La Llave & Lex., *Clausena Lansium* (Lour.) Skeels, *Severina monophylla* (L.) Tanaka, *Triphasia trifolia* (Burm. f.) P. Wils., and *Xanthoxylum Fagara* (L.) Sarg. *Alvaradoa amorphoides* Liebm., *Picramnia pentandro* Sev., and *Simaruba glauca* DC. are self-fertile in the Simarubiaceae. Again, *Cedrela mexicana* M. Roem., *Melia Azedarach* L., *Swietenia Mahagoni* (L.) Jacq., and *Trichilia hirta* L. are self-fertile in the Meliaceae. Many other records of apparent self-fertility for species of these families are found in Müller, Knuth, and elsewhere; but they concern only the ascertainment of the possibility of self-pollination, so I have used only the data from my own experiments. All that can be said of these outlying groups is that self-sterility has not been proved.

The results of self-pollination on the three families of the Malpighiineae, where there are so many lianas, are even less well known. I have records of self-fertility in the Malpighiaceae as follows: *Malpighia glabra* L., *Stigmaphyllon ledifolium* (H.B.K.) Small, *S. Sagraeanum* A. Juss., *Thryallis brasiliensis* L., and *Triopteris jamaicensis* L.

Of the Polygalineae nothing is known for the Tremandraceae, though the floral structure is such that self-sterility would not be surprising. Self-incompatibility would be even less surprising in the Polygalaceae, because of the prevalence of homogamy,

though thus far none has been found. Many species are known to be self-fertile, however, and cleistogamy is not uncommon. *Salomonia cylindrica* Kurz is apogamous.

The Dichapetalaceae are frequently unisexual through abortion. No pollination studies have been made on them, but self-sterility is unlikely on morphological grounds.

The family Euphorbiaceae, with over 200 genera and upwards of 5000 species, has unisexual flowers. Monœcism is the rule, but diœcism is fairly common. It is to be noted that the exclusively diœcious genera, of which there are about 70, are less successful than the monœcious genera. They are frequently monotypic, and seldom contain as many as 20 species. The great genera, such as Phyllanthus, Croton, Acalypha, and Euphorbia, are not exclusively monœcious, but diœcious species are relatively rare.

Pollination studies have been reported on only about 250 species distributed in some 50 genera. I have investigated the situation in selected species belonging to 12 genera. Clearly, the family has been inadequately sampled. Besides, self-fertility is not easily determined; for, although there is ordinarily an extended flowering period on a given plant, the time during which the stigma is receptive is very short. Nevertheless, self-sterility is indicated in only one species, *Euphorbia Cyparissias* L., since the suggestion of Trelease (1879) that *Euphorbia pulcherrima* Willd. is self-incompatible is certainly erroneous.

It has long been known that *E. Cyparissias* sets seed sparingly in Europe. Muenscher (1936) made the same observation on clones found in eastern New York, and proceeded to investigate the case. Twenty-two colonies, presumably clones in each case, produced no seed. Nineteen colonies were not only self-sterile but were cross-sterile with each other. Three colonies, however, though self-sterile, were cross-fertile with the other nineteen; and these nineteen colonies produced seed when cross-pollinated with pollen from each of the three in question. This experiment is the first to establish the fact that physiological incompatibility may exist in a monœcious species.

It may be of interest to say that in some experiments of mine made at Soledad, Cuba, on *Euphorbia splendens* Bojer, it was found that selfed pollen tubes (pollen 30 per cent inviable) grew through the 2–3 mm. of tissue necessary to effect fertilization as

rapidly as crossed tubes. Fertilization occurred, and the embryos became sufficiently large to see them with a hand lens. But the seeds stopped growing when about one-fourth the normal size, and none matured. It is possible that this is a new type of self-sterility, and should be investigated further.

The little monœcious family Callitrichaceae with its single, but widely distributed, genus Callitriche, ordinarily cannot be self-pollinated on account of a tendency to mature the gametes at different times. Some remarks of Hegelmaier (1864) have been interpreted as a suggestion that *Callitriche hamulata* Kützing is self-sterile, but this is a misconception.

24. Sapindales

In the exceedingly varied group of twenty-one families making up the order Sapindales, not a single case of self-sterility has been reported. Practically nothing is known concerning the reproductive physiology of several of these groups, viz., Cyrillaceae, Pentaphylaceae, Corynocarpaceae, Hippocrateaceae, Salvadoraceae, Stackhousiaceae, and Icacinaceae. Of the remaining families, only the Buxaceae, Anacardiaceae, Aceraceae, Hippocastanaceae, and Sapindaceae are reasonably well known from the experimental point of view. Even in these families, it is fair to say that the reactions are known in not more than 10 per cent of the monœcious and hermaphroditic species, and these are all self-fertile.

The reason why self-sterility is absent (or extremely rare) here is apparently because morphological sexual variations occur extremely frequently, and persist. A few genera are diœcious; numerous genera are monœcious; and dichogamy of all gradations is common in both monœcious and hermaphroditic types. There are extreme morphological sexual variations even within species. This is true, especially, in Ilex, where hermaphroditic plants are no rarity and are self-fertile; in the various groups of the Anacardiaceae, where related biotypes vary from complete hermaphroditism to nearly complete monœcism; and in Acer, where as many as five kinds of inflorescences may occur on the same tree.

There appears to be less extreme intrageneric variability in the Balsaminaceae and the Sapindaceae; and if self-sterility exists in the Sapindales, one should expect to find it in these

families or in the Anacardiaceae. In connection with this last statement, it may be noted that Mr. R. M. Grey has found that an isolated plant of *Schinopsis Lorentzii* (Griseb.) Engl. at Soledad, Cuba, has never fruited, although there is no apparent dichogamy and the pollen seems to be viable.

25. Rhamnales

A similar situation exists in the Rhamnales. No case of true self-sterility has been found in either the Rhamnaceae or the Vitaceae, although a very considerable amount of experimental work has been done on the reproductive physiology of such genera as Zizyphus, Vitis, and Parthenocissus. In the Rhamnaceae, the species are sometimes diœcious, and in both families polygamy is common. Even in the more strictly hermaphroditic forms, the tendency to dichogamy—usually protandry—is so great that natural self-pollination of individual flowers is impossible. Nevertheless, a considerable number of species are known to be self-fertile.

26. Malvales

Very little experimental work has been done on this group, except in the Malvaceae. Relationships in Chlaenaceae, Gonystilaceae, and Scytopetalaceae, are practically unknown. The Elaeocarpaceae and Tiliaceae are sometimes one-sexed through abortion, and a considerable amount of protandry is known. No self-sterility has been found. Genera where one or more species are known to be self-fertile are Elaeocarpus, Slonea, Muntingia, Aristotelia, Apeiba, Grewia, Luehea, Tilia, and Triumfetta. Certain Mollia species have ten rings of stamens, the five outer having green pollen and the five inner having yellow pollen (Darwin). The plants are not heterostyled, though, and are probably self-fertile.

The best known genera of the Malvaceae—Abutilon, Lavatera, Althaea, Malva, Malvastrum, Sida, Hibiscus, and Gossypium, are largely self-fertile. Dicliny is rare, but a slight amount of protandry is prevalent.

A few species are self-sterile in genera of the Malvaceae, where other species are self-fertile. These include *Abutilon darwinii* Hook. f., *A. striatum* Dicks, *A. venosum* Hook., and cer-

tain hybrid forms (F. Müller, 1873), and *Althaea ficifolia* Cav. (Comes, Knuth). Beasley (unpublished) has found the same situation in *Hibiscus syriacus* L. at Boston.

I have studied *Hibiscus Rosa-Sinensis* L. and *H. schizopetalus* Hook., and various hybrids between them. They appear to be self-sterile; but since no inhibition of pollen-tube growth could be demonstrated, it is possible that this appearance is due merely to the influence of external conditions, for the pistils have no exudation, and the flowers absciss easily.

The Bombacaceae are hermaphroditic trees (excepting dialycarpa) with little tendency to dichogamy, and many observations indicate that they are self-fertile. This is certainly true of most species and individuals of Adansonia, Bombacopsis, Bombax, Ceiba, Durio, Ochroma, and Pachira, where well isolated trees set fruit; and it is probably true of many other species for which it has been impossible to obtain satisfactory records.

It is possible, however, that there is some self-sterility in this group. Certain individuals of *Bombax Ceiba* L. and of *Pachira insignis* Savigny do not set fruit, although the flowers are profusely pollinated. The only observations that seem conclusive are on the second species. An isolated tree growing between Cienfuegos and Soledad, in Cuba, has been under the observation of Mr. R. M. Grey for many years. It has set less than a dozen fruits during this period. A cutting from this tree in flower (1933) at the Soledad garden set no fruit. Reciprocal crosses between the two, and hand-selfed flowers also set no fruit. The pollen was 97 per cent good. The longest pollen-tubes in the selfed and clone-crossed flowers, after 48 hours, were 50 mm., and the style (140 mm.) had begun to dry up at this time. Flowers crossed with pollen from unrelated trees, on the other hand, had pollen-tubes 110 mm. long after 98 hours, and the styles showed no signs of desiccation. Presumably, the pollen-tubes would have reached the ovary, and fertilization would have taken place if there had been an opportunity to make later observations. It seems rather likely, then, that this tree is physiologically self-sterile.

The species in the Sterculiaceae are usually hermaphroditic, though monœcious in the Sterculieae. From the morphology of the flowers, and the presence of so many trees and shrubs, one

would expect them to be self-fertile, although protandry is widespread, and proper determination is difficult, as not many observations are to be found in the literature. I have observations that certain species of Brachychiton, Dombeya, Guazuma, Helicteris, Hermannia, Sterculia, and Theobroma are self-fertile.

Knuth quotes Lanza as saying that "plants of *Chiranthodendron* (= Cheirostemon) *platanoides* H. & Bpl. never set fruit at the Palermo Botanic Garden apparently because the appropriate pollinating agent does not occur there." Naturally, this does not prove that the species is self-sterile, but it may well be so. Among the six species of Sterculia at the Soledad garden that I examined in 1933, four were self-fertile naturally, which leads one to assume that normal pollinating agents were present. Yet single isolated plants of *S. lanceolata* Cav. and of *S. Tragacantha* Lindl. set no seed, though the pollen was good (95 per cent and 99 per cent, respectively), and the small stigmas were so covered that 500 grains were obtained from a single one when rubbed on an albumen-covered slide. It was also determined that the pollen-grains germinated freely on the stigmas. Since no cross-pollinations were made, however, all one can say is that there is high probability that self-sterility occurs in the family.

27. Parietales

The heterogeneous lot of plants making up the Parietales, which have been divided into nineteen families of varied size and importance, might be expected to show a percentage of self-sterility comparable with other generally hermaphroditic and homogamous groups. As a matter of fact, self-sterility appears to be quite rare. Whether or not this appearance is real is another matter. Little is known about the physiology of reproduction in most of the families, even families as large as the Guttiferae, with nearly fifty genera. This is partly because so many of the families consist almost wholly of trees and shrubs, on which experiments are not easy to make. It is partly because the order is, generally speaking, an order of tropical plants, and few experiments have been made on tropical plants, except where they have come into greenhouse cultivation. After making adequate allowance for these points, however, one is still inclined to feel that self-sterility is truly rare in the group.

There are nine families in the Theineae. In each of the more important ones, there is a small amount of polygamy, or unisexuality through abortion, but usually there is hermaphroditism with homogamy. And the only case of self-sterility known is the single one reported by Darwin (A. & P.) on *Hypericum calycinum* L., with perhaps the heterostyled species *H. heterostylum* Parl. as a second.

Now, how many cases of self-fertility are known in this suborder? In the Dilleniaceae, there are several species of Hibbertia, Dillenia, and Actinidia; in the Eucryphiaceae, there is one species of Eucryphia; in the Ochnaceae, there are several species of Ochna and Ouratea; in the Caryocaraceae, there are two species of Caryocar and one species of Anthodiscus; in the Marcgraviaceae, there are several species of Marcgravia (cleistogamy, conditions in other genera unknown); in the Theaceae, there are several species of Thea (inc. Camellia), Gordonia, and Stewartia; in the Guttiferae, autogamy is widespread, and there are species in over half of the forty odd genera known to be self-fertile, including numerous members of Hypericum and Garcinia; in the Dipterocarpaceae, owing to the fact that the plants are arboreal, and that the flowers are usually pendent with the pistils longer than the stamens, there is indirect but substantial evidence that most of the species are self-fertile. It should be noted, however, that Darwin found that *Cratoxylon formosum* (Korth.) Benth. & Hook. is dimorphic and heterostyled, and it is quite likely that there are other species in a similar condition.

In the two genera of the Elatinaceae, self-fertility is the rule in Bergia, and self-fertility, or even cleistogamy, is found in Elatine (E. & P., Niedenzu).

Some species of Frankenia are self-fertile. Little is known of the relationships in the other genera of the Frankeniaceae.

No direct observations appear to have been made on the Tamaricaceae (inc. Foquiera), but they are said to be somewhat protandrous. *Tamarix dioica* Roxb. is diœcious through abortion.

Autogamy or cleistogamy is widespread in the Cistaceae, particularly in the genus Helianthemum.

In the Bixaceae, *Bixa Orellana* L. and some of the species of Maximilianea are known to be self-fertile. The three species of Amoreuxia have dimorphic stamens, but they are not hetero-

styled, and no self-sterility has been proved. The xerophytic Cochlospermums have proved self-fertile so far as they have been observed (I have investigated only *C. vitifolium* (Willd.) Spreng).

The species of the little family Winteranaceae are all self-fertile. The situation in the Violaceae is well known only in Viola, although a few species of Rinorea and Gleospermum are self-fertile; and St. Hilaire (K.) noted cleistogamy in *Hybanthus Ipecacuanha* (L.) Taub. Many of the violets have cleistogamous as well as chasmogamous flowers. So far as I can learn from published records and from seedsmen, there is no true self-sterility even in the latter. The Flacourtiaceae, with twelve subfamilies and about seventy genera, have been subjected to very little experimental work, but there is no indication of self-sterility. I have investigated one or more hermaphroditic species in eight genera; each proved to be self-fertile. Self-sterility is prevalent in the little family Turneraceae in all probability, however, for Urban states that more than seven-eighths of the seventy species of Turnera are heterostyled. Both monœcism and diœcism exist in the Passifloraceae. The two genera of importance are Adenia (30 species) which is monœcious and is supposed to be wholly self-fertile, and Passiflora (300 species) which is hermaphroditic and homogamous. Some thirty species of Passifloras have been investigated. Among them, *P. alata* Ait., *P. coerulea* L., *P. edulis* Sims., *P. gracilis* Jacq. (weakly), *P. laurifolia* L., *P. quadrangularis* L., and *P. racemosa* Brot., have been found to be self-sterile (Mowbray, Scott, Munro in Darwin, A. & P.). To these species I can add *P. vitafolia* H. B. K. The rest, where not diœcious, appear to be self-fertile, but one can rest assured that some of the latter are weakly self-sterile. On the basis of the sampling done, therefore, it is probable that there are at least 100 self-sterile species in the genus. Dimorphic flowers appear here as a rarity but heterostyly is unknown.

Of the remaining small groups, only the Loasaceae are hermaphroditic. They appear to be autogamous, when not protandrous, and sometimes even cleistogamous (*i.e., Loasa triloba* Comb.); but the relationship are not well known. One suspects that the monœcious genera, which make up the remainder, are all self-fertile. This is true of all the cultivated Begonias, a genus having over 400 species, many of which have been investi-

gated rather carefully. I have had experience with about 30 species. Darwin (A. & P.), it is true, found what he thought was self-sterility in hermaphroditic flowers of *B. frigida* A. DC., but this case should be reinvestigated in the light of modern knowledge. It should be noted, perhaps, that among these generally monœcious groups the commonly diœcious *Carica papaya* L. is self-fertile in monœcious or perfect-flowered varieties.

28. Opuntiales

The Cactaceae are wholly hermaphroditic and might be expected to have developed some self-sterility, but none has been demonstrated. A large number of species have been tested of all the important genera, and they have been found to be self-fertile. Testing has been simple because, with the exception of Peireskia, the flowers are usually solitary.

A certain amount of cross-pollination is inevitable because protandry is common, and the pistils are usually longer than the stamens. Natural hybrids are not unusual, particularly in Cereus and Phyllocactus. But the true reason for lack of self-incompatibility is probably the fact that asexual reproduction is so general that it may be called the characteristic means.

I have studied various sterile clones of *Opuntia Ficus-indica* (L.) Mill., and *Peireskia aculeata* Mill., *P. Blea* (HBK.) DC., and *P. grandiflora* Haw., where there appeared to be some suggestion of self-sterility. The non-fruiting clones of Opuntia proved to have bad pollen. Where the pollen was good, there was self-fertility. Of the Peireskias, the first two were self-fertile. The third did not fruit, although the pollen was excellent and the germination high. There is reason to believe, however, that lack of fruit-setting was done due to too cold nights (February, at Soledad, Cuba), rather than to self-incompatibility.

29. Myrtiflorae

The Thymelaeaceae are usually hermaphroditic and homogamous, but polygamy, monœcism, and diœcism are not infrequent. The stamens are often found in two rings at different heights. There is dimorphism or trimorphism in Aquilaria, Phaleria, Gnidia, Linodendron, Dais, etc. But whether there is any true heterostyly or self-sterility has not been determined. There is

a record of self-sterility in *Daphne mezereum* L. (Ludwig, 1900), but the species has also been reported as self-fertile (H. Müller). There is some dimorphism of the same type as the above in certain diœcious species of the Elaeagnaceae. What relation it has, if any, to heterostyly is problematical. *Wikstroemia viridiflora* Meissn. (= *indica* C. A. Mey.) is apogamous (Ernst, 1918).

Heterostyly is found in 27 species of the Lythraceae, but is confined to herbaceous species. Trimorphism exists in four species of Lythrum, three species of Nesaea, and in *Decadon verticillatus* (L.) Ell. Dimorphism exists in ten species of Lythrum, five species of Nesaea, three species of Rotala, and in *Pemphis acidula* Forst. (Koehne, Pflanzenreich). Self-fertility is known in some species of Lythrum and Rotala, as well as in other genera; and there is cleistogamy in Rotala, Ammannia, Peplis, and Nesaea. The only record of self-sterility in homogamous species is for *Cuphea purpurea* Hort. (Gärtner), but presumably it is found elsewhere.

Very little can be said about the various groups making up the remainder of the Myrtiflorae. There is some information that the genera Crypteronia, Blatti, Duabanga, Punica, and the various genera of the Lecythidaceae are self-fertile, because travellers find isolated fruiting trees. Conditions in the Rhizophoraceae are unknown. Nyssa appears to be self-fertile despite a tendency to a polygamo-diœcious relationship. The Combretaceae are said to be impossible to self-fertilize on account of protogyny. The Halorrhagaceae are also protogynous, and have some queer gynomonœcious relationships which require investigation. Thus the only families requiring consideration are the Myrtaceae, the Melastomaceae, and the Oenotheraceae.

The Myrtaceae is a wholly hermaphroditic family of over seventy genera and nearly three thousand species. There are seven genera of over 100 species each—Psidium, Myrcia, Eugenia, Jambosa, Syzygium, Eucalyptus, and Melaleuca. They are especially abundant in Australia and South America, and are largely confined to tropical or subtropical climates. One should expect, on general grounds, that some self-sterility would have been developed, considering that homogamy is almost universal, with the exception of a few protogynous species. But

this is not the case. All that I can learn from field botanists and from published records indicates complete self-fertility of single isolated trees. And the observations give us a fairly good cross-section of the family. My own personal experience includes over a dozen genera and about fifty species. The explanation that one inclines to offer is that the family is made up of trees and shrubs.

The Melastomaceae is, in some ways, a more varied family, with the 1800 species divided into about 150 genera. It contains both annual and perennial herbs, as well as shrubs and trees. Pollination is thought to be almost exclusively by insects. The genera are all hermaphroditic except Lyndenia, where diœcism has been developed. There are a number of epiphytes. The family is largely tropical in its distribution.

Records as to self-fertility, either through publications or through observations of experienced botanical collectors, have been hard to find. They are confined to a few trees, to species useful for fibers or coloring matters, and to genera like Tibouchina, Rhexia, etc., where the plants have been brought into cultivation in hothouses. So far as can be learned, there is no physiological self-sterility in the group. There are indications, however, that insect aid is necessary for normal pollination, even where there is no necessity apparent. The inflorescences are usually cymose, and geitonogamy is evidently common. Individual flowers, also, are frequently constructed so as to be veritable traps for small species of Diptera and Hemiptera. But, whatever the situation, insect aid is commonly a requisite. Darwin found that little or no seed was set in isolated plants of *Centradenia floribunda* Planch., *Rhexia glandulosum* Bertol., *Monochaetum ensiferum* Naud., *Heersia rosea* Triana (*Heterocentron mexicanum*), and an unnamed species of Pleroma; and Knuth reports that he described them as self-sterile. Instead, Darwin notes that they are "sterile without insect aid," but "produce plenty of seed when self-fertilised."

Self-sterility is sporadic in the Onagraceae. Raimann (E. & P.) divides the family into eight tribes, but four of them, Hauyeae, Gaureae, Lopezieae, and Circaeeae, contain only about fifty species altogether, and their fertility relationships are unknown. In the Jussieueae, a number of species of Jussieua and of Ludwigia have been tested and have proved to be self-fertile

with one exception. Darwin reported self-sterility in *Jussieua grandiflora* Ruiz & Pav. In the Epilobieae, the few species of the large genus Epilobium that have been made the basis of genetic work, have been found to be self-fertile, but since it has not been well sampled, self-sterility may well be present. Gärtner reports self-sterility in several species of the genus Fuchsia, which makes up the tribe Fuchsieae, although no later evidence has appeared.

The most important tribe, in the eyes of experimentalists, is the Onagreae, however, because of the part it has played in the development of genetics. Here many species have been tested for their fertility relationships; and these prove to be rather complicated, owing to the intricate way in which the so-called "complexes" due to reciprocal translocations are inherited. The subtribes Boisduvalinae, Clarkiinae, and Gayophytinae, are apparently all self-fertile. Raimann's other three subtribes have been grouped by later taxonomists into the single genus Oenothera having 14 or more sections. The most complete tests of fertility relationships have been made in the sections Eucœnothera (Onagra), Raimannia, and Anogra. Most of the species have no true physiological self-sterility. Dr. Stirling Emerson, who has made many observations, writes that he believes that it is safe to conclude that the species belonging to the sections Eucœnothera (Onagra) and Raimannia are all self-fertile. On the other hand, careful tests show that *Oe. macrosiphon* Wooton & Standley—which some authors place in Eucœnothera, but which Emerson thinks should be placed in a new section—is self-sterile and has certain reactions comparable to Nicotiana. Other species that are apparently self-sterile, but for which Emerson's data are not so conclusive, are *Oe.* (Anogra) *californica* Wats., *Oe.* (Pachylophus) *caespitosa* Nutt. var. *marginata* (Nutt.) Munz and var. *montana* (Nutt.) Munz, and *Oe.* (*Lavauxia*) *acaulis* Cav.

30. Umbelliflorae

Much has been written about sexual reproduction in the Umbelliflorae, whether the flowers are protogynous, protandrous, or homogamous, whether the inflorescences are made up of individual blossoms having similar or different degrees of fertility,

or having different sexual specializations. All this is a waste of time from the general biological viewpoint. The flowers are generally entomophilous, although there is considerable anemophily; and pollen is always available, both for selfing and crossing, owing to the varying times of maturity of the individual flowers. Isolated plants, therefore, can always set fruit, unless there is physiological incompatibility.

The Araliaceae, consisting of about 700 species, divided into fifty or so genera, consist almost entirely of trees, shrubs, and woody vines. Herbs are rare. There is a tendency toward bisexuality. Practically no experimental work has been done on the group, with the exception of a few observations on Aralia, Panax, and Hedera. No self-sterility has been discovered, and it is hardly likely that it will be found, for the individual plants of any given species hardly ever grow in great groups, but rather are found singly and more or less isolated from each other. In spite of this tendency, good sets of seed are common.

The Umbelliferae form a much larger family, with over 2000 species, divided into over 230 genera. Nearly all of them are herbs. The flowers are hermaphroditic, with rarely a tendency towards bisexuality through abortion.

It is almost a characteristic of the family for the fruits to have emergences of various kinds which make them easily carried around by animals, and this may have something to do with the wide distribution of the species and with their variability, but the plants have so little ornamental or economic value, and have been utilized so rarely by plant breeders, that almost nothing has been discovered about their fertility relationships. Parsnips, carrots, and parsley (Petroselinum) have been bred extensively and are self-fertile. A few spicy seeds, Carum, Coriandrum, Angelica, etc., are also grown by seedsmen; but no attempt has been made to improve them by genetic methods. There are no indications pointing to self-sterility, but nothing can be said definitely on the subject. Only about 20 species in some 8 genera are known to be self-fertile, although Knuth writes over 60 pages on the subject.

The little family Cornaceae, with a large proportion of the family made up of trees and shrubs, is pretty generally known to be self-fertile, when not diœcious.

METACHLAMYDEAE

1. Ericales

The various little groups appended to the Ericaceae all appear to be self-fertile. This is certainly true of the two Clethras native to this region. It is also true of the native species of Pirola and Monotropa, although the floral mechanisms are such that natural self-pollination is frequently excluded. No direct observations are available on the Lennoaceae, but the flowers of these parasitic plants are such that autogamy is highly probable and self-fertility practically certain. Of the little family Diapensiaceae, it can be stated that *Diapensia lapponica* L. and *Galax aphylla* L. are self-fertile, and are almost always self-pollinated as the flowers open. No observations are available for the Epacridaceae, except that Drude (E. & P.) believes that Epacris shows protogyny and Sphenotoma shows protandry. The flowers of several species are so constructed that autogamy must be frequent, however, and unless physiological self-sterility is later discovered, must be considered to be self-fertile.

The only large family of the group, if one may call a family of 1400 species large, is the Ericaceae. Since there are many genera among the nearly 70 listed that have only from 1 to 5 species, it might be said, with some justice, that the sexual relations are not so well known as they should be, from our point of view. Nevertheless, a fair number of species of the great genera Rhododendron, Gaultheria, Vaccinium, and Erica, have been studied, as well as representatives of some of the smaller genera, such as Ledum, Menziesia, Loiseleuria, Kalmia, Leucothoë, Andromeda, Lyonia, Epigaea, Cassiope, Arctostaphylos, and Gaylussacia.

All are woody plants, ranging from low creepers to large shrubs. Exceptionally, as in Epigaea, there is a subdiœcious condition, with dimorphic styles and stamens, though no heterostyly, in the Darwinian sense. All appear to be insect-pollinated. This is necessary because of widespread protandry, and because the great majority of the genera are characterized by more or less erect flowers that have pistils standing very much higher than the stamens. Moreover, though most of the group have pollen grains that could be carried by the wind, in the Rhododendroideae the pollen is massed together by gummy substances.

The species tested in the above genera (excluding Epigaea), together with isolated species of nearly a score of other genera, are physiologically self-fertile. It has been said that *Kalmia latifolia* L. is self-sterile (K., Beal, 1878), and it is the general belief of many nurserymen that most of the cultivated species and hybrid forms of Rhododendron are self-sterile. This belief arose from the fact that bagged inflorescences set no seed. But this is due to the protandrous tendency, the sticky pollen, and the protruding, elongate styles. Seed is obtained when hand-pollinations are made. It would be rash to assert that no self-incompatibility exists in the family, or the order, but it is safe to say that if it does exist, it is very rare.

2. Primulales

There have been no pollination studies on the Myrsinaceae. It is unlikely, however, that any self-sterility will be found because (1) of the prevalence of trees and shrubs in the family, and because (2) of the amount of monœcism and diœcism.

In the herbaceous family Primulaceae, on the other hand, self-sterility abounds. It was here that Sprengel first discovered heterostylic dimorphism, in *Hottonia palustris* L. The only other species in the genus, *H. inflata* Ell., is not heterostyled and bears cleistogamous flowers. Dimorphic heterostyly is pretty well distributed through the tribe Androsaceae. It is found in the very great majority of the more than 200 species of Primula described by Pax and Knuth (1905, Pflanzenreich, IV, 237), and in Dionysia, Douglasia, and Androsace of the Primulineae; in Soldanella, of the Soldanellineae; and in Hottonia of the Hottoniinae. It is also found in Lysimachia, Lubinia, and Glaux, of the Lysimachieae. There may be other heterostyled genera in these sections, but apparently there are only homostyled plants in the remainder of the family.

The heterostyled forms show varying degrees of self-sterility, so much so that many would ordinarily be listed as self-fertile; but a comparison of the experimental results of Darwin and of later workers leads one to believe that most of the supposedly fertile races are really pseudo-fertile, and that physiological incompatibility is a rather general characteristic of the group. Possibly there are exceptions.

There is autogamy in homogamous *Primula mollis* Nutt., in *Centunculus minimus* L., and in several Cyclamens (D.); but there is also self-sterility in homostyled forms, as *Primula verticillata* Forsk. and *Cyclamen persicum* Mill. (D.), *Lysimachia nummularia* L., and *Trientalis europaea* L. (Ludwig, 1900; Müller says protogynous) and *Cortusa Matthioli* L. and *Douglasia Vitaliana* Benth. & Hook. (Scott, 1865. Heterostyled?). Doubtless, other self-sterile species or varieties are to be found. Numerous homostyled forms are known in otherwise heterostyled species, and while they are often more self-fertile than the heterostyled forms, the reaction appears to be what we have called pseudo-fertility.

3. Plumbaginales

Some Brazilian species of Plumbago and Statice are dimorphic, according to Fritz Müller (M.). MacLeod (1887) showed that *Statice Limonium* L. is self-sterile; and thinks that it is an example of a trend from heterostyly to gynodiœcism. Müller states that Treviranus found species of Armeria to be autogamous.

4. Ebenales

The Sapotaceae, consisting wholly of trees, is a family having hermaphroditic flowers which occasionally become polygamous (Labatia, Sideroxylon, Vitellaria) or unisexual (Diploknema, Omphalocorpum) through abortion. I suspect, through investigations of species in Achras, Calocarpum, Chrysophyllum, Dipholis, Lucuma, Malacantha, Mimusops, and Pouteria, that the entire family is self-fertile, although insect-pollinated in the main, and sometimes protogynous.

The Ebenaceae, consisting only of trees and shrubs, are commonly monœcious, frequently diœcious, and but rarely hermaphroditic. The majority of the species is found in the monœcious or andro-diœcious genus Diospyros. Isolated plants fruit in the four species that I have investigated.

There are no published records on the little families Symplocaceae and Styracaceae. They consist of trees, however, and perhaps may be assumed to be self-fertile. *Symplocos salicifolia* Griseb., *S. tinctoria* (L.) L'Hér., *Halesia carolina* L.,

Styrax grandifolia Ait., and *S. americana* Law. produce fruit when isolated.

5. Contortae

The Oleaceae form a curious family. It consists of trees and shrubs; it has a great deal of polygamy, monœcism, and some diœcism, yet there are records of heterostyly and self-sterility.

Müller states that Kuhn mentions Jasminum as containing dimorphic species. Knoblauch (E. & P.) attributes the same observation to C. B. Clarke (1882). The question of self-sterility in such forms apparently has not been determined. *J. azaricum* L. and *J. officinale* L., the only species with which I have worked, are homogamous and self-fertile. The Forsythias, on the other hand, are definitely self-sterile and heterostyled, yet there is something queer in connection with them. There are supposed to be two species from China, *F. suspensa* Vahl. and *F. viridissima* Lindl., having several varieties. Gray long ago pointed out that only the short-styled form of the first and the long-styled form of the second are found in cultivation in the vicinity of Boston. Each form, attributed to each species, appears in herbaria, however, although there is some question as to whether there is justice in the distinctions noted. Hildebrand (*Bot. Ztg.*, 52, 1894) was able to get seed only from *F. suspensa* (short style) crossed with *F. viridissima* (long style). The result was a plant differing somewhat from either parent, which has been called *F. intermedia* Zabel.

No other species are known to be self-sterile, though it is suspected in Syringa. Cases of definite self-fertility are found in Forestiera, Fraxinus, Syringa (all three sections), Noronhia, Olea, and Ligustrum.

No species of the Loganiaceae are known to be self-sterile or heterostyled, in Darwin's sense, although there is some floral dimorphism (Gelsemium, etc.). There are practically no experimental observations on the group, and it is quite possible that self-sterility may be found. I can testify only to a few cases of self-fertility of isolated plants observed in Buddleia, Gelsemium, and Strychnos.

The Gentianaceae, being a family consisting largely of herbs, and having flowers that are usually hermaphroditic and homogamous and but rarely polygamous with a tendency to unisexuality,

may have a number of self-sterile species, but the only one definitely known to be self-sterile is *Menyanthes trifoliata* L., a heterostyled species that is the only representative of the genus. The two related genera Villarsia and Limnanthemum also contain dimorphic heterostyled species, though just how many no one appears to have noted. In Erythraea of the Gentianoideae, there are also markedly heterostyled species, while in some other genera there appears to be a weak heterostyly. In the genus Gentiana, which contains about two-thirds of the total number of species belonging to the family, there is some protandry and some protogyny, but most species are said to be homogamous and tend very largely to be autogamous or even pseudo-cleistogamous.

The Apocynaceae is an exceedingly variable group, as one may judge from the fact that the thousand or so species are divided into about 130 genera. There appears to be no heterostyly, but there are several cases of physiological self-sterility. This is exceptional, since the family consists of woody plants, though there are only a few trees. The majority of the species are shrubs or lianas, with occasional creepers, as in Vinca.

Natural pollination has been studied carefully in many genera by Darwin, Müller, Delpino, and others. The details are interesting but cannot be considered here, except to say that there are numerous arrangements whereby self-pollination is practically excluded. The plants are most of them self-fertile, however, even where this is the case. I have studied species of Allamanda, Carissa, Echites, Funtumia, Kopsia, Plumeria, Rauwolfia, Strophanthus, Tabernaemontana, Thevetia, and Vinca.

F. Müller (*Bot. Ztg.*, 1868) found the first case of true self-sterility in *Tabernaemontana echinata* Aubl. Darwin (C. & S. F.) found indications of self-sterility in *Vinca major* L. and *V. rosea* L.; and I have found that some strains of *V. minor* L. are self-sterile. Ludwig (1900) described the fertilization of *Apocynum hypericifolium* Ait. and *A. androsoemifolium* L. by butterflies, through the cementing of the pollen on the insect's tongue. There was some suggestion of self-sterility, but hardly a definite proof.

In my own experiments, *Allamanda Blanchetii* ADC., *A. cathartica* L. var. *grandiflora* (Lam.) Rafill, and *A. cathartica* L. var. *Hendersonii* (Bull) Rafill, proved to be self-sterile,

though seeding when crossed. The pollen was 25–30 per cent bad, however, with frequent production of diads; and this may have had something to do with the situation. Smears of the 60 mm. styles showed that pollen-tube growth was so slow that the tubes had not nearly reached the ovary before the flowers changed from violet to yellow and showed signs of withering, but it was not possible to compare these growth-rates with those of crossed tubes.

Beaumontia grandiflora (Roxb.) Wall., though always autogamous, proved to be self-sterile in Cuba, the pollen tubes growing down the long style at a very slow rate; though whether it is self-sterile in its native habitat or not, is unknown. The pollen appeared to be 80 per cent good in aceto-carmine preparations.

Several varieties of *Nerium Oleander* L., with about the same percentage of good pollen, were self-sterile. The pollen grains germinated well, but after 24 hours in the stylar tissue, the pollen tubes were only about ten times the diameter of the pollen grains in length. Apparently pollen tubes occasionally reached the ovaries, for the ovaries became markedly larger, but the seeds if any were fertilized did not mature.

Odontodenia grandiflora (G. F. W. Mey.) K. Schum., with excellent pollen, and a native West Indian species, also proved to be self-sterile.

It has been noted, more than once, that the members of the Asclepiadaceae rival the orchids in intricacy of mechanisms promoting cross-fertilization. These have been described in great detail, particularly by Delpino. It is clear that cross-pollination occurs naturally with a high degree of frequency; but botanists have not taken the trouble to find out whether the plants, which are usually homogamous, are self-fertile or not. Kuhn (M.) observed cleistogamy in Stapelia. And Delpino reported self-sterility in *Stephanotis floribunda* Brug., *Hoya carnosa* R. Br., and *Periploca graeca* L., the evidence being satisfactory in only the second case.

6. Tubiflorae

Convolvulineae. In the Convolvulaceae, autogamy has been observed in Quamoclit, Convolvulus, Calystegia, and in many species of Cuscuta (Peter, E. & P.). Cuscuta also contains

many cleistogamous species. In addition, cleistogamous flowers are known in *Ipomoea pes tigridis* L. and *Dichondra arvensis* L. The species of Ipomoea, Pharbitis, and Convolvulus that have been of interest to geneticists have been self-fertile.

Müller says that Darwin found species of Calonyction were self-sterile, but I have been unable to check the statement. Similar assertions are made about *Ipomoea Batatas* L. One cannot be certain about the matter, however, because sweet potatoes frequently show large proportions of bad pollen. Miller's experiments (1937) indicate self-fertility.

It is characteristic of this family that the flowers are short-lived, usually opening for one day only. For this reason, it is difficult to determine the fertility arrangements. The general evidence, however, indicates that self-sterility is rare. My own experiments have indicated, but have not definitely proved, that *Ipomoea crassicaulis* (Benth.) Robins. is self-sterile.

The Polemoniaceae, with only 8 genera, is, in general, self-fertile, although there is some protandry. Autogamy is widespread. The situation (self-fertility) is known in many species of Cantua, Phlox, Collomia, Gilia, and Polemonium. *Collomia grandiflora* Dougl. is cleistogamous (Peter, E. & P.). Darwin (D. F. of F.) states that *Gilia pulchella* (= *G. aggregata* Spreng.), *G. micrantha* Stend., and *G. nudicaulis* A. Gray, and perhaps *Phlox subulata* L. are heterostyled, the statement being based chiefly on differences in pollen size. The last species, he suggests, was once more definitely heterostyled, but has become subdiœcious. *Cobaea penduliflora* Karst. is self-sterile, according to Behrens (1880); Peter (E. & P.) also lists *C. scandens* Cav., but Behrens says that it is dichogamous.

Borragininеae. The species of Hydrophyllaceae are generally somewhat protandrous, but are self-fertile, so far as is known (see Willis, 1895).

Very little is known about the reproductive physiology of the Borraginaceae. It is an extremely variable family of 85 genera and about 1600 species, containing both annual and perennial herbs, shrubs, and trees. The flowers are nearly always hermaphroditic, but with an occasional tendency toward unisexuality. Knuth believes that the small-flowered forms, such as Lithospermum and Myosotis, are practically always self-pollinated, and that the facility with which self-pollination occurs

normally is inversely proportional to floral conspicuousness, but definite information as to self-fertility has been obtained in only a few cases. There is cleistogamy in American Lithospermums, in Pectocarya, and in two of the sections of the large genus Cryptanthe, accompanied in the last two genera by marked floral dimorphism (Information from Dr. Ivan Johnston).

Dimorphism is common, and true heterostyly occurs (Pulmonaria). Hildebrand (1864) experimented with both forms of *Pulmonaria officinalis* L. and *P. angustifolia* L. They were completely sterile in Germany, although Darwin found a moderate amount of fertility in England. Other species of the same genus are not heterostyled.

Numerous other genera (Amsinckia, Anchusa, Arenbia, Echium, etc.) have the floral parts so variable that heterostyly is simulated. Still others have definite stylar dimorphism (Cordia) but with the stamens the same length. It is not known whether such species are self-sterile.

Symphytum bulbosum Schimp. (Ludwig, 1900) and *Borrago officinalis* L. (Darwin, reaction weak) are self-sterile, and one suspects that numerous further examples could be found if investigations were made.

Verbenineae. The Verbenaceae are usually hermaphroditic, but with a tendency to polyœcism through abortion. The family contains herbs, shrubs, and trees. There is a trend toward heterostyly. The best known case is in Aegiphila (Darwin). Some of the instances are presumably pseudo-heterostyly without the pollen differences that Darwin found to be requisite. There is some cleistogamy and presumably a great deal of self-fertility. At least this is true of the familiar cultivated species of Verbena, and certain species of Citharexylum, Clerodendron, Duranta, Lantana, Tectona, and Vitex. I have found that no seeds set in *Citharexylum tristachyum* Turcz., *Congea tomentosa* Roxb., *Petrea volubilis* L., and *Verbena pulchella* Sweet, after hand selfings, although the pollen was excellent. Without further work, however, it would hardly be judicious to list them as self-sterile. There are apparently no published records of self-sterility.

The Labiatae form another heterogeneous group of some 3000 species, consisting mostly of herbs, but also containing some shrubs and trees. The flowers are usually hermaphroditic,

though there is a fair amount of gynodiœcism through abortion. Many of the herbs are cultivated as aromatics, yet presumably because of their small flowers, no one has discovered whether self-sterility has been developed. Protandry is widespread, and the flowers are ephemeral, so that even in Salvia, where the most careful tests have been made, and where there is considerable possibility of self-sterility (Darwin), the situation is uncertain. My own experiments have included *Salvia officinalis* L., *S. splendens* Sellow u. Nees, *Leonotis Leonurus* (L.) R. Br., and *Rosmarinus officinalis* L., all with extremely good pollen and little tendency to protandry; and while careful selfings resulted in no seed, the flowers wilted so rapidly that it was difficult to say whether self-sterility or other conditions were at the root of the matter. Only a small percentage of pollen grains germinated while the flowers were still in good condition (one day only), and no tubes could be seen in the styles. It should be noted that there are numerous mechanisms providing for a high frequency of cross-pollination.

Solanineae. One should not be surprised if self-sterile forms should be found in the Nolanaceae. But the only species that have been used for experimental work, as far as can be learned, are *Nolana prostrata* L. and *N. atriplicifolia* Hort. This second species may or may not be the species of Don, but is a true species and not a hybrid. Saunders (1934) found that both types were self-fertile. She speaks of self-sterility in the hybrids but evidently means true sterility.

The important family Solanaceae, with around 1800 species and 70 genera, where deviation from strict hermaphroditism is rare, and where most of the plants are herbs, has probably been tested for self-incompatibility as thoroughly as any other. Von Wettstein (E. & P.) states that there is inconspicuous heterostyly in the two genera Nicotiana and Datura. This is incorrect. There is variability of stamen length and style length in these two genera, as well as in several other genera, but no true heterostyly has been found. The flowers are homogamous. Von Wettstein says there is protogyny; but here, again, the alleged fact is dubious, to say the least.

From one to many species have been shown to be self-fertile very definitely in the following genera: Nicandrae—Nicandra; Solananeae—Lycium, Dunalia, Atropa, Scopalia, Hyocyamus,

Physalis, Capsicum, Solanum, Jaborosa, Mandragora; Datureae —Solandra, Datura; Cestreae—Cestrum, Nicotiana, Petunia, Nierembergia; Salpiglossideae—Salpiglossis, Schizanthus, Browallia, Brunfelsia. There is much autogamy.

It is quite possible, however, that self-incompatibility may be found in any of the larger genera when sufficiently well sampled. The first case was found in *Petunia violacea* Lindl. (D.). This was probably a cultivated form, since incompatibility has been found in certain stocks of all the garden Petunias, and since there is no evidence that pure wild species have ever been tested. Focke states that *Lycium rhombifolium* [probably *L. ovatum* Poir.] is self-sterile.

Stout and Clark (1924) and Clark (1927) have studied seven wild species of tuberous solanums, as well as numerous varieties of cultivated forms. *S. Caldesii* Humb. & Bonpl. var. *glabrescens* Duval, and *S. Chacaense* Bitt. were highly self-incompatible, and *S. Jamesii* Torr. was feebly self-incompatible, under the conditions prevailing at Presque Isle, Maine. The sterility found in cultivated forms was true sterility of various types. I can corrodorate the last conclusion, under the conditions found at New Haven, Connecticut, where I tested about 600 commercial varieties of potatoes over several seasons.

Perhaps the best sampled among the larger genera of the family is Nicotiana, where over 40 species have been given intensive genetic study. The haploid chromosome numbers in the American species are 9, 10, 12, and 24, while a variable set of chromosome numbers has been found in the less heterogeneous group found in and about Australia. The latter are all self-fertile, as well as most of the American species. Goodspeed (private communication) has found two self-sterile stocks among a series of self-fertile stocks in *N. glauca* Grah. *N. pompasana* O. Kuntze and *N. Cavanillesii* Dun. were also self-sterile. In these three species, the haploid chromosome number is 12. All stocks of the eleven other 12-chromosome species tested were self-fertile. Four 9-chromosome species are known:—*N. alata* Link and Otto, *N. bonariensis* Lehm., *N. Sanderae* Hort., and *N. Langsdorffii* Weinm. Here only the last is self-fertile.*

The nearly related family Scrophulariaceae, with about 180 genera and nearly 3000 species, where shrubs and trees are

* Since this paper was prepared, Goodspeed has written that he has found a self-sterile stock of *N. Langsdorffii*.

rarely found, and hermaphroditism (except Digitalis) and homogamy are virtually complete, has self-sterility well distributed. There is some tendency to protandry in Digitalis and Pentastemon, and to protogyny in Calceolaria, Euphrasia, Linaria, Odontitis, Scrophularia, and Veronica, but in no case do these manifestations appear to be extreme enough to prevent the possibility of some natural self-fertilization. There is also cleistogamy in certain species of Linaria, Scrophularia, etc., and there is autogamy in many types.

Self-fertility is known in one or more species or varieties in the following genera: Pseudosolaneae—Verbascum, Celsia; Antirrhinoideae—Angelonia, Calceolaria, Nemesia, Cymbalaria, Elatinoides, Linaria, Antirrhinum, Scrophularia, Chelone, Pentastemon, Mimulus, Bacopa, Micranthemum, Lindernia; Rhinanthoideae—Scoparia, Veronica, Digitalis, Gerardia, Castilleja, Melampyrum, Euphrasia, Pedicularis, and probably in many others.

Self-sterile species are *Verbascum phoeniceum* L. (Kölreuter, Gärtner, Darwin, Sirks), *V. nigrum* L. (Gärtner, Darwin), *V. phlomoides* L. (Focke, 1893), among the Pseudosolaneae; two undetermined stocks of horticultural derivation in Calceolaria, *Nemesia strumosa* Benth., *N. platysepala* Diels, *N. Pagae* L. Bolus, *N. capensis* (Thunb.) Kuntze, *N. cheiranthus* (E. Mey. mss.) Benth., *N. sp.* (Riley, 1935, *N. floribunda* Lehm. and *N. macrocarpum* (Soland.) Druce were self-fertile), *Scrophularia Scapolii* Hoppe (Correns, 1916), *Linaria vulgaris* Hill. (Darwin, weak reactions. Found self-fertile by others), *Antirrhinum majus* L. (Darwin. Baur finds certain races self-fertile along with *A. siculum* Ucr.), a large group of forms of the section Antirrhinastrum grouped by Baur (1932) as *A. latifolium* DC., *A. majus* L. (wild types from North Africa and the Iberian Peninsula), *A. Barrelieri* Bov., *A. glutinosum* Boiss. et Reut., *A. molle* L., *A. meonanthum* Hffgg., and *A. hispanicum* Chav., *Russelia equisetiformis* Schlecht. & Cham. (East), and *Paulownia Fortunei* (Seem.) Hemsl. (East) of the Antirrhinoideae; and *Digitalis purpurea* L. (Darwin. Most stocks are fertile, however), and *Veronica syriaca* Roem. & Schult. (Filzer, Lehman), of the Rhinanthoideae.

It is suggested that, on the basis of the self-sterility found in this family in proportion to species tested, there should be at least 300 species that are self-sterile as a whole or in part.

The Bignoniaceae is an extremely variable family with 100 genera and only 500 species. Herbs are rare. The plants are preponderantly lianas, or shrubs and trees. Fritz Müller (Darwin) thought that there was self-sterility in certain Bignonia species, and Delpino reported it in *Tecoma grandiflora* Delaun. I have found indications of true self-incompatibility in several species of Tecoma, in *Tabebuia serratifolia* (Vahl) Nichols, and in *Tecomaria capensis* (Thunb.) Spach. Nevertheless, more careful work must be done in order to determine the point. The flowers in many species are open only one day, and they are rather markedly protandrous. If unpollinated, they fall during the next day. The stigma lobes open up like a pair of forceps for a few hours and then close, thus hiding the sensitive surface. It is not certain, therefore, that self-pollinations, particularly in Bignonia, Tabebuia, Tecoma, and Tecomaria, reach the stigmas of the long pistils at the time of effectiveness.

A sufficient amount of selfed flowers have set on various species of Catalpa, Crescentia, Jacaranda, Kigelia, Oroxylon, Parmentiera, Spathodea, and certain Tabebuias, to show that they are self-fertile. I am inclined to believe that there is no true self-sterility in the family, therefore, but possibly it may exist.

Almost nothing is known about the remaining families of the Solanineae, but the following notes indicate that self-sterility is not likely to be found very commonly, though it may exist.

The Pedaliaceae are mostly herbs with short-lived flowers, as in the Bignoniaceae. There is a tendency toward protogyny, but a possibility of self-fertilization in most species. There is no marked dichogamy either here or in the Martyniaceae, but the proper time for fertilization appears to be so short that natural self-pollination may often be excluded. Similar remarks are in order for the parasitic Orobanchaceae, though protogyny is more marked than in the others. *Cistanche lutea* Hoffmgg. & Link has cleistogamous flowers.

All the better known members of the Gesneriaceae—*i.e.*, Corytholoma, Isoloma, Sinningia, Naegelia, and Saintpaulia, are known to be homogamous and self-fertile.

The pollination of the Lentibulariaceae has been described in detail by Hildebrand (*Bot. Ztg.*, 1869), at least for Utricularia and Pinguicula. His intention was to show how the species could be cross-pollinated by insects. He succeeded in this, of

course; but in the meantime he brought out the fact that self-fertilization is not only possible but very frequent, and that even cleistogamy is not rare. There is a possibility that *Utricularia vulgaris* L. is self-sterile.

The Globulariaceae, though somewhat protogynous, are known to by physiologically self-fertile.

Acanthineae. The Acanthaceae is a family of over 2000 species, which are divided into about 180 genera. It consists very largely of herbs, though there are some shrubs and occasional small trees. The species are all hermaphroditic, and the flowers are nearly all homogamous or slightly protandrous. And there is frequent cleistogamy (Ruellia, etc.). With this set-up, one might expect a considerable amount of self-sterility, provided the fact that the family has the most extreme specialization of all flowering plants in anther form and pollen form does not militate against physiological provision for cross-pollination.

Unfortunately, almost no experimental work has been done with the group, and casual observations are worthless because the showy flowers, with long stamens and pistils, are very delicate. Darwin (C. & S.) experimented with *Thunbergia alata* Boj. and found that selfed plants set seed as well as crossed plants, though both fruited poorly. I have had opportunity to make observations on only three species in which isolated plants set no fruit. *Graptophyllum pictum* (L.) Griff. had 95 per cent bad pollen. *Crossandra undulaefolia* Salisb. had good pollen, but none germinated under conditions found in Cuba during the latter part of February. The corollas remained open but one day, and then fell off, leaving the protruding styles, which appeared to stay in good condition for as long as five days; nevertheless, no pollinations at any time during this period showed germinating pollen grains. The third species, investigated at the same time, was *Thunbergia grandiflora* Rox., of which there was a blue variety and a white variety. Both had good pollen. They were known to set fruit reciprocally, but not after selfing. A series of selfings and reciprocal crossings was made. The pollen germinated well in every case, and tubes were found in the smeared styles when stained with aceto-carmine. No fruits were set because the treated flowers were finally whipped off by wind, but the selfed pollen tubes grew only about 2 mm. in length in 24 hours, while the crossed pollen tubes grew about 4 times as far.

One may conclude tentatively, therefore, that self-sterility occurs in the family.

Myoporineae. *Phrymineae*. Nothing definite is known about either of these little groups. *Phryma Leptostachya* L. is said to be self-fertile, though somewhat protogynous.

7. Plantaginales

Practically speaking, this order is illustrated only by the 200 species of the hermaphroditic-flowered genus Plantago; although there are two monotypic genera, Litorella, which is monœcious, and Bougeria, which is polygamous. There is cleistogamy (*P. virginica* L.), gynodiœcism (*P. lanceolata* L.), heteranthery (*P. major* L.), and other slight sexual variations. Moreover, the plants are mostly wind-pollinated. For these reasons, it is unlikely that physiological incompatibility will be found in the group.

8. Rubiales

The Rubiaceae contains over 4500 species, divided into approximately 350 genera. The family is generally hermaphroditic, though there is a marked trend toward bisexuality through abortion. It contains some herbs, but is characterized rather by the numerous species of trees and shrubs.

There are more heterostyled genera and species than are found in any other family, and the proportion of heterostyled species is probably also greater. It is all dimorphic heterostylism.

Following Schumann in Die Pflanzenfamilien, the situation is roughly as follows:

Cinchonoideae			
Condamineae	Gen.	1–10	Homostyled
Oldenlandieae	Gen.	11–42	Some heterostyly, Houstonia, Otomeria
Rondeletieae	Gen.	43–59	Homostyled
Henriquezieae	Gen.	60–61	Homostyled
Cinchoneae	Gen.	62–96	Often heterostyled, Cinchona, 40 sp. het.
Naucleeae	Gen.	98–105	Rarely heterostyled
Mussaendeae	Gen.	106–141	Occasionally heterostyled, Gouldia
Gardenieae	Gen.	142–196	Many diœcious
Alberteae	Gen.	197–205	Homostyled
Knoxieae	Gen.	206–207	Often heterostyled, Knoxia, part; Pentanisia

Coffeoideae		
Vanguerieae	Gen. 208–217	Homostyled
Guettardeae	Gen. 218–227	Many heterostyled
Chiocacceae	Gen. 228–237	Homostyled
Ixoreae	Gen. 238–251	Homostyled
Psychotrieae	Gen. 252–283	Part heterostyled; Rudgea mixed
Paederieae	Gen. 284–288	Rarely heterostyled
Anthospermeae	Gen. 289–308	Frequently diœcious, rarely heterostyled
Morindeae	Gen. 311–317	Homostyled
Spermacoceae	Gen. 318–335	Sometimes heterostyled, Schwendenera, Perama—part, Borreria—part
Galieae	Gen. 336–346	Monœcious—polygamodiœcious in part, homostyled

The earliest experiments on self- and cross-fertility in the family were those of Darwin (D. F. of F.). He found that both types of flowers of *Mitchella repens* L. were self-sterile, but that the short-styled plants had the more active reaction. The short-styled forms of a species of Borreria and of *Houstonia coerulea* L. (observations of A. Gray) were also found to be more perfectly self-sterile.

The later investigations have been brought together by Fagerlind (1937), who should be consulted. It appears that although autogamy is favored in several species of Coffea, Crucianella, etc., self-sterility is as frequent in shrubs and trees as in herbs, and is found in many homostyled species; in fact, it is the rule throughout the tribe Galieae, and, through experiments with *Coffea liberica* Hiern., *C. Laurentii* Wildem. (= *C. robusta* L. Linden), and *C. conephora* Pierre, ex Froehne, is definitely known to be due to differential pollen-tube growth.

I have had the opportunity of observing natural fruiting of single isolated specimens of one or more species of Aliberta, Calycophyllum, Exostema, Genipa, Hamelia, Ixora, Morinda, Pogonopus, Portlandia, Posoqueria, Psychotria, and Rondeletia. In spite of a general prevalence of protandry in the individual flowers, there is always an abundance of pollen by which self-fertilization can be obtained; and these species must be called self-fertile, although it is quite probable that there is some manifestation, however weak, of the self-incompatibility reaction. The only strong self-incompatibility reactions were found in *Gardenia Thunbergia* L. f. and *Mussaenda luteola* Delile, in both of which the pollen was extremely good.

A small amount of self-sterility is distributed in the Caprifoliaceae, and is probably found in all the tribes. The common

species of Sambucus are self-fertile, but I found definite self-sterility in one stock of *Sambucus Simpsonii* Rehd., though it is true that the plant had 15 per cent bad pollen. The Viburnums are usually self-fertile, but there is some indication of self-sterility in several of the species in the large collection at the Arnold Arboretum, and Dr. Sax has found that *V. Carlesii* Hemsl. is completely self-sterile. All species of Symphoricarpus are self-fertile, but there is some indication, though without definite proof, of self-sterility in Linnea. The species of Lonicera and of Diervilla, of which there is a large collection in the Arnold Arboretum, are all self-fertile, with the exception of certain forms of *Lonicera japonica* Thunb. There is no heterostyly.

There is no indication of self-sterility in the other families of the order. The cultivated types of the Valerianaceae—*i.e.*, Valeriana and Valerianella—are self-fertile, although there is some tendency toward polygamo-diœcism. So, also, are the better known genera of the Dipsacaceae; Dipsacus, Scabiosa, Cephalaria, Morina, when protandry is not marked enough, as it sometimes is, to make self-fertilization impossible.

9. Cucurbitales

The Cucurbitaceae are preponderantly monœcious or diœcious. The only information on self-fertility comes from observations on the commonly cultivated monœcious species belonging to less than a score of the 84 genera. They are all self-fertile, and this, perhaps, is to be expected.

10. Campanulatae

The Campanulaceae is a fairly compact family, consisting of about 1200 species. Most of them are herbs, though there are a few shrubs and trees. They are hermaphroditic, with only a rarely manifested tendency toward unisexual flowers through abortion. The family divides naturally into three subfamilies, Campanuloideae, Cyphioideae, and Lobelioideae; but the Cyphioideae contains only 4 genera and about 25 species, on which there are no experimental observations.

In the Campanuloideae, there is cleistogamy in Specularia (all American species), and possibly in other genera. Protandry is marked throughout the group, but nevertheless the species are generally self-fertile. This is known to be the case for

numerous species of Campanula, and for several species of Adenophora, Trachelium, Wahlenbergia, Phyteuma (usually autogamous), and Jasione. There appears to be no published record of self-sterility, except Darwin's supposed record on *Campanula carpathica* Jacq., which was accepted by Knuth; but Darwin merely says that "it is quite sterile without insect aid." I have tried several stocks of this species, together with many other Campanulas, and they have always proved to be self-fertile when hand-pollinated.

It would not be just, however, to maintain that self-sterility is wholly improbable in the Campanuloideae, for the Lobelioideae, where the plants are just as protandrous as in the other subfamily, has some well authenticated cases. Gärtner (1849) established the fact that *Lobelia fulgens* Willd. is self-sterile; and there is fair evidence in *L. fulgens* var. *ramosa* (Darwin) and *L. cardinalis* L. (Focke, 1881). It is also possible that *Isotoma axillaris* Lindl. (Hildebrand, 1869) and some other Isotoma, probably *longiflora* Presl (Darwin), are also self-sterile.

There is Darwin's reference to self-sterility in *Leschenaultia tubiflora* R. Br. of the little Australian family, Goodeniaceae. Schönland (E. & P.) also reports *Selliera radicans* Cav. to be self-sterile. One obtains the idea from published descriptions, however, that self-fertilization obtains in many cases. The flowers are protandrous, and the pistils show many variations; but the stigma usually ends in a kind of cup which receives pollen before the flower opens.

There is some development of unisexuality in both the Candolleaceae and the Calyceraceae, and for this reason, together with the marked protandry, self-sterility is unlikely. There are no specific observations on these groups, however.

The great family Compositae, which consists mainly of hermaphroditic herbs, and contains over 800 genera and approximately 14,000 species, probably has more widespread and persistent self-sterility than any other group. It is true that little is known, experimentally, on the family as a whole; nevertheless, owing to the very high percentage of self-sterility found whenever genetic work is reported, it would not be astonishing to find, ultimately, that fully a quarter of the species show the sterility reaction in some degree.

It is well known, of course, that there is apogamy in Antennaria, in Hieracium, in Taraxacum, in Eupatorium (Ernst, 1918), and probably in other genera, that there is sterility, or unisexuality, or both, in certain parts of the inflorescence in various groups; but these sexual aberrations apparently do not exclude the development of self-incompatibility in closely related types. Unfortunately, there are not sufficient observations to make it possible to correlate these morphological specializations with the physiological reactions.

The only early reference to self-sterile species is that of Darwin (C. & S.) to his experiments with cultivated varieties of *Senecio cruentis* DC.

In post-Mendelian days, the most extensive series of experiments have been those of Stout (1916, 1917, etc.) on *Cichorium Intybus* L. Stout found that while many stocks were quite self-sterile, still there were others that were sufficiently self-fertile to make it possible to obtain selfed seed rather easily. My own personal feeling, after a careful study of Stout's data, is that the species, as a whole, is a self-sterile one, and that the apparently self-fertile plants actually possess the reaction weakly (pseudo-fertility). Moreover, it seems probable that highly variable reactions of this kind are characteristic of most of the composite species in which self-sterility has been observed.

Recently, the Compositae have come in for more attention by geneticists. Their observations have not been published in detail, but permission to quote results has been obtained from several of the investigators. In addition, through the kindness of Mr. C. M. Rick, information has been received concerning the ease or difficulty of obtaining selfed seed from caged plants of the various cultivated types of composites at the Floradale nursery of Burpee in California.

Compositeae. Little is known about the relationships in the Vernonieae; but since two undetermined species of the large genus Vernonia proved to be self-sterile when tested, and since all of the tribe are homogamous, it is probable that a considerable amount of self-incompatibility exists.

The same statement may be made about the Eupatorieae. The cultivated varieties of Ageratum are mostly self-fertile (Burpee), although some strains tend to be self-sterile. I have tested only one undetermined species from the large genus

Eupatorium. It was self-sterile. One can draw no conclusion from such limited experience; but it may be suggested that, since Eupatorium is such a difficult genus, taxonomically, and has been found to contain apogamous species, perhaps a great deal of the reproduction is asexual, and self-sterility may be common in the remainder. How far it extends throughout the subtribes, is wholly unknown.

One might expect that much self-fertility should be found in the tribe Astereae, on the grounds that heterogamy is often well developed; and in the subtribe Baccharidinae, diœcism is prevalent. This may be so, but in the few genera on which there are experimental data, self-sterility appears to be practically complete. Dr. R. H. Wetmore has tested six species of Solidago, and has not obtained a single viable seed. Three species—*S. sempervirens* L., *S. rugosa* Mill., and *S. asperula* Desf.—were selfed repeatedly by hand. *S. canadensis* L., *S. Drummondii* T. & G., and *S. racemosa* Greene were first bagged; then, at the time of anthesis, the bags were removed for a minute, while the heads were rubbed together. Complete self-sterility was also found for twelve species of Aster tested. *A. multiflorus* Ait., *A. novae-angliae* L., and *A. amethystinus* Nutt. were repeatedly selfed by hand. *A. ericoides* L., *A. linarifolius* L., *A. Shortii* Lindl., *A. turbinellus* Lindl., *A. laevis* L., *A. patens* Ait., *A. novi-belgii* L., *A. puniceus* L., and *A. tataricus* L. f. were bagged and treated as above.

Information has been received, largely from Burpee, that the cultivated types of Aster, Felicia, Heteropappus, and Venidium, while occasionally setting a few seeds when individual plants are caged, may be considered to be generally self-sterile.

There is no safe information at hand about the large and varied tribe Inuleae. I have heard that some of the cultivated forms belonging to the genera Helichrysum and Inula are self-fertile, but no satisfactory evidence is available. From the amount of heterogamy known, and the varied different types of sexual specialization developed, one might supposed that self-sterility is less likely to have been developed here than in any other tribe. None has been reported; but it is not safe to generalize.

Were it safe to conclude that self-fertility is prevalent in the Inuleae in advance of testing, the same conclusion would have to

be drawn for the Heliantheae, since here, also, there is highly developed sexual specialization with monœcism (Ambrosinae) and diœcism (Petrobinae). Yet there is a greater proportion of self-sterile species here than in any other tribe—though it is true that the only well-sampled subtribe is the Madinae, which is rather well known, thanks to the industry of Dr. Jens Clausen.

In the other subtribes, information that there is variable, but on the whole a highly developed self-sterility reaction, has been obtained for the cultivated Zinnias (Burpee), *Rudbeckia hirta* L., *Helianthus annuus* L., *H. tuberosus* L. (some varieties apparently self-fertile), two undetermined species of wild Helianthus, most cultivated types of Coreopsis (Burpee), some cultivated lines of Dahlia with good pollen, and most cultivated forms of Cosmos.

The only extensive sampling, however, is in the Madinae. Dr. J. Clausen permits me to quote from a letter dated November 24, 1937, that among the approximately 85 species in this subtribe, only 14 are self-fertile. These are: *Achyrachaena mollis* Schauer, *Lagophylla ramosissima* Nutt., *Layia hieracioides* (DC.) Hook. & Arn., *L. carnosa* (Nutt.) T. & G., *Madia yosemitana* Parry, *M. citriodora* Greene, *M. dissitiflora* T. & G., *M. sativa* Molina, *M. capitata* Nutt., *M. chilensis* (Nutt.) Reiche, *M. glomerata* Hook., *M. exigua* (Smith) Gray, *M. madioides* (Nutt.) Greene, and *Hemizonella minima* Gray.

All species of Holozonia, Hemizonia, Holocarpha, and Calycadenia are self-sterile. In addition, there are 3 self-sterile species in Lagophylla, 11 in Layia, and 8 in Madia. Dr. Clausen also writes that all the self-sterile species have large and showy rays, while all the self-fertile species have short and inconspicuous rays.

There is no satisfactory information available on the Helenieae and Anthemideae. This is unfortunate. It would be especially interesting to know the situation in tribes where a considerable number of shrubby forms have developed. It would seem as though there should be data available on Chrysanthemum and such other genera as have had popularity among garden lovers, but the florists have merely selected seeds from unbagged plants. The large-rayed florist chrysanthemums, developed from *C. indicum* L. and *C. sinense* Sabine, are propagated asexually, of course.

The tribe Senecioneae has hardly been tested at all. Florists report that the cultivated types of Senecio are almost completely self-sterile; but they are all included in half a dozen species, and the genus contains over 1200.

No self-sterility has been reported in the remaining tribes of the Tubiflorae, viz., Calenduleae, Arctotideae, Cynareae, and Mutisieae; but then, the only records are those of seedsmen, who maintain that the cultivated species and varieties of Dimorphotheca, Calendula, Arctotis, Gazania, Echinops, and Centaurea are all self-fertile. No one could say, therefore, that self-sterility may not be found later, since both the sampling and the type of data available are unsatisfactory. It may be added, perhaps, that I have a few records on Carduus and Cirsium which show self-fertility. Even the ordinarily gynodiœcious *Cirsium arvense* (L.) Scop. is self-fertile when functional ovules are found on the so-called male plants.

Liguliflorae. In the Cichorinae, the available data appear to include only the work of Stout, already mentioned, on the self-sterile species *Cichorium Intybus* L., and some intimations by vegetable breeders that *C. Endiva* L. is sometimes weakly self-sterile.

No data have been discovered on the Leontodontinae. On the other hand, the Crepidinae are pretty well known, thanks to the extensive investigations of Babcock and his co-workers. The data reported have come wholly from a letter received from Babcock, under the date November 19, 1937.

Babcock writes that *Launea asplenifolia* Hook. is self-sterile. Four species of Lactuca tested are self-fertile, and this agrees with the results of other observers. Of five species of Sonchus tested, only *S. tenerrimus* L. is self-sterile. Two Troximons and one Youngia tested are self-fertile. Of two diploid sexual Taraxacums tested, *T. duplidens* H. Lindberg is self-sterile, and *T. serotinum* Poir. is self-fertile. In all, 60 species of Crepis were tested under bags. Of these species, 26 set no seed, and 26 set some seed. The remaining 8 species are known to have both self-fertile and self-sterile subspecies or strains. As Babcock says, one can only call the species that set some seed self-fertile; but it must be noted that until more intensive work is done, the possibility remains that all these forms may show the incompatibility reaction in some degree.

Summary

1. Although our understanding of the physiological and genetical bases for self-sterility has been greatly advanced by modern research, knowledge of its distribution has made little progress since the time of Darwin.

2. The presence or absence of self-sterility has been investigated in more than 800 species of flowering plants. The results are summarized in taxonomic sequence, together with such information as is available in the scattered literature on the subject. The incidence of self-sterility in the various orders of flowering plants is summarized in Table 1.

3. The survey of the incidence of self-sterility in the flowering plants is necessarily incomplete and fragmentary. It does show, however, that self-sterility is widespread among the various orders of plants, although the distribution is not at random. Self-sterility is much more frequent in herbaceous than in woody plants. Of the eighteen orders which consist primarily of woody plants self-sterility has been definitely established in only two. Among the twenty-five orders which consist entirely or primarily of herbaceous plants, self-sterility has been found in fourteen. The correlation between the incidence of self-sterility and anatomy may be due, in part, to the fact that the plants of agricultural value are predominantly herbaceous types, and consequently have been studied more thoroughly. A comparison between the size of the order and the number of species examined in the various orders indicates that some correlation exists, but that the survey represents a fair cross section of the more important orders, even though the economic genera have been studied more adequately.

The greater incidence of self-sterility in the herbaceous plants is to be expected if the herbaceous types have been derived from the woody forms, as the geological and anatomical evidence indicates (Sinnott and Bailey, 1914). Self-sterility must be a relatively recent innovation in the reproductive process in view of its biological and genetic complexity (East, 1929). It is obvious that self-sterility, by insuring cross-pollination, is an important factor in producing variability. At the same time it serves as a mechanism for perpetuating heterozygosity, both genetic and structural, in loci adjacent to the self-sterility alleles. This mechanism is particularly effective in species with localiza-

TABLE 1

Tabular Review of the Distribution of Self-sterility in the Flowering Plants. Summarized by Orders, in Taxonomic Sequence

Class	Order	Self-incompatibility Definitely Established	Heterostyly	Cleistogamy
Monocotyledoneae	Pandanales			
	Helobiae			
	Glumiflorae	x		x
	Principes			
	Synanthae			
	Spathiflorae			x
	Farinosae	x	x	x
	Liliiflorae	x		x
	Scitamineae	x		
	Microspermae	x		x
Dicotyledoneae	Verticillatae			
	Piperales			
	Salicales			
	Myricales			
	Juglandales			
	Fagales			
	Urticales			
	Proteales			
	Santalales			
	Aristolochiales			
	Polygonales		x	x
	Centrospermae	x		x
	Ranales	x		x
	Rhœadales	x		x
	Sarraceniales			
	Rosales	x		
	Geraniales	x	x	x
	Sapindales			
	Rhamnales			
	Malvales	x		
	Parietales	x	x	x
	Opuntiales			
	Myrtiflorae	x	x	x
	Umbelliflorae			
	Ericales			
	Primulales	x		x
	Plumbaginales	x		
	Ebenales			
	Contortae	x	x	x
	Tubiflorae	x	x	x
	Plantaginales			x
	Rubiales	x	x	
	Cucurbitales			
	Campanulatae	x		x

tion of chiasmata, as is found in Tradescantia. The evidence indicates that self-sterility is an important factor in producing variation and may serve to perpetuate heterozygosity. Both of these factors are of evolutionary value, and apparently self-sterility has played an important part in the evolutionary development of the flowering plants.

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CENTENARY CELEBRATION

THE WILKES EXPLORING EXPEDITION OF THE UNITED STATES NAVY

1838–1842

AND SYMPOSIUM ON

AMERICAN POLAR EXPLORATION

FEBRUARY 23–24, 1940

CONNECTION OF THE AMERICAN PHILOSOPHICAL SOCIETY WITH OUR FIRST NATIONAL EXPLORING EXPEDITION

EDWIN G. CONKLIN

Executive Vice-president of the American Philosophical Society

(Read February 23, 1940, in Centenary Celebration of the Wilkes Exploring Expedition, 1838–1842)

In the last year of the administration of Andrew Jackson the Congress of the United States on May 18, 1836, authorized an Expedition to the great Southern Ocean, in view of our interest in the whale fisheries and to make surveys in aid of navigation. Thus its primary purpose was to aid commerce and navigation, but an important secondary aim was, in the words of the Act, "to extend the bounds of science and to promote knowledge."

With these objectives in view it was natural that the promoters of this Expedition should have turned for suggestions to various organizations which had as their objectives these same aims. Among organizations so consulted were the East India Marine Society of Salem, Mass., and the Naval Lyceum of New York, whose counsel was sought on matters of navigation and commerce; the American Philosophical Society and the Academy of Natural Sciences of Philadelphia were asked to outline the scientific program and to recommend a scientific corps. The first named of these societies has no account in its records of its response to this request; the second, namely the Naval Lyceum of New York, responded in a document which has been loaned for this occasion by its owner, Mrs. Minta L. Hull, a descendant of Admiral Wilkes; the long and carefully prepared report of the American Philosophical Society which went to the Secretary of the Navy has also been loaned by Mrs. Hull. The more extensive records of the American Philosophical Society which were used in preparing this report are in the Archives of this Society.

The Secretary of the Navy in 1836 was the Honorable Mahlon Dickerson, for thirty years previously a member of the American Philosophical Society, during three of which years (1810–

12) he was a member of its Council. Mr. Dickerson wrote to the venerable President of this Society, Peter S. DuPonceau, the following letter:

NAVY DEPARTMENT.
31st. August 1836.

Sir,

As preparations are making to fit out an exploring and surveying expedition for the Southern Ocean, to leave the United States as soon as conveniently may be, I take the liberty, through you, of asking the advice of the Society over which you preside, as to the formation of a scientific corps for this expedition, and their recommendations of scientific gentlemen, of suitable age, to be employed as members of this corps, who may be well acquainted with Geology and Mineralogy,—with Botany, with Zoology in all its numerous branches, with meteorology, magnetism, electricity and other subjects connected with natural history.— Also their recommendation of a Phylologist [*sic*] to collect catalogues of words of the different languages,—and an artist to take the portraits of the natives—of the different countries and islands which may be visited.—

I would also respectfully ask of your Society, a series of inquiries as to subjects of natural history—Meteorology, Magnetism etc., to which, in their opinion, the researches of this corps should be particularly directed,—together with such suggestions as they may believe calculated to promote the objects of the expedition.—

I am with great respect,
Your obdt. servt.
(Signed) MAHLON DICKERSON.

The President of the
American Philosophical Society.

The American Philosophical Society considered this request at a meeting on September 7, 1836, and appointed a Committee consisting of the following members with their dates of membership in the Society indicated:

Peter S. DuPonceau (1791–1844), Robert M. Patterson (1809–54), Nathaniel Chapman (1807–53), James P. Espy (1835–60), Titian R. Peale (1833–85), Charles Pickering (1828–37), Henry D. Rogers (1835–66).

This Committee drew up a reply in the form of an extensive document of more than thirty closely written pages. In this report eight different fields of scientific observation and research were outlined by the Committee. President DuPonceau indicated what should be done, or at least attempted, in Philology and Ethnography; Dr. Patterson dealt with the sciences of Astronomy and Physics; James P. Espy outlined at length the work

which should be undertaken in Meteorology; Titian R. Peale indicated briefly some of the important aims and methods in Zoology; and a similar report was made for Botany by Dr. Charles Pickering; Professor Henry S. Rogers presented some of the problems in Geology which should be attacked; and Dr. Nathaniel Chapman outlined some interesting inquiries which should be made concerning the prevalence of various diseases and the methods of treatment among the natives of the islands and countries to be visited. These different reports were assembled, condensed and, together with certain general recommendations, sent to the Secretary of the Navy in October 1836, in a document, the original copy of which is owned by Mrs. Minta L. Hull, of Washington, D. C. The rough draft of this letter is in the Archives of the American Philosophical Society and reads as follows:

[Hon^ble^. Mahlon Dickerson
Secretary of the Navy]

HALL OF THE AMERICAN
PHILOSOPHICAL SOCIETY.
Philadelphia, Oct. 17 1836.

Sir,

Your letter of the 31st of August, requesting the advice of the American Philosophical Society as to the formation of a Scientific Corps for an exploring and surveying expedition about to be sent by Government to the Southern Ocean,—inviting their recommendation of Scientific gentlemen as members of this Corps,—and asking of them a series of inquiries as to the subjects to which the researches of the Corps should be particularly directed,—was laid before the Society on the 7th of September, and the undersigned were appointed a Committee with instructions to take order thereon, except as to the recommendation of members of the Corps, with regard to which it was the wish of the Society not to interfere.

The first and most important point referred to us for our advice regards the constitution of the Scientific Corps. As we are not made acquainted with the scale on which the expedition is to be fitted out, we cannot give any recommendation as to the number of persons that should be employed in the several departments, but we would take the liberty of suggesting, that if there be more than one principal vessel, each of them should be furnished, if possible, with a complete Corps,—and we would also call your attention to the importance of so multiplying the members as to provide for the losses which may be looked for from various causes. In the celebrated expedition to the South Seas, sent by the French Government, at the commencement of the present century, and of which Péron has given the account, sixteen out of the twenty-four men of science and artists that originally embarked from France were prevented by sickness or death from complet-

ing the voyage. With the provisions thus recommended, it appears to us that a Scientific Corps should be composed of the following members.

1st. An Astronomer, and his Assistants. To these should be committed, especially, the astronomical observations to be made on land for determining all important positions; the experiments on the length of the pendulum: the magnetic and electrical experiments: and all those physical observations not immediately belonging to meteorology.

2d. A Surveyor, and his Assistants,—to whom should be committed the survey of coasts and harbors, and the construction of the charts.

The Assistants of these two departments might, with great advantage, be selected from the younger Officers of the Navy.

3d. A Meteorologist.

4th. A Zoologist, with Adjuncts and Assistants.

5th. A Botanist, with a practical Gardner as an Assistant.

6th. A Geologist, and Assistant.

7th. Painters of Natural History: including animals and plants.

8th. A Landscape Painter.

9th. A Portrait Painter.

10th. A Philologist, who should attend to the manners and customs as well as the language of the natives.

11th. The Historiographer of the expedition; who is to write the general narrative, and to have access, for this purpose to the journals, charts, drawings, &c., of the other members of the Corps.

In order to comply with your request that a series of inquiries should be presented as to the subjects to which it is thought proper that the researches of the Corps should be particularly directed, we have asked the individual members of the Committee to give their views in the several departments to which they have devoted their attention, and we now communicate these views, as nearly as possible in the form in which we have received them.

Astronomy and Physics. [By Robert M. Patterson].
(Here copy from Dr. Patterson's papers, pp. 1–7.)

Astronomy.

The practice of nautical astronomy is now brought to such perfection, and is so well understood by the scientific officers of our navy, that we offer no suggestions on this subject.

For observations on land, the expedition should be furnished with good telescopes to observe occultations, and the eclipses of Jupiter's Satellites; and they should also have a portable transit-instrument; and a vertical circle for the latitude. The two might be combined.

It has been asserted, that, with suitable address, the eclipses of Jupiter's Satellites may be observed at sea. It will be interesting to have this fairly tested during the proposed expedition.

Observations to determine the length of the pendulum vibrating seconds should be made on every suitable occasion; particularly in the

highest southern latitudes reached. The methods to be pursued are well known, and they should, if possible, be practised by the Officers specially charged with the astronomical department, before setting out.

Atmospheric Phenomena.

Halos: to be always noted and described. The horizontal and vertical distances from the central body, (Sun or Moon) to be measured by the sextant, and recorded.

Rain-bows: The supplementary arcs of green and purple to be carefully observed; their number and breadth noted; and how far they may be seen toward the ends of the bow.

Looming: All the phenomena to be noted, and figures given. Temperature of the air and water at the time, hygrometric state of the atmosphere, and other accompanying circumstances that may have a connexion with these appearances to be always recorded.

Zodiacal Light: This phenomenon to be observed and described with care; giving the breadth at the horizon, the general form, the place of the apex, &c.

Falling Stars: Some officer, on watch, to be instructed to note, as far as practicable, all these appearances, stating, by a reference to the stars, the point of their first and last appearance, their general aspect, whether they leave trains behind them, &c. The place of the ship, the date, and the time of night, to be of course always recorded. It is desirable that particular attention should be paid to these appearances about the middle of November.

The Ocean.

Currents: Careful observations on these are of great interest and importance; they will be made by comparing as frequently as possible, the change of place of the ship as estimated by Astronomical observation and by dead reckoning.

Tides: It is hoped that these will be observed with great care, whenever the opportunity is presented. The time and amount of the greatest and least elevation above any fixed level should be noted, with a statement, at the same time of the direction and strength of the wind.—The tides in the Pacific, it has been asserted, obey the influence of the Sun, rather than of the Moon. If this be so, it is very desirable to know whether this influence may not be exerted through the medium of the land and sea breezes.

Depth of the Sea: It is hoped that opportunities of solving this interesting problem will not be neglected; and we would suggest that copper wire might be more suitable for the purpose than hempen cord. It is strong, it is not buoyant in water, and it is so thin that a vast length of it may be wound upon reels without occupying much space.

Temperature of the Sea at great depths: This may be determined either by sinking a self registering thermometer, or by drawing up a sufficiently large quantity of water from the depth, as practised by Dr. Jno. Davy. The instruments for such experiments will be found described in several of the works of which a list is subjoined.

Salt and Air in Sea Water: Biot has described a method of determining the quantity of salt, and the amount and kind of air contained in sea water, which we would recommend to be used, in this expedition, at different depths, extending to the greatest that can be reached.

Sound: When the air is extremely cold, it is stated that sounds may be heard at a much greater distance than when it is warmer; but no means of exact comparison have yet been used. It is desirable, then, that experiments be made, under these different circumstances, in calm weather, as to the distances at which the same persons hear the ringing of the same bell, or the explosions of percussion caps of the same kind fired in the same manner.—It is also of great interest to determine the velocity of sound through air of extreme coldness.

Magnetism.

Variation of the Needle: This should be observed as frequently as possible. The methods are familiar.

Local Attraction of the Vessels: It is indispensable that this should be determined before the expedition sails. The methods are fully detailed in Barlow's work on Magnetic attraction. Barlow's compensating plates should be taken with the vessels; and the method of determining the local attraction, by comparing the compass on deck with a compass at the masthead should also be employed.

Daily Variations: should be observed when on land; and the effect of auroras upon the needle noted.

Dip of the Needle: It is a matter of great scientific interest that this should be carefully observed, whenever an opportunity is presented. To be satisfactory the observations should be made with the utmost care, and with instruments of the best construction.

Magnetic Intensity: Observations for determining the magnetic force of the earth should be made, at every opportunity, by the vibrations of the dipping-needle, and by the horizontal vibrations of magnets. The needles and magnets prepared for this purpose should be tin'd frequently before the departure of the expedition. As some of the islands have high mountains, it will be interesting to try the vibrations of the needles at the bases and tops of these mountains.

South Magnetic Pole or Poles: This is an object of such deep interest that the research will not fail to be prosecuted with every exertion.

Auroras.

Observations on the *Aurora Australis* will be looked for with great interest. Every appearance of this kind should therefore be noted and carefully described,—the observations to include the position of the central part at the horizon;—the height and amplitude of the arches when formed;—the position of the vanishing point of the streamers; and, when practicable, the effect on the magnetic needle.

METEOROLOGY. [BY JAMES P. ESPY].

Formation of Clouds.

There is a certain kind of cloud which forms only in the day, when the heavens are not overspread with other clouds, and when the dew point is not too low, which when well formed generally appears with a broad dark base and narrow top something in the form of a cone with sides as white as snow. There is no cloud ever seen below the base of this cloud, but it frequently rises with its top above the highest clouds which it pierces with its snow white top. As it passes through this uppermost or feathery cloud it seems to lift the thin cloud before it and condense it into a semitransparent vail [*sic*], which at first appears at some distance above the top of the columnar cloud, but finally as the columnar cloud moves upwards its well defined top reaches the thin veil and penetrates it. Very soon afterwards by the upward motion of the columnar cloud, the veil coalesces with the cloud and can no longer be distinguished from it. The same phenomenon frequently takes place when no feathery clouds are to be seen in the higher strata of air. When the top of the columnar cloud reaches a great elevation it is seen to form above it at a short distance a similar veil or cap which it gradually overtakes and coalesces with, as before mentioned. The bases of these clouds are probably all on the same horizontal level: and if the theory which has been lately advanced in the Journal of the Franklin Institute is correct, the height of these bases is as many hundred yards as the temperature of the air is above the dew point at the moment of observation in degrees of Fahr.

We invite the special attention of the meteorologist to be directed to this most interesting cloud. Let him watch it from the moment of its beginning to form in the morning, taking drawings of it through all its stages, noting the length of time from one stage to another, until it dissipates or produces rain. If it dissipates without raining let him try to ascertain the cause. Did its top rise into a current of air moving in a different direction and preventing it from rising perpendicularly, slicing off its top and dissipating it in air not saturated with vapour? Or did it spread out in all directions and thus dissipate? Or did its failure depend on the dew point? Or what were the circumstances in which it differed from columnar clouds producing rain? In case of producing rain let the top of the cloud be particularly noted: does it change its appearance about the time or a little before the rain is seen to descend from its base? And in case the cloud becomes very lofty does the base of the cloud sometimes descend to a lower level and appear convex below? And is an extension of this appearance the Water Spout? Does the cloud also swell out sometimes above so as to form a shape something like an hour glass, or double cone with the apices together? What kind of cloud does this columnar or hour glass rain-cloud form after the rain is over and how does it differ from the cloud which dissipates without raining? Does it become the feathery cloud sinking a little

at the top and rising at the base and spreading out in the direction of the upper current? What is the direction of that upper current at the Equator? Is it towards the west, if so, do storms travel in that direction? And with what velocity? Near the Equator on the north side do the storms recede a little from the Equator as they travel westwardly and so on the south side of the Equator? Or is there any general law on this point? During the rapid formation of a columnar cloud is the wind affected? If the Theory alluded to above is correct it should blow in all directions towards the forming cloud, and upwards in the region of the cloud itself. Is there such a thing as a white squall without any cloud formed or about to be formed over the region of the squall?

In case of a great storm or hurricane does the wind near the Equator always set in from some western point, and do tornados [*sic*] always travel in the direction of the stratum of air which the tops of columnar clouds penetrate? Or is there any law on this point? Do columnar clouds more frequently form over islands than in the open sea? And do they only form in the day when the heavens are not overcast with other clouds and do they disappear in the night as they do on land? Are columnar clouds formed every day not overcast; if not what is the cause? Is it because the dew point is too low when compared with the temperature of the air? What is the greatest depression of the dew point below the temperature of the air when columnar clouds form? —How soon do they begin to form after sun-rise and when do they cease forming in the afternoon? And when do they disappear entirely in the evening? Or is there any law on this point?

Mr. Redfield of New York has shown that the storms which visit the West Indies travel northwestwardly while in the torrid zone. Does the wind in these storms which sets in from the n.w. change round on the n.e. side by the north and on the s.w. side by the west in such a manner as to show that the wind blows towards the centre of the storm? Or what is the general law on this point? Are these storms always attended with electrical phenomena, and is there anything peculiar in the appearance of the lightning? When the observations are made on land, note, whether the lightning descends vertically, as has been asserted, and rolls over the ground like melted metal. Inquire, when these tornadoes, occur whether they sometimes lift up large trees and set them down in a different place on the broad base of their roots without overturning them, whether they lift off the roofs of houses and prostrate the walls outwards as if by explosion, and tear up the floors of others and leave the walls standing? Are they ever attended with hail? Do these storms remain for sometime stationary on reaching an island, and what evidence is there of a lull or calm in the centre of the storm? Has the Barometer ever been observed at the moment of this lull, and what is the greatest depression recorded by a credible witness Does the rain cease at the moment of the lull of the wind, and are the clouds seen at the same time to move on all sides towards the zenith

In case of violent storms in the torid [*sic*] zone, do they always set in from some western point and terminate from some eastern point? If they set in from a point far north of west does the wind veer round by the north, and if from a point far south of west, does the wind veer round by the south? If this question should be answered in the affirmative, the importance of the discovery will be of immense advantage to navigation, for it will afford an indubitable proof that the wind blows towards the storm; and the knowledge of this fact will enable the Mariner to avoid the storm by sailing in a direction from the point to which the storm is advancing.

Suppose for example that it has been discovered that near the equator the storms travel towards the west and that the wind in great storms blows towards the centre of the storm, and a violent gale sets in from the n.w. it is manifest, if the Mariner sails towards the n.e. he will soon be out of reach of the storm, whilst on the contrary if he should direct his course southwardly he would penetrate the very heart of the storm, and thus be exposed to all its violence. It is then a matter of high importance to ascertain the course which storms travel in all the different latitudes. If the uppermost current of the atmosphere gives direction to violent storms, it is highly probable that near the equator they travel towards the west. For as the air at the equator is lighter than air at high latitudes, both on account of greater heat and greater moisture, it will ascend, and in ascending it will recede to the west in consequence of the earth's rotation.

This upper current may probably be detected by the direction in which the lofty columnar clouds lean, for their tops when they rise to a great height will be bent over in the direction of the upper current. Besides as the feathery or hazy cloud spoken of above is probably formed out of the tops of columnar clouds which have rained, this upper current will most likely be indicated by the hazy clouds. Let these clouds be carefully noticed and described. Is their velocity uniform or various? Does their acceleration indicate rain? Does their increase in number indicate rain? Is their direction uniform in the same latitude? Is this direction towards the west, near the equator,—n.w. near the Tropic of Cancer—s.w. near the Tropic of Capricorn—nearly towards the n. about 30° n. latitude and nearly towards the s. about 30° s. latitude? then gradually turning towards the n.e. in higher n. latitudes, and towards the s.e. in higher s. latitudes? For these are the directions the uppermost current must take as it flows down the inclined plane of the upper part of the atmosphere from the equator towards the poles. Or is there any general law on this point. The observer will be careful to distinguish between violent storms and ordinary rains; For it may be that ordinary rains are very irregular in the direction of their motion, the tops of the clouds producing them not reaching into the uppermost current, which is probably nearly uniform in its direction, while the tops of the clouds producing tornados [*sic*] may all reach into the uppermost current and thus great uniformity in the direction of their motion may be produced.

Mr. Redfield has shown that there is a remarkable uniformity in the progressive motion of storms or hurricanes which traverse the West India islands, all moving in the direction which theory would seem to give to the uppermost current as it passes off from the equator towards the pole. The greater heat and higher dew-point of the inter-tropical air will cause it to be about one sixth lighter than the air in the frigid zones, and, of course, it will stand proportionably higher, and will therefore roll off towards the poles, carrying with it, in some measure, the diurnal velocity which it had at the equator, and so moving faster east than the earth, at the latitude which it has reached. In confirmation of this theoretical result it is known that the highest of all our clouds in the latitude of Philadelphia come constantly from near the west or s. west and all the tornados [*sic*] which have been observed by scientific men travel from a little s. of west in that latitude. It may be added that of eleven tornados [*sic*], or land-spouts which passed through New York, Pennsylvania and New Jersey, every one had the trees thrown down with their tops inwards and forwards, not one tree being ever discovered with its top lying out at the side. Thus proving beyond a doubt, that in land-spouts the wind blows towards the centre of the spout. How is it at sea? In several great storms in the United States, of several hundred miles in diameter, which have been investigated with great care by the Joint Committee of the American Philosophical Society and the Franklin Institute of the State of Pennsylvania, the wind has been discovered to blow for many days in succession towards the storm on all sides round the storm. Is this the case at sea? It is known however that rains of moderate size produced by columnar clouds when the base is not too low to permit it, cause by their cooling effect on the air below the base of the cloud, and also by their weight, the wind to blow outwards at the surface of the earth on all sides from the rain. It is true that in many cases of this kind the lower parts of the cloud have been seen at the same time moving in from the circumference towards the centre, which proves that in this case too, the air at some distance above the surface of the earth, blows towards the cloud.

It is highly desirable to investigate all the causes producing this variety of effect, so that it may be predicted by the appearance of a cloud whether the wind will blow from the rain or to it at the surface of the sea. Do columnar clouds at sea generally disappear at night as early as they do on land? And what time of day do they form most rapidly? If, as is probable, opportunities be presented of observing volcanoes, let it be seen whether they ever produce violent rains immediately after they break out, first in the neighborhood of the volcano, and afterwards extending to greater distances. Are these rains ever attended with violent tornados [*sic*], and sometimes with hail? Are the smoke and ashes carried in all directions above to a great distance or are they carried farther on one side than on another?

The fluctuations of the Barometer in connection with storms will of course, be noted. Much knowledge is yet wanted to be able to read the

indications of this valuable instrument. Observe whether it ever rises before the depression which always takes place in great storms. And at the moment of the calm, which may probably be experienced in the middle of the storms, mark the greatest depression.

The four diurnal fluctuations of the Barometer should also claim particular notice. Very laborious and highly interesting observations on these fluctuations have lately been made in India which are recommended to the attention of the Meteorologist.

If these fluctuations depend on the increase and diminution of elasticity of the air by heat and cold, according to a theory published in the Journal of the Franklin Institute, the morning maximum at a considerable elevation ought to be greater than on the plain below, and the afternoon minimum should not be so low as on the plain below—Again the night maximum should be less and the minimum greater than on the plain. These predictions have been verified by observations. If time and opportunity should be found they might be repeated. The theory however goes further; it indicates that at very great elevations there are but two fluctuations in a day. A maximum at about 12 or 1 in the day, and a minimum about day break. To verify or refute this inference from theory, simultaneous observations at an elevation of 14 or 15 thousand feet and on the plain below continued for a few days will be sufficient, if the fluctuations on the plain are regular.

If a cloud is produced by the cold generated by the expansion of the air as it ascends, it is manifest that the base of the cloud will be low in proportion as the dew-point approaches near to the temperature of the air, and high in proportion as the dew-point recedes from that temperature. Let the dew-point therefore always be carefully taken on the approach of a storm, and the character of the cloud observed with reference to the dew-point. The height of the base from the surface of the ocean—the rapidity of the formation—the final perpendicular height of the cloud above the base—and if it forms the hour-glass shape let the relative size of the two parts be noted, and the height of the narrow part of the cloud. Does this hour-glass cloud only form when the dew-point is high? and is the rain ever seen descending from the upper inverted cone into the clear air? If so, has this fact any connection with hail as the air on the outside of the cloud is much colder than the air on the inside in consequence of the latent caloric given out in the condensation of the vapour in the formation of the cloud; or does hail depend entirely on the great height to which the drops of rain are carried up by the upward motion of the air in the cloud itself? In calm days when a sudden breeze springs up over a very limited space which is seen by the ruffling of the water; observe whether the wind blows towards that space by aspiration or from it by impulse.

The Franklin Kite Club at Philadelphia have lately discovered that in those days when columnar clouds form rapidly and numerously their kite was frequently carried upwards nearly perpendicularly by columns of ascending air, and they say in their report that this circumstance became so familiar during the course of their experiments, that on the

approach of a columnar cloud just forming they could predict whether it would come near enough to affect their kite; for if the cloud did not pass directly over the kite, the kite would only move sideways towards the cloud. Now these upward columns were probably formed of air heated from contact with the ground—Is the same effect produced at sea? Is there any connection between these sudden and very limited breezes at sea and the formation of columnar clouds?

If thunder storms occur at night, endeavour should be made to find out whether they originate at night, or whether they are continuations of storms originating in the day. If they form at night note whether the columns ascend from the stratum of middle clouds, or whether they rise from below them—observe where the electricity first appears in them, and at what stage of their advancement, and the whole phenomena as contrasted with storms or rains in the day.

ZOOLOGY. [BY TITIAN R. PEALE].

The Zoologists should observe, draw and describe the various animals inhabiting the countries which may be visited by the Expedition. The assistants should be qualified to collect, draw, or prepare specimens for preservation. The selection of such assistants should be made by the Principals with the approbation of the Secretary of the Navy.

The Zoologists should be instructed to collect information of the habits, localities, times of gestation, food &c. of all the large mammiferous animals, such as Seals and Cetacea, that inhabit the southern oceans, and which constitute the great source of commerce in those seas; to extend their observations to the various branches of Ornithology, Entomology, Conchology &c., to make themselves particularly acquainted with the times, and places where the numerous Sea Fowl of those regions resort to breed, the eggs and young of which are known to add largely to the health and comfort of the seamen engaged in the above commerce; to observe the various Turtles, and Molluscae with the same views; the Pearl fisheries; to dredge in deep as well as shallow water for the numerous inhabitants of the ocean, and to ascertain as nearly as possible, the different depths at which those animals exist; the depths from which the various species of Zoophytes erect their fabrics and form Islands, many of which in after-times become the residence of Man; to ascertain the time requisite for the maturity of such; their food; and in fact to collect all the information which can be reasonably obtained of that race of animals, which though among the smallest, hold notwithstanding one of the most important places in the chain of created beings.

The Insects of the various Islands should be collected and preserved, and as far as possible their metamorphoses should be observed and recorded. Drawings should be made on the spot, particularly of those animals, of which prepared specimens cannot be brought home.

In observing the Fish, it will be important that the Zoologists should particularly remark, when, where and how they are taken, and whether

they will be likely to be worthy of consideration in a commercial point of view, like the Cod, Mackarel [*sic*], Herring &c.

To accomplish all the above views, it will be requisite for the persons employed to collect and prepare specimens, as far as practicable, of all the animals noticed, both as vouchers to the accuracy of the observations made, and to correct errors which might be committed in the hurry of a varied occupation:—It will be imperatively necessary that the Zoologists be liberally provided with appropriate Nets, Dredges, Boxes, Casks, Spirits, and all the various instruments and materials used for procuring and preserving specimens; They should also be provided with Books of reference, which they will require in order to be constantly aware of the labours of their predecessors in the same field.

BOTANY. [BY CHARLES PICKERING].

The Science of Botany is now reduced to such a state, that a competent individual will well understand the duties that devolve upon him; and it may be added that a competent individual will think it a privilege to perform them. There are however certain points that fall less within the regular routine, and may be recommended to his attention.—It will require wide extended knowledge, not to be overwhelmed with the variety of productions that may be met with—perhaps indeed it is altogether unavoidable.

The different localities should be carefully noted, also the kind of soil; and the utmost attention should be paid to any useful properties, medical or otherwise, that can be ascertained. There are many important questions respecting the plants which produce certain drugs, some of which it may be in the power of the Expedition to solve.

An experienced practical Gardner is indispensable to the Botanical Department, as well for the collection & preservation of roots & seeds, as to pronounce upon such plants as may be adapted to our climate.—France may well be proud of the fact that all the coffee cultivated on the American Continent owes its origin to a plant from the National Garden at Paris; and that the Bread-fruit was introduced from the same quarter, while the well known Expedition of Capt. Bligh failed in its object.

A Botanical Draughtsman could also render essential service in delineating such objects as must necessarily be attended to on the spot.

GEOLOGY AND MINERALOGY. [BY HENRY D. ROGERS].

It will be the business of the Geologist to investigate the geological structure and the mineral resources of the regions visited by the Expedition. He should be allowed to select an assistant qualified to collect, arrange and analyse specimens, and to execute the drawings necessary to the proper illustration of the phenomena and objects observed; the selection of the assistant being subject to the approbation of the Secretary of the Navy. It is suggested that the Geologists be instructed to

pursue their inquiries in as systematic a manner as practicable, by traversing the districts they may visit in such directions as to furnish the data when time permits for presenting a Geological map of the more interesting parts: that with this view they explore the coasts wherever there are natural exposures of the rocks and ascertaining from observations there made the bearings or directions of the strata that they then proceed along one or more lines as nearly at right angles to the strata as practicable, taking note as they advance of the dip, order of superposition, and relative width of the several formations crossed, also marking the distances and elevations of the localities inspected so as to construct from the whole a geological section of the belt explored. They are recommended to select two characteristic specimens of each description of rock *in situ,* numbering them in progressive order from the point of departure, and taking care to note upon the geological map or the sections, the position of each of the specimens by inserting the same number which is affixed to the specimen. Besides thus numbering the specimens, they should each have a label signifying the spot from whence it has been taken.

Traversing a country in directions transverse to the bearing of the strata will be found not only to furnish in the briefest and easiest manner practicable a knowledge of all the rocks of the region in their true relations of extent and position, but will exhibit in succession all the characteristic features of its physical geography, the greatest diversity of soils and localities, of minerals, plants and animals.

The advantages of embracing a greater variety of objects than are likely to present themselves by following any other directions to the same distance, is an inducement for the Zoologists and Botanists to accompany the Geologists in their excursions conducted upon this plan, which furnishes another recommendation in its favour, as it will obviously be desirable for the Scientific Corps to travel as much in company as possible, for the sake of additional safety and convenience.

In forming the geological sections care should be had to preserve as nearly as possible the same scale for the elevations and horizontal distances, in order to avoid distorting the outlines of the surface and the attitudes of the strata.

It is likewise suggested that the especial attention of the Geologists should be directed towards those formations containing materials of value in Commerce or the Arts, such as the useful and precious metals, coal which may be possibly made to assist some day the navigation of those seas, or salt valuable to the fisheries of the same region. Should such be found, some effort should be made to ascertain their accessibility to the coast with their probable extent. An extra quantity of specimens of this sort ought to be preserved for the purpose of submitting them to chemical analysis.

Where coasts are precipitous, much instruction may be procured of the adjacent geology by causing exact drawings to be made of the cliffs, and it is recommended for the purpose of procuring similar natural

sections from the interior, to seek out and traverse the gorges of those rivers which take their course *across* the strata.

Much information curious to Science may occasionally be gleaned from the boulders and water worn debris of the rocks, strewing the surface of a country and the beach upon the coasts, and suites of such specimens should be collected, especially where the interior of a region cannot be visited. With the same view whenever opportunity offers specimens should be taken of the soils and gravel and loose blocks of rock not unfrequently found upon the surface of the floating ice in high latitudes. These will sometimes furnish interesting deductions as to the nature of remote polar lands, from whence perhaps it may forever be beyond the reach of Man to bring any more decisive indications of their composition or natural history.

It is conceived that a certain amount of good to Navigation and Science may accrue from procuring specimens of the sediment at the bottom, taken by soundings at regular distances from a coast, especially if the direction and force of the currents be noted in connection.

We would enjoin a close study of all the geological phenomena connected with causes now in action, as well the volcanic where such occur, as those arising from the operation of the ocean upon the land. The circular figure and deep water of the Coral Islands having given rise to the conjecture that these fabrics of the Zoophytes are based upon the craters of submarine volcanoes, the collection of any facts calculated to throw light upon this subject will form one of the interesting duties of the Geologists.

The Geologists will do well to equip themselves with a complete apparatus for Blow pipe analysis, besides a supply of tools and tests, together with measuring chains and instruments of a portable kind for taking bearings, perhaps the best among the latter is Schmalkalder's Patent Portable Compass.

PHILOLOGY. [BY PETER S. DUPONCEAU].

The object of this Science is from the variety of languages which exist on the surface of the Earth, to trace, as far as possible, the history of Mankind; it tends also to facilitate Commercial intercourse. It is only since the beginning of the present Century, that barbarous languages, as they are called, that is to say, the idioms of those Nations who are not possessed of the Art of Writing, has been seriously attended to—But great progress has been made in the acquisition of this knowledge. Not only Vocabularies, but Grammars and Dictionaries have been published, of languages hitherto unknown, except by name, and sometimes even not so far. Before that time, Missionaries and Travellers alone paid attention to the languages of Savage Nations—the former, having only in view their laudable object, and not caring otherwise for the promotion of Science, kept their works in manuscript, or printed only a few copies for the use of their professional brethren, and those could not be purchased from the Booksellers; the latter confined themselves to a few meagre vo-

cabularies, dispersed through numerous books of travels, and of little use to the Philosopher who had not access to the large libraries of Europe, in few of which the Collections could be found complete. Philosophy, at last, took hold of the subject, and then began a new Science, called "the Comparative Science of languages." The learned Societies of the United States have acquired their share of distinction in the promotion of this Science; they have made known the forms and character of our Indian languages, and contributed largely to the mass of acquired knowledge.

Since the establishment by Great Britain of a Colony in New South Wales, the languages of the South Sea Islands, now considered by Geographers as a fifth part of the World, under the name of Australasia or Australia, have been particularly studied, and it has been discovered that most of them possess a considerable analogy with the language of the Malays, in the Peninsula of Malacca. What has been collected respecting those languages, is disseminated in various books of travels, and in the Memoirs of the London and Paris Asiatic and Geographical Societies. It would be in vain to attempt to collect them in this Country. There is however, a book, which we would recommend to be procured, if possible; it is entitled "An Account of the Nations of the Tonga Islands in the South Pacific Ocean, with an original Grammar and Vocabulary of their Language," compiled and arranged from the communications of Mr. William Mariner, by John Martin, M. D. London, 1817. 2 Vols. oct°.

Cook's Travels and other books of Travels in those seas are also recommended.

Since the Comparative Science of languages has been an object of attention among the learned, two important Works have been published in Europe, which would be of great use to the Philologist.

The first is the Mithridates, published in Germany about the beginning of this Century, in four or five Volumes, Octavo, according as it is bound. It gives a comparative view of all the then known languages existing on the surface of the Earth; it is the text book of Philologists, but it is written in German and not yet translated. The other written in French, is more modern; it was published at Paris in 1826—it is entitled "Atlas Ethnographique du Globe, ou Classification des Peuples Anciens et Modernes, d'apres leurs Langues, par Adrien Balbi."

This Work is a large folio in the form of an Atlas—Languages are in it divided and classed into families, and a tabular view is given of the languages of each family, compared with each other. The numerals from one to ten, and sixteen words of those in most common use, are thus given. Each comparative sheet of languages is preceded by an Ethnographic Sheet, giving a description of the different Nations to which the languages belong. There is also an introductory Volume, in Octavo.

If this book alone could be procured, it would be of great use to the Philologist—The American Philosophical Society possesses it, and would probably give it to the Expedition, on Government consenting to replace

it. The Society would then agree to procure for themselves another copy from France, at the expense of Government.

We now proceed to the duties of the Philologist.

Languages are to be considered in two points of view:

1. As to the words of which they are composed.
2. As to their Structure or Grammatical forms.

The short stay which the Expedition will probably make, in the different places where it will stop, will give but little opportunity to the Philologist to become acquainted with the Grammar of the several idioms, as that seems to require some previous knowledge of the language. Yet that may be obtained in some degree, at least, with the aid of Missionaries and Interpreters, where these are to be found. It would be desirable that they should, at least obtain, the conjugation of some verbs, and some sentences of the most common use. The Lord's prayer in each language, is particularly recommended, not because it is the best in a Grammatical point of view, but because it is that which Philologists have chosen from time immemorial, and, therefore, is the best to serve as an object of comparison—it is also recommended because it is the easiest to be obtained from Missionaries. The Philologist should parse it as well as he can, so as to give the full meaning of each word of which it is composed.

But the least difficult, though not the least important task of the Philologist, will be to obtain Vocabularies of the different languages. In or about the Year 1826, Mr. President Adams, as we have been informed, at the suggestion of Mr. Albert Gallatin, formed the project of collecting Vocabularies of all the languages spoken by the Indians in the United States, and to cause that Collection to be published as a National Work. With a view to that object Mr. Gallatin prepared a List of English words intended for Indian Vocabularies, with blanks for the Indian words. This List or blank Vocabulary was printed by order of the Government and the copies lodged in the War Office; to be sent to Missionaries and Indian Agents, to be by them filled up and returned to the Government. This plan, however, was only carried into partial execution and ultimately failed. It is believed that Copies of that blank Vocabulary still exist in the War Department. If so, a sufficient number might be given to the Philologist, to be filled up with the corresponding words in the different languages. One Copy, at least, it is presumed, must exist in that Department, from which a sufficient number might easily be printed off. It is believed to be rather too long, but it is a defect which may easily be cured. The Philologist will only take care to have the same words in his different Vocabularies—it is very important for the comparison of languages.

The English Orthography is very inconvenient for writing barbarous languages, not only on account of its numerous dipthongs, but because of the uncertain sounds of its vowels, which necessarily requires the separation of syllables. It is recommended to adopt the German and

Italian sound of the vowels; the spelling adopted by the Missionaries at Taiti, (Otaheiti) appears best adapted to the languages of the South Sea Islands.

ETHNOGRAPHY. [BY PETER S. DUPONCEAU].

Among the learned of America and Europe, Ethnography is now classed as a separate Science. It consists of the knowledge of the habits, manners, and customs of the different Nations of the Earth. Their manner of making War and Peace, their food, their dress, their festivals, their marriages and funerals, the education of their children, the rank which their Women hold in Society, the division of labour among them; their mode of living, by hunting, fishing or agriculture, their traditions, their laws, their industry, their exercises, all these and other analogous subjects offer a vast field of information which it is important to collect as much in detail as possible.

The charge of attending to this duty may with propriety be committed to the Philologist, with whose immediate pursuits the subject of Ethnography is closely related.

MEDICINE. [BY NATHANIEL CHAPMAN].

1. What is the state of Medicine among the inhabitants of the barbarous regions which may be visited? Have they any regular modes of treating diseases, or made any advances towards the formation of a Materia Medica, or Collection of remedies?

2. What are the most prevalent diseases among them? Are they liable especially to phthisis pulmonalis, or to scrofula or to arthritis, or to any of the morbid affections, more especially belonging to the state of civilization, and refinement. Does Mania, in any of its varieties, exist among them?

3. Has variola, nibeola, scarlatina, syphilis, or pertussis, or any other disease of modern origin, dependent on a specific contagion, extended to them, or have they any diseases peculiar to themselves?

4. What are their usual remedies, and are any of these worthy of attention? If so, endeavour to acquire an accurate knowledge of them, and of the modes of preparation and administration.

We are aware, Sir, that the series of inquiries thus presented is very far from comprising all the subjects that will demand the attention of the Corps: nor have we thought it essential that they should do so. The gentlemen appointed to the different situations will not fail to use every exertion to make themselves thoroughly acquainted with their duties before the sailing of the Expedition. Indeed, if time can be allowed for the purpose, it would be very desirable that some of the leading members of the Scientific Corps should make a visit to Europe, for the purpose of procuring instruments, books &c., and of obtaining useful information on the subjects committed to their charge.

We would recommend, as a measure of necessary precaution, that, before entering port on the return of the Expedition, the Commanding Officer should require all journals, charts, collections, and drawings made by Officers, Members of the Scientific Corps, or others, to be given up into his hands, for the Navy Department, to be there retained until after the journal of the Expedition shall be published under the directions of the Government, when such papers and other articles as may justly be considered the private property of individuals shall be restored to them.

We have the honor to be with the highest respect
Sir
Your most obedient humble Servants
Peter S. DuPonceau
Robert M. Patterson
N. Chapman
James P. Espy
Titian R. Peale
Charles Pickering
(Prof. Rogers, one of the Committee absent at the time of signing.)

List of Books, recommended to be taken on the Expedition for the use of the Officers and Scientific Corps.

Lord Anson's Voyage round the World in H. M. S. Centurian [*sic*]. (1740.)
Beechey's Narrative of a Voyage to the Pacific and Beerings Straits &c. (1828.)
Bougainville's Voyage round the World (Forsters translation) 1769.
Ellis's Polynesian Researches, 1829.
Freycinet's Narrative of a Voyage rd the World. 1820.
Kotsbue's Voyage of Discoy in the Sth Sea.
Morrell's Narrative.
Parry's Journals in search of N.W. Passage.
Péron's Voyage de decouvertes aux terres Australes.
Porter's Narrative.
Yate's New Zealand, 1835.
Bennett's Wanderings in N. South Wales &c. 1834.
Tyerman's Journal of Voyages and Travels &c. (South Sea Islands, China and India), 1831.
Weddell's Voyage towards the South Pole. 1825.
Adanson's Senegal.
Bennett's New South Wales.
Chronological History of discoveries in the Sth Sea by Capt. Burney, R.N.
Desmarest's Mammalogie.
Cuvier's Animal Kingdom.
do Dents de Mammiferes.

Traité d'Ornithologie par R. P. Lesson.
Cuvier's Histoire et Anatomie des Mollusques.
Dillwyms Catalogue of Shells.
Lumark's [Lamarck's] des Animaux sans vertèbres.
Latreille's Histoire Natur^e. des Crustacés et des Insectes.
De la Beche's Geological Manual.
De la Beche's Theoretical Researches.
De la Beche's How too observe Geology.
Lyell's Principles of Geology.
Humbolt's [Humboldt's] Works.
McCulloch's Classification of Rocks.
Dr. Danberry on Volcanos [*sic*].
Von Buck's Work upon Volcanos [*sic*].
Elie de Baumont.
Transactions of the Geological Society of London.
Article, Geology in the Encyclopedia Metropolitana.
Besides the Standard Works on Mineralogy.

In Silliman's Journal, Vol. 1^st, page 71, and Vol. 3^rd, page 249 useful instructions are to be seen relative to the choice and preservation of Geological Specimens.

These recommendations were gratefully received and were made a part of the detailed instructions issued to the Commander of the Expedition when it finally sailed in August 1838. Before that event Martin Van Buren had been elected eighth President of the United States and his Secretary of the Navy, the Honorable J. K. Paulding, was the ultimate authority in carrying out the provisions of the Congress in respect to this Exploring Expedition.

There was much disagreement in Congress and among the public regarding the need for this Expedition, and many diverse opinions among its promoters as to its proper size, equipment and personnel. After four naval officers, a Commodore and three Captains had finally declined to serve as Commander, the Honorable Joel Poinsett, Secretary of War, and for twenty-five years (1827–51) an honored member and benefactor of this Society, recommended Lieutenant Charles Wilkes, U.S.N. (Member A.P.S. 1843–77), who was given command of the Expedition March 20, 1838. The American Philosophical Society has no record of having helped to select the Scientific Corps, but as several were members of the Society it seems probable that some of these were recommended by individual members although the Society expressed the wish not to interfere in this matter.

The connection of the American Philosophical Society with our First National Exploring Expedition was largely in outlining the scientific work to be undertaken and the methods to be employed. These were referred to by the Secretary of the Navy in his "Instructions to the Commander" in the following words:

As guides to yourself and to the scientific corps, the Department would, however, direct your particular attention to the learned and comprehensive Reports of a committee of the American Philosophical Society of Philadelphia, the Report of a Committee of the East India Marine Society, of Salem, Massachusetts; and to a communication from the Naval Lyceum of New York, which accompany, and are to be regarded as forming a part of these instructions, so far as they may accord with the primary objects of the Expedition, and its present organization. You will, therefore, allow the gentlemen of the scientific corps the free perusal of these valuable documents, and permit them to copy such portions as they may think proper.

The difficulties and disagreements were not confined to the organizing and outfitting but continued throughout the expedition and even afterwards. Commander Wilkes in his Narrative refers briefly to some of these as "attempts to throw impediments in my way"—"many misstatements have been circulated"—"persecution I met without any hearing"—"no reference here to proceedings of the courts martial,"—"dissatisfaction was felt at the outset by a few of the naturalists because they were not allowed all opportunities they desired of making investigations."

Among these naturalists were Titian Peale and Charles Pickering, whose dissatisfaction lasted until after the return of the Expedition. Two enthusiastic letters from Peale which are in the possession of this Society are of such interest that I reproduce them exactly as follows:

[T. R. Peale to R. M. Patterson, U. S. Mint, Philadelphia]

U. S. Ship Peacock, at Sea
Nov[r] 13[th], 1838

My dear Sir

We are now rolling and plunging merily before a strong trade wind, with studden sails below and aloft, on both sides, in way that surpasses any Eagle with spreading wings designed for coin; *all* the men are scrubbing, holy stoning, and sluising the ship, to be ready for painting inside and out, buckets and swabs fly in all directions; Saturday in Phil[a] is a fool to it; I sought refuge in my room, not safe here for even

now the water is spurting in jets through the key hole; every one heaves with a good will, and well they may, we have been three months at sea, —nearly and expect to be in Rio Janairo in as many days, there?—you'd be bothered to know whats there if you could hear either men or officers, —everything from an onion up to a bullock is to be had.

The service thus far has been pleasant, but tedious, we have been on fruitless searches for some half dozen shoals, of course little or nothing for *me* to do; I had a few days at Maderia, and one at the Cape de Verde, they were overwhelming because I had to do everything myself; shoot, write, draw, and explain to the uninitiated; there is not an individual in the Scientific Corps but who is now before the world for the first time, some who do not know even the rudiments of the service they are on; of course fuss, and mystified dignity, is constantly elbowing with quiet labor. The selection of officers has been a very happy one,—we get along very happily—I feel lucky in getting with Capt[n] Hudson, and only regret suffering them to take Williams from me, and if I cannot supply his place in Rio Janairo, it is questionable with me whether I ought not to leave the expedition, for I can be little more than the stuffer of skins—or be considered a churl, no one in the whole squadron understands it, and without vouchers Nat! history is questionable, particularly that of beginners. Capt Wilkes's assertion that I would find sailors ready and able to do anything, and that I should have their services whenever required, arose merely from the wish to remove impediments to the sailing of the expedition as quietly as possible, *all* the sailors are required for their own immediate duties, and they are not willing to have more imposed on them than they can help: There will be a "beggarly account of empty boxes" and in this one respect at least the English and French will hold their way. Our "Scientific Corps" is precisely in the state of our Militia in Penn[a]; the officers must be their own boot blacks, and washerwomen, because they have no men.

Last night or rather this morning was the anniversary of the "Meteoric Shower" we had sundry observers but the greatest number observed in one hour was 71, of these I strongly suspect a large proportion were caused by the rolling of the ship, indeed when she pitches sometimes it looks as though the whole firmament was coming down; we saw what was more singular a few weeks since, which was the aurora australis, or aurora from south of the line.

Rio Janairo Nov[r] 20th 1838

We arrived here this afternoon, but to our surprise and disappointment found we were the first of our Squadron from which have parted since we have been searching for shoals near the equator; the Relief we have some apprehension for, as she was to meet us at Porto Praya; the frigate Independance Com[r] Nicholson is here expecting to sail in a few days. The scenery of this harbour is Magnificent beyond anything I have ever seen, and as we expect to be detained some time I shall prob-

ably have some time to attempt a description, at present remember me to all enquiring friends and believe me

Yours truly

(Signed) T. R. PEALE

[T. R. Peale to Jn. K. Kane Esq Philadelphia, Penna.
Pr Whale ship Lydia]

U S S Vincennes Bay of Isl$^{ds.}$
New Zealand April 5th 1840

My dear Sir

. . . The *Antarctic Continent* was discovered and surveyed for about 1500 miles by our squad$^{n.}$ The French Exped^{n} under d'Urville made land at another point in the afternoon of the same day, a few hours after our ships, but made no other attempt and immediately returned to secure their discovery before the world at home, they met the Brig Porpoise whilst among the ice, but made all sail and ran away without speaking her the moment they clearly made out what she was, although near enough to distinguish the officers' faces with a glass, d'Urville himself on deck. So much for french courtesy; our officers were all delighted at the prospect of exchanging civilities, when "Crapo" turned and *run.*

Our next land is the Feejees towards which we all look with great interest.

The Peacock was near being lost in the ice, during a heavy gale, but escaped with the loss of rudder keel, bulwarks, spars, &c., and succeeded in getting into port Jackson, to repair, and will follow to Tongataboo, where I expect to join her again, all the rest of our squadron are now in good [shape] expecting to sail tomorrow at daylight.

Yours,

(Signed) T. R. PEALE

It is because of these connections of this oldest learned Society of America with our First National Exploring Expedition that it has been considered appropriate to hold this Centennial Celebration in the Hall of the American Philosophical Society.

CONNECTION OF THE ACADEMY OF NATURAL SCIENCES OF PHILADELPHIA WITH OUR FIRST NATIONAL EXPLORING EXPEDITION

JAMES A. G. REHN

Corresponding Secretary of the Academy of Natural Sciences of Philadelphia

(Read February 23, 1940, in Centenary Celebration of the Wilkes Exploring Expedition, 1838–1842)

At a meeting of the Academy of Natural Sciences of Philadelphia held September 6, 1836, there was read a communication from the Honorable Mahlon Dickerson, Secretary of the Navy, requesting the advice of the Academy in the formation of a Scientific Corps to accompany what was described as ". . . an exploring and surveying expedition for the southern ocean," and also calling upon our Society for the suggestion ". . . of inquiries as to subjects of natural history . . . to which the researches of this corps should be particularly engaged."

The letter in its details and wording is almost identical with that already read [1] by Dr. Conklin as having been received by the American Philosophical Society, and the date it bears is the same—August 31, 1836.

After the reading of this letter at the meeting of September 6, 1836, it was referred for consideration to a committee of three, consisting of Drs. Charles Pickering, R. E. Griffith and Samuel George Morton, the last of whom later became President of the Academy. At the same meeting Dr. Pickering laid before the Academy a plan concerning the Scientific Corps which it was intended to have accompany the expedition that had been recommended by the New York Lyceum, together with suggestions from several scientific gentlemen, all of whom were referred to the committee appointed to consider this matter.

On September 13, 1836, the committee reported verbally to the Academy; its report was accepted and the committee discharged. A resolution was adopted at the same meeting stating the Academy concurred in the suggestion of the committee, ". . .

[1] Connection of the American Philosophical Society with our First National Exploring Expedition. E. G. Conklin. Proc. Amer. Philos. Soc. 82: pp. 519–41.

that a nomination of such persons as are suitable to occupy situations in the proposed Scientific Corps is not advisable, and therefore that the invitation to nominate persons be not carried into execution."

At the same meeting, however, the Academy by resolution authorized the appointment of a committee of eight persons ". . . to subdivide themselves into minor committees in the several branches of Natural History, with a view to a joint report to be composed of the several distinct reports of the subcommittees in conformity with the wishes of the Secretary of the Navy, and to report at the next meeting." The committee then appointed consisted of Drs. R. E. Griffith, Samuel George Morton, Robert Bridges, Benjamin H. Coates, Paul B. Goddard, Richard Harlan, Reynell Coates and Mr. Richard C. Taylor. A further resolution adopted at the same time invited individual members of the Academy to present to that body, or its committee, suggestions as to the desired objectives of the scientific work of the Expedition.

The minutes of the Academy meeting of September 20, 1836, show that the committee in charge of the Exploring Expedition recommendations was deeply engaged in its work, and at its request the body was continued to report at the next meeting of the Academy. However, we are informed certain of the reports of the subcommittees were read, including one by Dr. Reynell Coates on "Moluscous Animals", another by Dr. Benjamin H. Coates on medical information to be sought, and a third by Dr. Richard Harlan on general zoological objectives.

On September 27, 1836, the minutes state ". . . the Committee on the Proposed Expedition to the Southern Ocean presented a joint report, accompanied with the reports of the Sub-Committees numbered from one to six." This group consisted in part of one report on the organization of a Scientific Corps, another on botany by Dr. Griffith, and a third on geology and mineralogy by Dr. Morton and Mr. Taylor. The inclusion of a subcommittee report on physical science was, by motion, dispensed with, but this was added by action taken subsequently on October 18 of the same year. After the presentation of the joint and subcommittee reports these were formally adopted and the committee discharged.

The record book of the Corresponding Secretary of the Acad-

emy, then Dr. Samuel George Morton, gives a transcript of the letter to the Secretary of the Navy, under date of September 27, transmitting ". . . by the hands of Dr. Reynell Coates, the Reports presented by the several Committees of this institution in relation to the proposed Scientific Exploring Expedition. These documents embrace:

"1. The Report of the Committee containing suggestions for the Organization of the Scientific Corps.

"2. The Reports of the subcommittees on the various branches of Science which claim the attention of Government on this highly important occasion,—so full of promise to the cause of Science, and so honourable to the National councils."

A subsequent letter, under date of October 25, 1836, transmitted to the Secretary of the Navy the report of the subcommittee on physical science.

Unfortunately in the course of over a century the retained drafts of these reports—if in fact any were retained—have disappeared, and we are unable to say from our records exactly what the recommendations may have been, or how broadly they eventually figured in the plans, personnel or operations of the Expedition.

The chronological records, however, show gratifying celerity in the preparation and transmission of the recommendations, which in large part reached the Secretary of the Navy in less than a month from the presentation of his request to the Academy, rather remarkable speed when the usual lack of swiftness of committees of this type then and now is considered.

While we are unacquainted with the details of the recommendations we know that of the nine individuals included in the scientific personnel of the Exploring Expedition, as set forth by Captain Wilkes on page 29 of his official Instructions in the first volume of the Narrative of the Expedition, Messrs. Charles Pickering and Titian Peale, classified as the "naturalists," were very active Members of the Academy, while Messrs. Joseph P. Couthouy, the conchologist, and James D. Dana, the mineralogist, were Correspondents of the same institution. I may say in explanation that Correspondents of the Academy are non-residents elected to this honorary category in recognition of special preeminence in their respective fields of research within the natural sciences. From this point the rôle of the Academy and

its membership was chiefly in the preparation and publication of the technical results of the natural history phases of the Expedition's researches.

Preceding the publication of the main reports it should be noted that the Academy published in its *Proceedings* eleven preliminary papers based on the Expedition's collections. These were: one on mammals and two on birds by John Cassin, appearing in 1853, 1854 and 1855; four on Crustacea by James D. Dana, in 1850, 1851 and 1852; and four on reptiles by Charles Girard, in 1853, 1856 and 1857. While these papers were in most cases quite short, they gave priority of publication to many new discoveries, made by the Expedition, over the subsequent full reports by as much as five years in certain cases.

While it is not my purpose to make a bibliographic analysis [1] of the various volumes published on the work of the Expedition, it is, however, necessary briefly to comment on those of a natural history character to understand the part taken by Academy members and correspondents in the study and production of these results. The index of United States Government Publications issued in 1885 by the Government Printing Office lists, aside from the 1851 reprinting of the five volumes of the "Narrative," twenty-four projected volumes, of which it is stated five were not published, *i.e.* a second volume (XVIII) by Asa Gray on botany, the volume by Pickering on "Geographical Distribution of Animals and Plants" (there called Volume XIX), two volumes on ichthyology by Agassiz (XXI and XXII) and a volume on physics by Wilkes (XXIV). We know very definitely the Pickering volume was published, although not in the official series; the Academy's copy is here exhibited and it is also to be found in other libraries at home and abroad, including that of the British Museum of Natural History, while the suppressed and therefore exceedingly scarce Titian Peale report on the mammals and birds is totally ignored. The Peale volume, issued in 1848, ten years prior to the Cassin report on the same subjects, is probably the scarcest volume issued by the United States Government, and we are able today to show also the Academy's copy of this important and valuable work.

[1] For complete bibliography see "Reports of the Wilkes Expedition, and the Work of the Specialists in Science". H. H. Bartlett. PROC. Amer. Philos. Soc. 82: pp. 601–705.

The 1885 summary of the "official" reports lists 19 volumes and 9 atlases of the original series as published. The Library of the Academy of Natural Sciences of Philadelphia contains of the Expedition's reports 18 volumes and 9 folio atlases, although we know two volumes and one atlas bearing on subjects not germane to our work are unrepresented. The discrepancy seems to be due to the 1885 listing not giving the quarto atlas of the Narrative as a distinct volume, as it is numbered (Volume VI) on the original binding of the cover of the copy in our possession. The Peale report on mammals and birds was omitted in the 1885 listing, and Pickering's distributional report was said to be unpublished, true probably in an official sense, but not when scientific publication is concerned. While the Academy does not have in its Library the volume on meteorology by Wilkes, which fortunately this Society possesses, or that on hydrography by the same author, it does have all bearing upon our field of work, *i.e.* the natural sciences. The 1885 listing gives but two folio atlases on botany, but three were published, all of which are in the Academy library.

Of the eighteen authors of all of the various reports contained in the impressive series of volumes and atlases issued on the results of the United States Exploring Expedition, four were Members and eight Correspondents of the Academy of Natural Sciences, while of the nine officially listed members of the scientific staff of the Expedition four were Members or Correspondents of the same organization.

The justified desire of those concerned in producing a series of scientific reports on the Exploring Expedition's work comparable to those presenting the scientific results of the voyage of the French corvette *Astrolabe,* was fully realized. Some of the handicaps under which the Scientific Corps did their work are little known today, but they show the caliber of the men who carried this burden. It seems that while navigating and similar instruments were available, as Wilkes says in his narrative these were purchased in Europe by him especially for the Expedition, microscopes were not so readily procured for the scientists. Unofficial information brought to the Academy's attention in recent years advises us that Dr. Paul B. Goddard, an active Member of the Academy and a well-known physician and photographic pioneer of Philadelphia, helped the Corps meet this deficiency

by loaning his personal microscope to aid the field work of these investigators.

We know the limited editions of the reports and the methods of their distribution caused considerable concern. A letter to the Academy from James D. Dana, under date of January 26, 1849, asks that further efforts be made to obtain larger editions of the volumes and the donation of copies to scientific, college and public libraries. As Dana states in his letter to Dr. Morton, then President of the Academy, "Congress, as you know, requires frequent efforts, before even a good object can be secured. There is need of influence in behalf of the Expedition and Science at the present time, for there is much opposition to us." He further asks, "Could you not also secure a petition from the Philosophical Society?" Just what effect these efforts directly produced, if any, I am unable to say.

It is worthy of note, however, that although the Academy by invitation of the Federal Government took an active part in the formulating of a program for the Expedition, and its members of various types constituted a considerable part of the Scientific Corps and the bulk of the natural history reports were prepared by men of the same character, we owe our splendid series of report volumes not to the generosity of the Federal Government, but almost entirely to the gifts of the authors themselves or to the unrivalled generosity of Dr. Thomas B. Wilson, the modest, retiring patron, who died in 1865 after having resigned as President of the Academy, of which he had been the greatest single benefactor. Where Dr. Wilson purchased these volumes we do not know, but that along with hundreds of similar classic volumes, we owe them to his keen interest we cannot forget.

The natural history collections secured by the Expedition today naturally are scattered, there having been no active central governmental repository at the time the studies were undertaken. It was not until some years after the Expedition had returned that the new Smithsonian Institution was fully functioning, and in consequence the natural history materials will be found in part at least distributed among institutions and organizations with which authors of the various reports were associated. The Academy possesses a number of ornithological specimens handled by Peale and Cassin, several of which with original expedition labels are here exhibited, a few of the mam-

mals, certain of the reptiles and lower invertebrates, and a representation of botanical specimens, as well as considerable ethnological material. Certain of these collections were presented to the Academy in later years by the Smithsonian Institution.

In closing it seems desirable to suggest that an inventory be made of the known distribution in American libraries of copies of the various reports of the Expedition. The information now available shows that at least some library representations are very incomplete, and such an analysis would greatly assist future consultations. Like the reports of the great French voyages of the first half of the nineteenth century and of the *Challenger,* those issued on the work of the United States Exploring Expedition are not of merely transient or ephemeral value, but will go on through the ages as definite landmarks in the acquisition of natural knowledge.

THE PURPOSE, EQUIPMENT AND PERSONNEL OF THE WILKES EXPEDITION

CAPTAIN G. S. BRYAN, United States Navy

Hydrographer of the Navy

(Read February 23, 1940, in Centenary Celebration of the Wilkes Exploring Expedition, 1838–1842)

THE whaling industry in the New England States reached its peak prior to the year in which this country gained its independence. Both this war and that of 1812 were fraught with disaster to the whalers, but nevertheless, following each of these conflicts, only a few years were required for recovery.

Shortly after the War of 1812, whaling ships with their hardy crews first began to round Cape Horn and seek the more profitable whaling grounds which had just been discovered in the Pacific. At first confining their whaling to the waters off the coasts of Peru and Chile, they soon spread out over the entire Pacific, even as far north as Kamchatka. At the same time they began to frequent the high southern latitudes of the Antarctic regions where they found not only whales but also seals in abundance.

New whaling grounds were continually being sought for. Incident to the search, numerous islands, reefs, and shoals, hitherto unknown, were reported in the Pacific, while the sealers were equally successful in locating new lands in the Antarctic Regions. In 1828, an official government report prepared by Mr. J. N. Reynolds listed some 200 of these discoveries. This list, however, was far from complete as, with the intense rivalry between the different whaling ships, few captains cared to broadcast to their competitors the information they had picked up regarding good whaling grounds, or even good harbors for use as bases.

Reports of the discoveries of new lands, however, were too often accompanied by other reports of a more ominous nature. Stories of shipwrecks, of massacres by the natives, of long open boat voyages as a result of these, all too frequently trickled back to the whaling centers of New England. As a natural sequence,

demands soon began to be made that the Government do something to remedy this situation.

In 1826, there was considerable agitation before Congress as to the desirability of the Government undertaking an expedition for the purpose of surveying and charting those parts of the world most frequented by our whaling vessels. This continued for two years, at which time Reynolds (who was mentioned in a previous paragraph) added his efforts to the end that a bill authorizing such an expedition was actually introduced in the House, although it was never passed. The following year saw still another abortive effort made to put through a bill.

At this time, a new Administration came into power with a policy of economy and retrenchment, and efforts to revive the project were unsuccessful. In 1831, Edmund Fanning and Benjamin Pendleton, in a memorial submitted to Congress, again focused attention on the subject. For several years following, petitions and memorials covering this subject continued to pour in on Congress until finally an amendment to the Naval Appropriation Bill was passed by both houses authorizing a surveying and exploring expedition, and appropriating $300,000 to cover it. It had thus required ten years to get a bill through authorizing the Expedition.

This act simply authorized the President to "send out a surveying and exploring expedition to the Pacific Ocean and South Seas," leaving to him all further details. With all the agitation that had preceded the passage of this bill, however, the motive back of the expedition was apparent to all. With the bill passed and the appropriation provided for, it was generally supposed that the expedition would be ready to sail within about three months. This proved to be far from correct.

President Jackson appears to have been quite enthusiastic about the Expedition but Secretary of the Navy Dickerson was generally credited with opposing it, in spirit if not in fact. The scientific world of that day was intensely interested and played an active part in the planning of the scientific work of the Expedition. Suggestions came from various scientific societies, including the American Philosophical Society, all offering their cooperation.

It was at first planned to use the frigate *Macedonian,* the brigs *Pioneer* and *Consort,* and the schooner *Pilot,* with the

Relief as storeship. Commander Thomas Ap Catesby Jones was designated to command the Expedition and also placed in charge of recruiting men for this service. Lieutenant Charles Wilkes was sent to Europe to purchase the necessary instruments. It looked as if the project were off to a good start.

Unfortunately, however, this was not the case. On his return to the Capital after about four months absence, the President was informed by Secretary Dickerson that he was unable to persuade sufficient men to enlist for this duty, and that due to the lateness of the season it would be necessary to postpone the Expedition until the following year.

In February, 1837, Congress took a hand and called for information as to the progress that had been made. The report of the Secretary of the Navy, in response to this resolution, was forwarded to Congress by the President with a strong indorsement favoring the project. In spite of this indorsement and the appropriations made available by Congress, Secretary Dickerson appears to have still been very lukewarm regarding the Expedition, giving as his reason the fact that the naval ships were required for other duties of a more urgent nature.

From the viewpoint of the present time, it appears very much as if the Secretary of the Navy were trying to kill the project by a policy of procrastination. Two separate Boards of Officers were appointed during the year 1837 to investigate the possibility of reducing the size of the Expedition, and both Boards proved to be composed of perfect "Yes men". Meanwhile some of the equipment procured for the Expedition had been supplied the *Porpoise,* then surveying Georges Bank off the coast of Massachusetts. The point was then made that the Expedition would have to be delayed, as otherwise the survey then in progress would have to be cut short. Finally, it was announced that the appropriation for outfitting the Expedition had all been expended, and that nothing remained except to make use of vessels already in the Naval service and to utilize regular naval appropriations as far as possible.

Commodore Jones, discouraged and disgusted, and now in failing health, tendered his resignation as Commander of the Expedition. Commodore Shubrick was offered the command but declined on account of the character of the vessels of the squadron. Captains Kearney and Gregory were nominated in

turn, but due to the injection of politics into the situation, they were also forced out of the picture. Captain Joseph Smith, the next nominee, would accept only under certain conditions, one of which was that Lieutenant Charles Wilkes command one of the ships. As that officer declined to go in that status, Smith also withdrew.

Baffled by the manner in which all the high ranking officers were disinclined to serve in command of the Expedition, Acting Secretary Poinsett, Secretary Dickerson being ill, decided to choose a younger and more junior officer, and the choice fell naturally on Wilkes. That officer promptly accepted and as we all know remained in command of the Expedition until its return to this country.

Charles Wilkes was born in New York City on April 3, 1798, and entered the Navy as a midshipman in 1818. He made one cruise in the Mediterranean, another later in the Pacific, was promoted to the rank of Lieutenant in 1826, and in 1833 was assigned in charge of the Naval Depot of Charts and Instruments, the forerunner of the present Hydrographic Office. Equipped with a natural scientific turn of mind, he was here in a position to exploit it, and in the course of this duty he constructed what is said to be the first Observatory built in this country, using a five foot transit instrument which had been obtained for the Coast and Geodetic Survey.

It was while he was on this duty that Wilkes was sent to Europe to supervise and purchase the instruments required for the Expedition. Shortly after his return to Washington in 1837, he was detached from the Depot of Charts and Instruments and assigned to the brig *Porpoise* for the duty of surveying Georges Bank from which he was called to head the Expedition.

This brief sketch of his career, up to the time of his appointment to command the Expedition, shows the scientific background of Wilkes but throws little light on his other qualities. He was a scholar, a stern disciplinarian, and possessed a high degree of courage. At the same time, he was quite self-opinionated and possessed of a fiery temper, which combined with an innate stubbornness to keep him engaged in controversies with his superiors throughout his entire naval career.

At the time Wilkes was assigned to command the Expedition, the matter of organizing it had been bungled so completely for

the two years subsequent to its authorization, that the public was completely disgusted. The crews that had enlisted for the venture had already served for some time under these conditions and openly showed their dissatisfaction with the manner in which the Expedition had been mishandled.

To make things worse, Wilkes was confronted with the fact that it was now necessary to reduce greatly the size of the Expedition, and that new ships had to be fitted out and equipped. The duty also devolved on him personally to conduct the necessary tests of the instruments intended for the squadron.

Whatever his faults, Wilkes certainly did not lack either decision or industry. He was informed on the 20th of March, 1838, that he had been placed in command of the squadron. He was not informed until a month later as to what ships were to compose the Expedition. Conscious of the fact that two years had been wasted in vain attempts to get the Expedition underway, Wilkes set a sailing date for his squadron of August 10th. In spite of the fact that a complete reorganization was necessary, the Expedition sailed a few days after the assigned date.

The ships assigned to the Expedition were as follows:

The *Vincennes*—Flagship of the squadron, a sloop-of-war of 780 tons.
The *Peacock*—Also a sloop-of-war, of 650 tons.
The *Porpoise*—A brig of 230 tons.
The *Relief*—Store ship of the Expedition.

To augment these ships, two New York Pilot boats of 110 and 96 tons, respectively, were purchased and renamed the *Sea Gull* and the *Flying Fish*. These were acquired on August 3rd, and the necesssry alterations promptly made so that they joined the squadron at Hampton Roads nine days later.

All of the ships assigned were of very small size. Some attempt was made to provide as much living space as possible on each of them. Another deck was added to the *Vincennes* and a poop cabin and a forecastle were added to the *Porpoise*. Such alterations as were made, however, added little to the comfort of the crew or to their protection from the elements during the Antarctic cruises.

The "Narrative" is replete with complaints of the poor condition of the ships and of the hardships and discomforts oc-

casioned the crew during the polar cruises of the squadron. The fact that the ships were not in good condition and were also poorly equipped for the cruise was well known by Wilkes prior to his departure. He was a man of action, however, and rather than add another chapter to the vexatious delays with which the Expedition appears to have been cursed, he decided that the best solution of the problem was to get the Expedition started and to utilize the resources of the squadron itself as far as possible to improve the condition of his ships whenever an opportunity presented.

This action was characteristic of Wilkes. Many times in his "Narrative", he questions the safety or advisability of certain operations, particularly that of the Antarctic cruises. However, he always arrived at a prompt decision. "We had orders to do this," he generally explains which to him was sufficient to brush aside any and all obstacles.

The outfit of clothing procured for use in the polar regions afterwards proved to be of an inferior grade and entirely inadequate. This had been purchased at great expense but it appears that the government had been swindled as, according to Wilkes, none of the articles of this description came up to the quality of samples exhibited.

The scientific equipment was of a quite different nature. Wilkes himself had been sent to Europe to purchase these articles and he notes that "every expense that could be lavished on this equipment was incurred". Also he personally did all the necessary inspection and calibration of instruments at the Observatory that he had set up on Capitol Hill in Washington.

One of his principal difficulties was to reduce the amount of equipment purchased to a size which the reduced squadron could conveniently carry. It must have cost him many headaches to have to leave behind so many instruments that he felt to be essential to the success of the Expedition.

The Expedition, as it put to sea from Hampton Roads, included a total of 83 officers and 342 enlisted men. Replacements were made from time to time in a number of ports visited on the cruise, but most of the officers at least served for the entire cruise. Some were never to return, as disease, shipwrecks, and attacks by natives all took their toll of lives during the four years.

Some of the officers bore names which were afterwards to be well known in history. For the cruise of the Expedition, however, Wilkes was the guiding spirit and his name was always the one that was dominant. The U. S. Exploring Expedition is better known as the Wilkes Expedition.

The roster of scientific members of the Expedition also included some whose names were afterwards destined to be quite illustrious. It was at first planned to take 18 scientists and various societies were asked to recommend suitable persons. The list of those that actually accompanied the Expedition, as given in the ''Narrative'', however, is as follows:

Mr. Hale—Philologist.
Mr. Pickering—Naturalist.
Mr. Peale—Naturalist.
Mr. Couthouy—Conchologist.
Mr. Dana—Mineralogist.
Mr. Rich—Botanist.
Mr. Drayton—Draughtsman (artist).
Mr. Agate—Draughtsman (artist).
Mr. Brackenridge—Horticulturist.

The activities of these scientists are being covered in other papers to be read before this meeting. Suffice it to state here that the representatives selected were men who had the confidence of scientific societies of that day and that their work reflected great credit on the Expedition.

The instructions to the Expedition, which were issued by the Secretary of the Navy, Honorable J. K. Paulding, and which Wilkes claims were mostly drafted by himself, constitute a very interesting document. The first sentence, outlining the general scope of the project, is quoted here in full:

> The Congress of the United States, having in view the important interests of our commerce embarked in the whale-fisheries, and other adventures in the Southern Ocean, by an act of the 18th of May, 1836, authorized an Expedition to be fitted out for the purpose of exploring and surveying that sea, as well as to determine the existence of all doubtful islands and shoals, as to discover and accurately fix the position of those which lie in or near the track of our vessels in that quarter, and may have escaped the observation of scientific navigators.

This simple sentence outlines the purpose of the Expedition quite completely. The remainder of the instructions simply cover details. The term "explore" was, of course, interpreted to cover the scientific investigation of the lands visited as well as the hydrographic. To quote the instructions further:

> Although the primary object of the Expedition is the promotion of the great interests of commerce and navigation, yet you will take all occasions to extend the bounds of Science and promote the acquisition of knowledge.

Special care was enjoined on the Expedition when dealing with the natives. Apparently there were no illusions in this country as to where the responsibility lay for the antagonism experienced by the crews of the whalers in their intercourse with the South Sea Islanders. Officers and men of the squadron were enjoined to show courtesy and kindness towards them at all times; moderation and forbearance were urged in dealing with cases of theft which was the usual excuse for starting trouble; and interference with native customs and habits was particularly forbidden.

One feature of the instructions, with a rather modern journalistic flair, was the strict prohibition against furnishing any outsider with any paper or other information concerning the work of the Expedition. In fact, orders were issued that all papers of this sort should be surrendered to the captain of each ship prior to the return of the Expedition to the States. A violation of this order by Wilkes himself, when he sent a copy of his Antarctic Chart to Ross, was later to result in much unjust and unmerited criticism being directed against him.

Particular attention was directed in the Instructions to the "Feejee" Islands, the Expedition being charged with the selection of a good harbor in that group for the use of vessels engaged in the whale fishery. It was also stated to be the intention of the Navy Department to station a man-of-war in these regions. After such a harbor had been selected, Wilkes was enjoined to "teach the natives of the modes of cultivation", and to encourage them to "raise hogs in greater abundance". The idea here appears to have been to provide for a supply of food for whaling ships.

The instructions scheduled three objectives which were to be covered in the Antarctic regions. The first of these was to make a dash to the southward from some base in Tierra del Fuego with two of his ships. The port selected was Orange Harbor, just a short distance north-west of Cape Horn. From this base, Wilkes was to:

> Explore the southern Antarctic to the southward of Powell's Group * and between it and Sandwich Land following the track of Weddell as closely as practicable, and endeavoring to reach a high southern latitude.

On the completion of this trip, Wilkes was to rejoin his other ships in Tierra del Fuego and the entire squadron was then to:

> Stretch toward the southward and westward as far as the *Ne Plus Ultra* of Cook, or longitude 105° West.

The third and much the most important of the three cruises in the Polar Regions was directed to be made after fitting out at Sydney the following year. Wilkes was here instructed to make:

> A second attempt to penetrate within the Antarctic Region, south of Van Diemen's Land, and as far west as longitude 45° East, or to Enderby's Land.

The first two of these objectives were very badly timed. According to the Instructions, Wilkes was to explore to the southward of the South Orkneys following the track of Weddell as closely as possible, which would have sent him into the Weddell Sea. He was cautioned to avoid being caught in the ice and was instructed to rejoin his other ships at Tierra del Fuego between the middle of February and the beginning of March.

It is not clear whether the delay was due to the late sailing of the Expedition, or to delays encountered on the way to Tierra del Fuego, but the fact remains that Wilkes did not leave Orange Harbor until February 25th, 1839. For some unknown reason, also, he appears to have headed through Bransfield Strait to the westward of Palmer Land and not into Weddell Sea. Due to the lateness of the season and the bad weather encountered,

* The South Orkneys.

there was very little accomplished on this cruise although a landing was made on Deception Island by the *Sea Gull.*

Apparently the second Antarctic voyage was to be only a reconnaissance as it was considered jointly with the first cruise south, and was to be undertaken after the completion of that venture. This would have made it extremely late in the season, even if the original schedule had been carried out. However, Wilkes showed good judgment in dividing his forces at Orange Harbor and sending the *Peacock* and *Flying Fish* to cover the western section at the same time that he was in the vicinity of Palmer Land with the *Porpoise* and *Sea Gull.*

Even this maneuver, however, was not effective. Almost the entire time the two ships were beset by storms and fog, and, while the ice barrier was soon sighted and an extreme southern latitude of 70° reached, the ships were finally compelled to head north on March 24th.

Very little was accomplished on this venture which is hardly surprising in view of the lateness of the season. However, the experience gained in these two trips was extremely helpful in making the third Antarctic Cruise a successful one. As this cruise is being covered in another paper before this Meeting, I need not discuss it further here.

The real purpose of the Expedition was perhaps best expressed in the words used in the Instructions which are quoted verbatim here:

> . . . you will use your best endeavors wherever you may go, to leave behind a favourable impression of your country and countrymen. The Expedition is not for conquest but discovery. Its objects are all peaceful; they are to extend the empire of commerce and science; to diminish the hazards of the ocean and point out to future navigators a course by which they may avoid dangers and find safety.

The sentiments contained in the preceding quotation are typical of the spirit of American Exploration wherever conducted, not alone in the era in which Wilkes carried on but down to the present time, a century later.

THE DISCOVERY OF WILKES LAND, ANTARCTICA

WILLIAM HERBERT HOBBS

Professor Emeritus of Geology, University of Michigan

(Read February 23, 1940, in Centenary Celebration of the Wilkes Exploring Expedition, 1838–1842)

WILKES was not the first to discover Antarctic land—that had been done twenty years earlier by Palmer within the American sector of the Antarctic—but he was the first to explore a stretch of coast long enough to prove its continental character. His expedition first brought out the fact, later found to be true of the continent as a whole, that it is protected from the explorer by a fringe of sea-ice and frozen-in icebergs—a variable zone usually some tens of miles in width which Wilkes referred to as the "Icy Barrier". The Antarctic land itself was seen as a high snow-covered expanse with few outstanding features which had sufficiently individual characteristics to be recognized from more than one position. Hence landfalls could not be fixed in position with certainty upon the map by means of intersecting sights from different ship positions. The landfalls, snow-covered as they were, were also in some cases difficult to distinguish from tabular icebergs many miles long and other forms of off-shore ice, and especially from that form later known as shelf-ice. The landfalls had then of necessity to be fixed upon the map through direction from a single ship position and with the distance estimated.

What neither Wilkes nor much later explorers realized was that the air near great masses of inland ice is so free from dust and moisture as to be of a clarity then quite unknown. As a consequence distances in the Antarctic have generally been greatly underestimated, and the land has been placed upon the map sometimes a fraction only of its real distance away. Still less appreciated has been a fairly common phenomenon of *looming*—superior or polar mirage—which for considerable intervals of time brings land into view even when it is very far

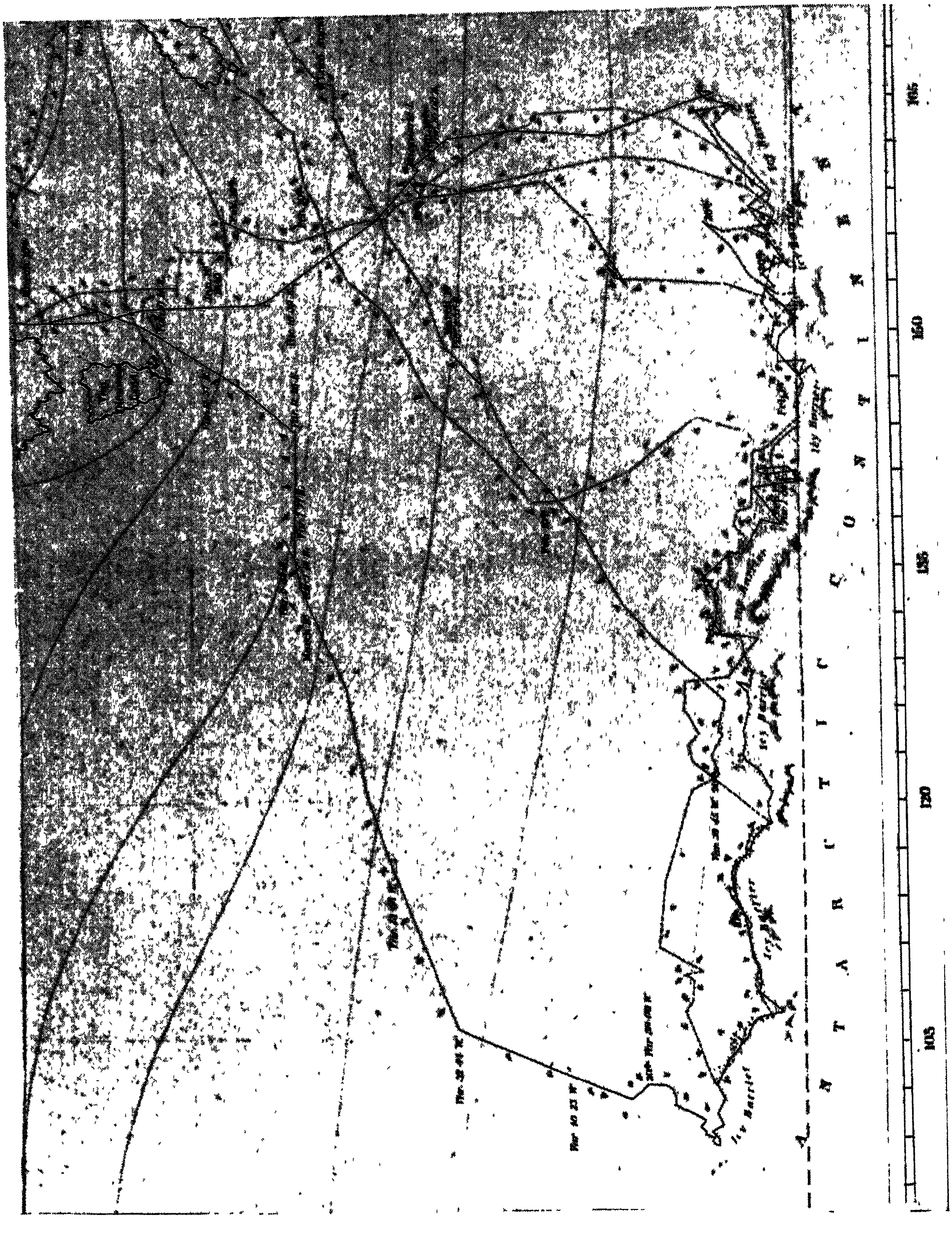


FIG. 1. Wilkes's map of his Antarctic cruise (smaller map).

below the horizon.[1] It will be necessary to refer again to this matter of looming after the Wilkes landfalls have been discussed.

The American expedition left Sydney in New South Wales on December 26th, 1840, with a squadron of four ships; the *Vincennes,* flagship, *Peacock* (Hudson), *Porpoise* (Ringgold) and *Flying Fish* (Knox). Within a week the *Flying Fish* and *Peacock* had become separated. The *Vincennes* and the *Porpoise* proceeded and on January 11th they reached the Icy Barrier in latitude 64° 11′ and longitude 164° 30′ E. No appearance of land had been seen until the 13th, 16th and 17th. These first appearances were quite doubtful and Wilkes with proper caution did not then enter in his journal reports of certainly discovered land. When he arrived at Sydney, Wilkes at once announced

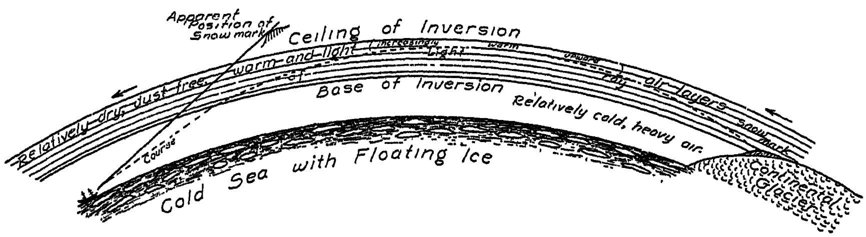


FIG. 2. Diagram to show the nature of looming or polar (superior) mirage, especially common off the borders of large ice masses. The inversion air layer is usually above 1000 meters of altitude, and has been warmed adiabatically in descending the glacier surface. Objects are seen at very considerable distances only, usually in excess of 200 statute miles, and within a relatively narrow ring centered on the object seen. (From *Geogr. Rev.*, October, 1932.)

(Sydney *Herald* of March 13, 1840) "land was first seen on the morning of the 19th January, in latitude 64° 20′ south (a misprint corected the following day to 66° 20′), longitude 144° 18′ east. . . . The Vincennes . . . completed the discovery, and ran down the coast from 154° 18′ to 97° 45′ east longitude, about seventeen hundred miles, within a short distance of the land, often so near as to get soundings with two fathoms of line".

Later, when Wilkes had learned of the discovery of the Balleny Islands he was more inclined to think the earlier "appearances" real. The "appearance" of the 13th was probably one of

[1] W. H. Hobbs, "Visibility and the Discovery of Polar Lands," *Geografiska Annaler*, 1933, Hefts 2 and 3, pp. 217–224, figs. 1–6. Also, "Conditions of Exceptional Visibility within High Latitudes, Particularly as a Result of Superior Mirage," *Annals Assoc. Amer. Geogr.*, Vol. XXVII, Dec. 1937, No. 4, pp. 229–240, figs. 1–8.

the Balleny Islands, volcanoes which had been discovered the year before by the British skipper, John Balleny, who in the same year saw land at a point far to the westward in longitude 116° E.

Wilkes's first landfall which he felt sure was not an island was made on January 19th, and this was named Cape Hudson after his second-in-command. Antarctic land was seen on the same day but ten hours later by a French exploring expedition and was named Adelie Land. This landfall was about 350 nautical miles farther to the west and was made by Captain D'Urville.[2] So close did the American and French expeditions come to making simultaneous landfalls in this sector of the Antarctic. According to the French captain's report he made his discovery in advance of Wilkes by a few hours, but as was afterwards to be shown [3] he had failed to add a day to his log when he crossed the 180th meridian, and this made him later by about ten hours.

From this first definite Antarctic landfall of Wilkes made on January 19th, 1840, the cruise was continued to the westward, sometimes in terrific storms, but during better weather getting views of the land behind the fringing shore-ice and entering it upon the map at frequent intervals. The coast thus skirted extended through a stretch of at least 54 degrees of longitude or about 1500 miles, an expanse sufficient to indicate its continental character. Neither Wilkes nor anyone else was able to set foot upon Antarctic land for another sixty years, so efficient is the fringing shore-ice as a defense set up against the explorer. Even today the land has been reached directly from ships at a few points only, and the latest modern technique for other localities has been to fly over the barrier from the ship to the land.

That Wilkes was in reality dealing with a land area and not with a floating ice formation, he was able to show, not alone by its obvious altitude and its hummocky surface, but at one locality (Piners' Bay) by the observation of rock masses beneath the ice. Still further, he proved the presence of a continental shelf such as surrounds the continents by his soundings to bot-

[2] M. J. Dumont D'Urville, *Voyage au Pôle Sud et dans l'Océanie sur les Corvettes l'Astrolabe et la Zélée*, Paris, 1844, Vol. VIII, pp. 176–177.

[3] John E. Pillsbury, Admiral U. S. N., "Wilkes' and D'Urville's Discoveries in Wilkes Land," *Natl. Geogr. Mag.*, Vol. XXI, 1910, pp. 171–173. Also in *Proc. U. S. Naval Inst.*, Vol. XXXVI, 1910, pp. 465–468.

tom in shallow depths at a number of localities, one of these of 30 fathoms only.[4]

Near the western limit of the cruise the American ships were forced northwestward by a great tongue—a form of floating ice then quite unknown, but now described as shelf-ice. It rose from 80 to 100 feet above the sea and extended 180 miles out from the shore, which as since determined continues westward at the back close to the Antarctic Circle. Wilkes named it all Termination Land. It was not until the twentieth century that shelf-ice was

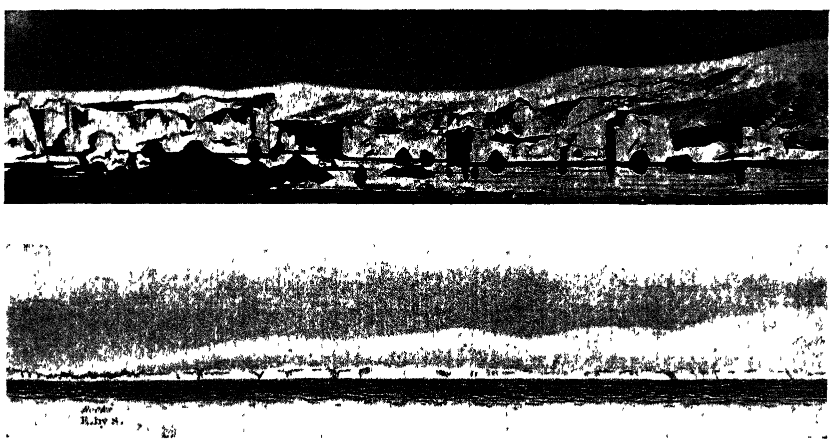


FIG. 3. Sketches of the same stretch of coast of the Antarctic continent by D'Urville (above) and Wilkes (below). Wilkes's sketch made at the head of his Piners' Bay is reproduced from his "Chart of the Antarctic Continent", while D'Urville's sketch made from a closer position, *Pointe Géologie*, is from the *Atlas Hydrographique*, C. A. Vincendon-Dumoulin, Paris, 1847, Pl. XXXV, accompanying D'Urville's "Voyage au Pôle Sud". Note the correspondence of the rock exposures under the ice. (*Geogr. Rev.*, Oct. 1932, p. 637.)

proven to rest on water and not on land. In his honor Termination Land was renamed Termination Ice Tongue, though this was later changed to Shackleton Ice Shelf.[5]

At Termination Ice Tongue Wilkes completed his Antarctic cruise and turned back toward Sydney on February 23rd, 1840, just a century ago today. In summary he wrote:

The land, covered with snow, was distinctly seen in many places, and between them such appearances as to leave little or no doubt in my mind

[4] W. H. Hobbs, "Wilkes Land Rediscovered", *Geogr. Rev.*, Vol. XXII, No. 4, Oct. 1932, pp. 632–655, folding map.

[5] John K. Davis, *With the "Aurora" in the Antarctic 1911–1914*, London, 1919, p. 164. Douglas Mawson, *The Home of the Blizzard*, Vol. II, p. 48.

of its being a continuous line of coast, and deserving the name bestowed upon it, of the *Antarctic Continent,* lying as it does under that circle.[6]

In his Narrative Report Wilkes wrote (Vol. II, p. 335):

> Along the Antarctic Continent for the whole distance explored, which is upwards of fifteen hundred miles, no open strait is found. The coast, where the ice permitted approach, was found enveloped with a perpendicular barrier, in some cases unbroken for fifty miles. If there was only a chain of islands, the outline of the ice would undoubtedly be of another form; and it is scarcely to be conceived that so long a chain could extend so nearly in the same parallel of latitude. . . . The formation of the coast is different from what would probably be found near islands, soundings being obtained in comparatively shoal water; and the colour of the water also indicates that it is not like other southern lands, abrupt and precipitous. This cause is sufficient to retain the huge masses of ice, by their being attached by their lower surfaces instead of their sides only.

In American atlases this stretch of Antarctic coast was long entered as "Antarctic continent", but it is now generally known as Wilkes Land.

Until recently it was not known when or by whom the name Wilkes Land was first applied to this coast of the Antarctic continent. Dr. Edwin Swift Balch, distinguished student of Antarctic exploration and a former president of this Society, on a trip to Europe especially investigated the matter by study of map collections. He was able to trace the use of the name as far back only as Stieler's Atlas of 1866.[7] Thanks to the remarkable collection of atlases brought together by Professor L. C. Karpinski of the University of Michigan, the writer was able in 1932 to trace the name to a map of 1841, a year only after the discoveries had been made, and a year before the expedition had returned to the United States. The name was given, not by American, but by German geographers. It appeared first in an atlas published by the Geographical Institute of Weimar, and the map is dated 1841. Three years later it appeared in Meyer's Great Reference Atlas, edition of 1844.[8] The Karpinski atlas collection is now included in the collections at Yale University.

[6] Charles Wilkes, Esq., Synopsis of the Cruise of the U. S. Exploring Expedition during the Years 1838, '39, '40, '41, and '42; delivered before the National Institute, by its commander, on the 20th of June, 1842, Washington, 1842, pp. 1–56, folding map (p. 20).

[7] Balch, *Antarctica,* 1902, p. 164.

[8] W. H. Hobbs, *Wilkes Land Revisited,* 1932, pp. 649–651.

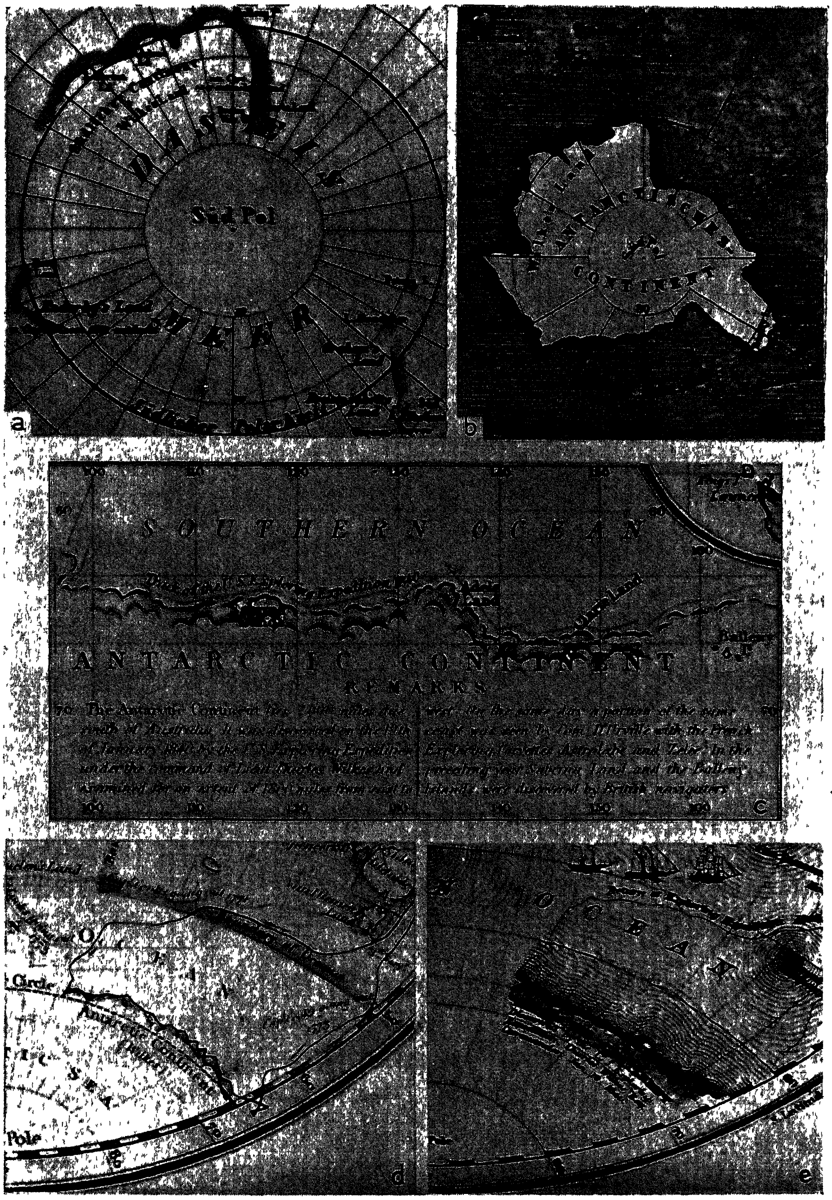


FIG. 4. Reproductions of portions of maps from German and American atlases of the 1840's and 1850's on which the Antarctic continent is represented. (*a*) The Southern Hemisphere, *Atlas of the Geographical Institute of Weimar*. The map is dated 1841 (Karpinski Atlas Collection in Yale University Library). (*b*) The Southern Hemisphere, Meyer's *Great Reference Atlas*, edition of 1844 (Karpinski Atlas Collection). (*c*) The Pacific Ocean including Oceana, Mitchell's *New Universal Atlas*, editions of 1852 and 1854. (*d*) The World on a Globular Projection, Mitchell's *Atlas of 1854*. Map dated 1850. (*e*) Olney's *School Atlas* of 1844. Same map in Smith's *Atlas of 1843*. (From *Geogr. Rev.*, October, 1932.)

Soon after the first publication of the discoveries by Wilkes, they were sharply challenged by British explorers and geographers. Sir James Clark Ross in command of a British Antarctic expedition which reached the Antarctic only a year after the American and French Antarctic expeditions had set out upon the return, discovered South Victoria Land in a sector of the continent which adjoins on the east the Wilkes Land coast. Wilkes had sent to Ross a tracing of the map of his discoveries, and on this he had included the doubtful appearance of land on January 13th as reported by Captain Ringgold of the *Porpoise*. Ross, a veteran explorer of the Arctic, forestalled in discoveries in his published report charged Wilkes with improper motives in going to the Antarctic in advance of him, though the American expedition had been announced in London in 1836, whereas the British expedition had not been planned until 1838.[9] Ross asserted that there was really no Antarctic continent, that Wilkes's landfalls were "of inconsiderable extent, of somewhat uncertain determination, and with wide channels between them." He sailed over the doubtful patch of land represented on the Wilkes preliminary map tracing, and to discredit Wilkes he published this map with the track of his vessel at a time when the official map by Wilkes without this doubtful patch had already been in his hands.

This challenge by Ross was the starting point of a series of attacks launched against Wilkes in England, and these have continued to the present day, though the Royal Geographical Society had in 1845 conferred upon Wilkes its much prized Founders Gold Medal in recognition of his Antarctic discoveries and his publication of the observations.

In his article on the "Polar Regions" in the ninth edition of the Encyclopedia Britannica (1885), Sir Clements Markham, President of the Royal Geographical Society, wrote that Wilkes had mapped "a large tract of land in the latitude of the Antarctic circle for which he claimed the discovery. But as a portion of it had already been seen by Balleny, and the rest had been proved not to exist, the claim has not been admitted."[10]

Thereafter, nearly every British explorer who, with the re-

[9] Captain Sir James Clark Ross, R. N., *A Voyage of Discovery and Research in the Southern and Antarctic Regions, during the Years 1839–43*, Vol. I, London, 1847, pp. 116–117, and appendix 6. See also Hobbs, *Wilkes Land, etc.*, pp. 639–640.

[10] Vol. XIX, p. 330.

newal of Antarctic exploration at the beginning of the Twentieth Century, went out to the Antarctic, made definite attempts to discredit Wilkes and so far as the doubtful appearances of land were concerned, with success. Thus Borchgrevink (1901–1902) sailed in the *Southern Cross* over the same doubtful patch of land on the map tracing and labelled it "Track of S. Y. 'Southern Cross' over Wilkes' Land." A little later Captain Pennell of the *Discovery* on Captain Scott's First Antarctic Expedition with favoring ice conditions sailed to the south of other doubtful appearances of land observed by Wilkes's expedition before January 19 (Ringgold's Knoll, Eld's and Reynold's Peaks) and near enough to their charted positions to show that they did not exist at least at the places where represented. He then turned northwestward, and sailing between Reynold's Peak and Cape Hudson was able to show that Cape Hudson also could not exist where it had been represented upon the map.[11] Scott set down in his narrative ". . . thus once and for all we have definitely disposed of Wilkes Land."

All but one of these observations, it will be observed, had reference to the doubtful landfalls of the Wilkes expedition previous to the discovery of Cape Hudson, though the latest observation of Pennell threw discredit upon that as well.

Alone of the British explorers Mawson made observations to the west of Cape Hudson, and his observations have therefore special pertinence to the main discoveries by Wilkes.[12] Mawson was at one point able to sail over a salient represented upon Wilkes's map of the continent (Cape Carr), and further to show that most of the other landfalls were not accurately placed upon the map; they were almost invariably too far to the north and they were not always in the same longitudes as his own, which were likewise separated by gaps which depended upon the visibility on different days of the cruise. Mawson claimed that most of the discoveries by Wilkes were not genuine, and he removed

[11] C. E. Borchgrevink, *First on the Antarctic Continent*, London, 1901, second chart at end of volume. Scott, *Voyage of the "Discovery,"* Vol. II, p. 392.

[12] Sir Douglas Mawson, D.Sc., B. E., *The Home of the Blizzard*, Vol. I, pp. 9–10. Further: The Unveiling of Antarctica (Presidential Address), Rept. Melbourne Meeting of the Australian and New Zealand Association for the Advancement of Science, July, 1935, pp. 1–37, maps 1–10; and "Wilkes' Antarctic Landfalls," *Proc. Royal Geographical Society*, South Australian Branch, Session 1932–33, pp. 69–113. See also, Frank E. Ross, "The Antarctic Explorations of Lieutenant Charles Wilkes," *ibid.*, Session 1933–34 (Vol. XXV, 1935), pp. 130–141.

the name Wilkes Land from the fifteen-hundred mile stretch of coast. He affixed the name Wilkes Land, however, to a stretch

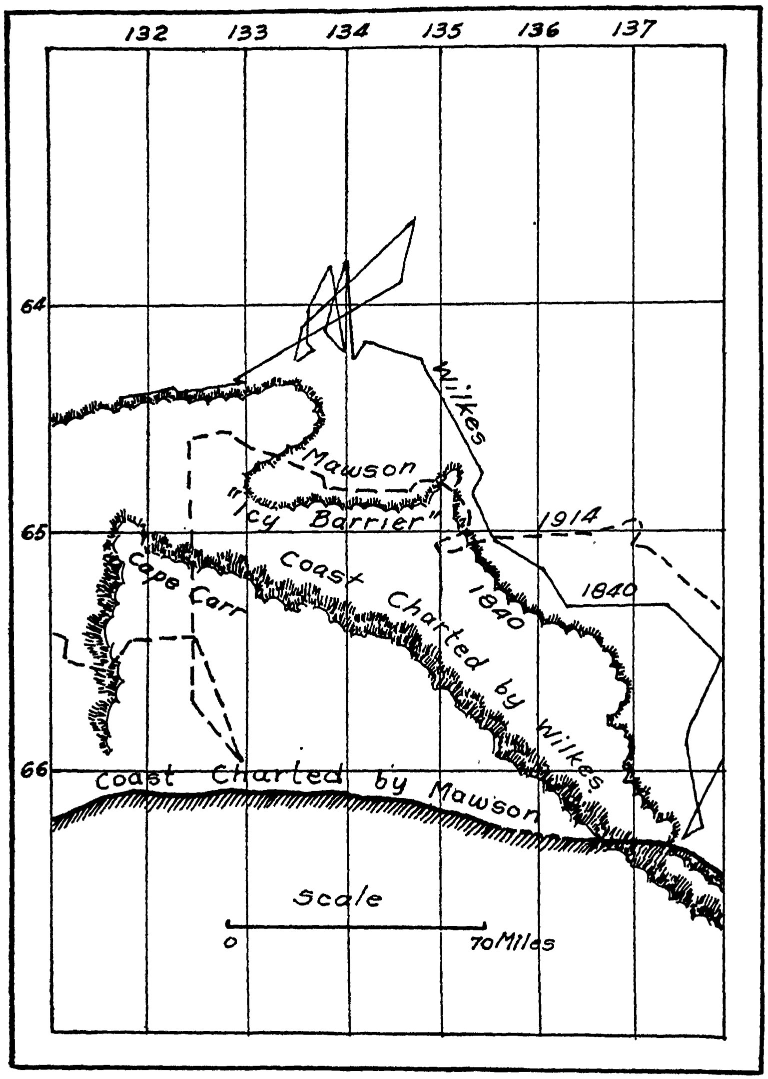


FIG. 5. Map to show the land mapped by Wilkes (Cape Carr) and the correction of its position by Mawson in 1914.

of five degrees of coast near longitude 134° east, which he claimed "had never before been seen" but which Wilkes "believed he had discovered in these seas."

Since Ross's attempt to discredit Wilkes British geographers have largely ignored the Wilkes Antarctic landfalls, and with one or two notable exceptions they have left the name Wilkes Land off their maps. Repudiating the Wilkes discoveries and basing the claim upon Mawson's supposed new discoveries in 1912–1914, the Wilkes Land sector of the Antarctic with exception of the short Adelie Land coast discovered by D'Urville, was in 1933 taken over as a British possession under the name "Australian Dependency". In the report of the Imperial Conference held in London in 1926 there is "a list of Antarctic regions to which a British title exists by virtue of discovery,"[13] and in this list is included the Wilkes Land coast.

Having regard to the conditions of laying down landfalls upon the map on the basis of direction and estimated distance, the remarkable clarity of the air which makes underestimate of distance almost inevitable, and the further fact that Mawson's longitudes were checked by daily radio time signals, whereas Wilkes's could not be; all the differences between the maps of Wilkes and Mawson are easily accounted for without any disparagement of Wilkes's work. Quite on the contrary, Mawson's explorations constitute a splendid verification of the Wilkes discoveries. This is shown by my map on which the two sets of landfalls have been plotted together.[14] When he set out on his expedition, Wilkes was already an experienced map maker and he brought out the first hydrographic charts by the American government—models of their kind.[15]

To reconcile the eastern landfall of Wilkes with later discoveries is not so simple a matter. As we have seen, Pennell of Scott's First Antarctic Expedition showed that the eastern doubtful landfalls of Wilkes, and Cape Hudson as well, could not lie in the positions where he had placed them upon the map. As regards Cape Hudson, this challenge was very serious, for Wilkes had said he felt sure of this landfall, and sketches had been made of it from the *Peacock*. Furthermore, Mawson

13 Summary of proceedings of the Imperial Conference held at London in 1926. This territory was again described in the British Order-in-Council of February 7, 1933.

14 W. H. Hobbs, *Geogr. Rev.*, Vol. XXII, No. 4, Oct. 1932, pp. 632–655, especially folding map of Pl. V.

15 See *The Hydrographic Office of the Navy, One Hundredth Anniversary, 1830–1930*, published by the Hydrographic Office under the authority of the Secretary of the Navy, December 6, 1930, pp. 1–5.

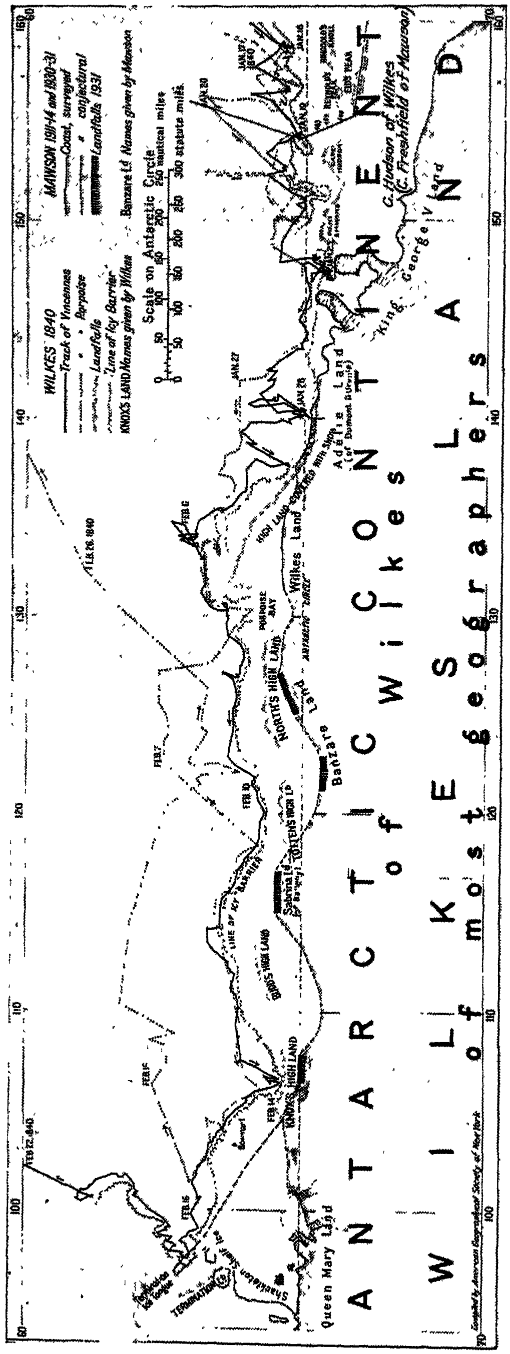


FIG. 6. Map to show comparison of the Wilkes landfalls of 1840 and their confirmation by Mawson in 1911–14. (From *Geogr. Rev.*, October, 1932.)

brought out that when he sledged over the inland ice to the southward in these longitudes and delineated the coast with its salient of Cape Freshfield, he could look off in good visibility and see that no land was in sight off Cape Freshfield, located two hundred miles to the south southwest of Cape Hudson.[16] If Wilkes had seen land in the direction indicated, the position fixed upon his map could not be accounted for by any exceptional clarity of the atmosphere. It might, however, by polar mirage under favoring conditions, and the sketches which were made from the *Peacock* strongly favored this view. The future was to bring a very remarkable verification.

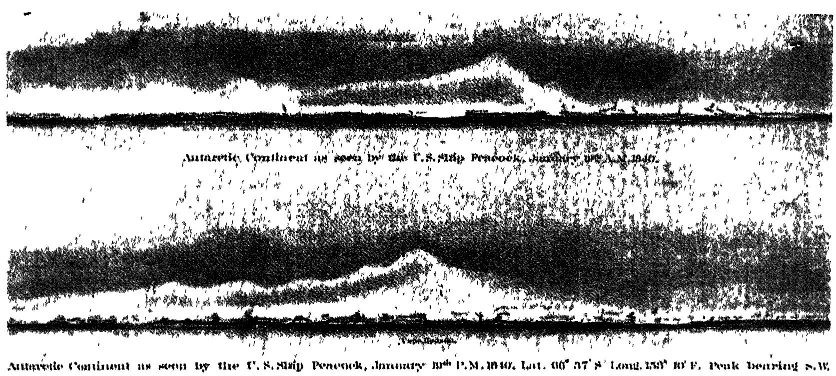


FIG. 7. Sketches of Cape Hudson made from the *Peacock* of Wilkes's squadron in the morning (above) and in the afternoon (below) of January 19, 1840. From Wilkes's "Chart of the Antarctic Continent" in the atlas which accompanies the "Narrative". (From *Geogr. Rev.*, October, 1932.)

In the year 1915 the British expedition ship *Aurora,* Captain J. R. Stenhouse, had become beset near its base on McMurdo Sound and, held fast in the ice, it had drifted in the pack during many weary months until on November 23rd it was a few miles only from the place where Cape Hudson had been sighted and sketched from the *Peacock* three quarters of a century earlier. When the ship had reached this position, a startling phenomenon was observed. Off to the southward—the exact direction could not at the moment be fixed because the ship's compasses had been thrown out of adjustment—land was seen with great clearness, *although none had been observed at any time throughout*

[16] Personal communication to the writer.

the long drift when the ship had been so much nearer to the land. Stenhouse entered in his journal:

November 23.—At 3 a.m. Young Island, Balleny Group, was seen bearing north 54° east (true). The island, which showed up clearly on the horizon, under a heavy stratus-covered sky, appeared to be very far distant. By latitude at noon we are in 66° 26′ S. As this is the charted latitude of Peak Foreman, Young Island, the bearing does not agree. Land was seen at 8 a.m. bearing south 60° west (true). This, which would appear to be Cape Hudson, loomed up through the mists in the form of a high, bold headland, with low undulating lands stretching away to the south-south-east and to the westward of it. The appearance of this headland has been foretold for the last two days, by masses of black fog, but it seems strange that land so high should not have been seen before, as there is little change in the atmospheric conditions.

November 24.—Overcast and hazy during forenoon. Cloudy, clear, and fine in afternoon and evening. Not a vestige of land can be seen, so Cape Hudson is really "Cape Flyaway". This is most weird. All hands saw the headland to the southwest, and some of us sketched it. Now (afternoon), although the sky is beautifully clear to the south-west, nothing can be seen. We cannot have drifted far from yesterday's position. No wonder Wilkes reported land.[17]

There can be no doubt that this view is to be explained by polar mirage, since the land was so far away. Confirmation is found in the fact that the *Aurora* was so near to the position of the *Peacock* three-quarters of a century earlier, for in the case of any given object a mirage can be observed under similar conditions only within a quite limited belt.

When this incident came to the writer's attention in 1932, he set out to determine whether one of the sketches made from the *Aurora* might not be brought to light and compared with the sketch made from the *Peacock.* Stenhouse reported that his own sketch had been lost, yet so vivid was his recollection that he was able to make one roughly from memory. Later, when he was with the writer in London, the quest was again taken up and a sketch was found which had been made at the time the land was in sight on November 23rd, 1915, by A. H. Ninnis, one of Stenhouse's own men on the *Aurora.* When this was compared with the sketch from the *Peacock* of the Wilkes expedition, it was seen that the two are as nearly equivalent as two sketches would be likely to be if made by different persons sketching the same

[17] Sir Ernest Shackleton, C. V. O., *South, The Story of Shackleton's Last Expedition of 1914–1917,* London, 1919, p. 326.

object at the same time and from the same position. Most fortuitously, upon the same page of the Ninnis sketchbook was found a sketch of Young Island, which had been seen and recognized at the same time. Now since this volcanic island 2000 feet in altitude is the westernmost of the Balleny group and lies about 230 statute miles away to the eastward, it gave the approximate distance at which mirage would reveal objects at the time. The high ground above Cape Freshfield of Mawson's map was at about the same distance. Quite as important, this sketch supplied a correction to the compasses of the *Aurora,* for Young

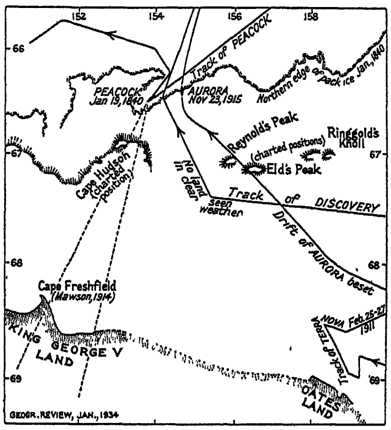


FIG. 8. Map to show the tracks of various exploring vessels in the seas east of Cape Hudson. From the *Peacock* on January 19, 1840 and from the *Aurora* on November 23d, 1915, in near the same position, the high land above Cape Freshfield was seen and sketched from a distance of about 230 statute miles due to polar mirage. It is likely, but not proven that earlier "appearances" of land from the Wilkes ships are explained in the same way. (From *Geogr. Rev.*, January, 1934.)

Island was known to be in the same latitude as the *Aurora's* position. With this correction the line of vision in both instances prolonged past the position of Cape Hudson on Wilkes's map intersected the high land above Cape Freshfield. The evidence seems to be complete and we may now conclude that Cape Hudson seen by Wilkes in 1840 is the salient of Cape Freshfield later mapped by Mawson, and that the distance is about 230 statute miles, instead of about 30 miles as Wilkes thought at the time.[18]

Further to bring out the importance of taking account of polar mirage in Antarctic exploration within areas both east and

[18] W. H. Hobbs, "Discovery of a New Sketch of Cape Hudson in the Antarctic", *Geogr. Rev.*, Vol. XXIV, No. 1, Jan. 1934, pp. 115–117, figs. 1–2.

west of Wilkes Land, the two sharpest critics of Wilkes, Ross and Mawson, each had their errors in mapping later accounted for through exceptional visibility conditions, Antarctic air clarity and mirage.[19]

Mawson in the southern summer of 1829–30 discovered and roughly mapped some 500 miles of new Antarctic coast lying to the westward of Wilkes Land. In the following season he returned to the same coast and under better ice conditions he got

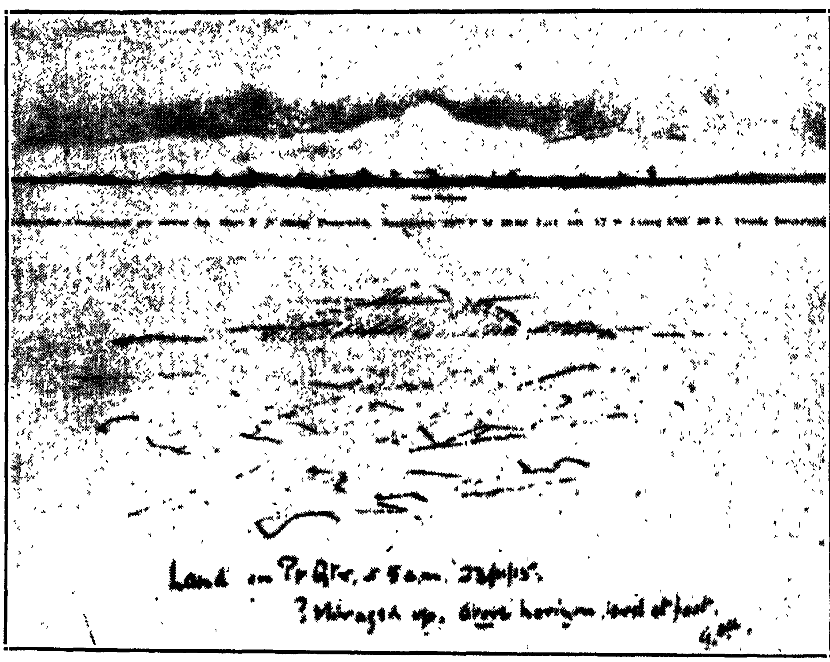


FIG. 9. Comparison of the sketches of the high land above Cape Freshfield made from near the same point (above) on the *Peacock* in 1840 and (below) on the *Aurora* in 1915. The distance is about 230 statute miles. Cape Freshfield is the Cape Hudson of Wilkes. (From *Geogr. Rev.*, October, 1932.)

much nearer to the land and found that during his first season he had mapped a coast at a distance of from 150 to 200 miles. Without resort to mirage this may perhaps be explained through the exceptionally clear air of the Antarctic, for Mawson had ascended from his ship in a small seaplane to heights at which the rotundity of the earth may not have imposed a bar to his

[19] W. H. Hobbs, "Conditions of Exceptionable Visibility, etc.," *Ann. Assoc. Amer. Geogr.*, Dec. 1937, pp. 233–239.

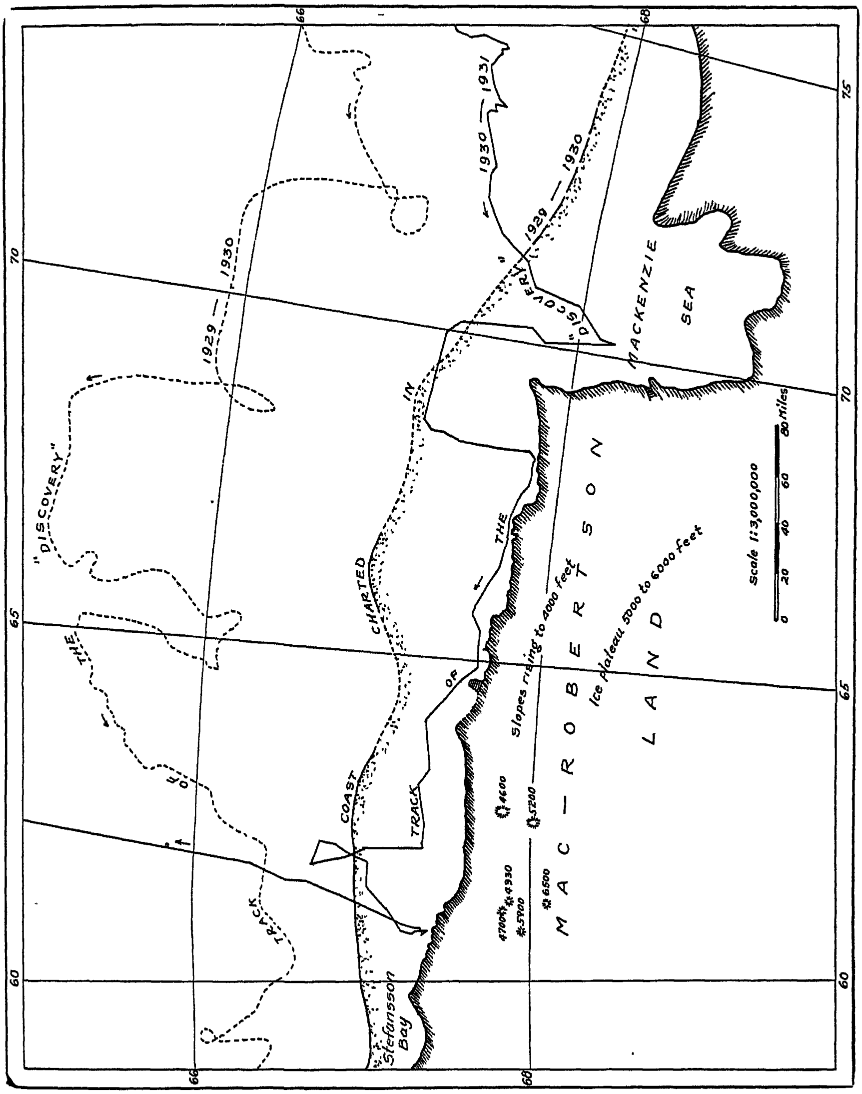


FIG. 10. Comparison of maps of MacRobertson Land made by Mawson in 1929–1930 and in 1930–1931. The two maps have been reduced to the same scale and superimposed.

views of the coast.[20] In his second cruise Mawson sailed over land which he had mapped the year before, not for a distance of some 80 miles, as he had done over the Cape Carr salient put upon the map by Wilkes, *but over his own discovered land for a distance of some 400 miles.*

Ross's error more resembled that of Wilkes in underestimating the distance of Cape Hudson because of the mirage, but also in taking shelf-ice for land, as Wilkes did at his "Termination Land". When in 1841 Ross was off the shelf-ice near McMurdo Sound, he saw a lofty range of mountains at an apparent distance of about 30 miles, and he placed the range upon his map as the "Parry Mts."[21] Sixty years later his compatriot Captain Robert Falcon Scott visited the area and found no mountains at all, but only floating shelf-ice.[22] Ross and his companions had unquestionably seen a range of lofty mountains along the same direction, but across some 275 miles of shelf-ice instead of 30 miles as they supposed.[23]

The examples of polar mirage here given are not so unusual as might be supposed. I could cite from my own experiences in Greenland, but I prefer to mention that Captain Robert Bartlett, famous Arctic explorer who is on the program of this meeting, has in forty years of Arctic experience seen many examples of polar mirage, the most remarkable of all in the summer of 1939 when cruising to the eastward of Greenland in his exploring ship *Morrissey.* From a point midway between Cape Farewell and Iceland he and his ship companions saw crystal clear the lofty snow-covered Snaefells Jökull in southwestern Iceland at a distance of 335–350 statute miles.[24]

In summary, with the exception of the doubtful appearances of land seen at the beginning of his cruise in longitudes to the

[20] Douglas Mawson, "The Antarctic Cruise of the 'Discovery,' 1929–30," *Geogr. Rev.*, Vol. XX, No. 4, Oct. 1930, pp. 535–554, figs. 1–12, folded map. Also, "The B. A. N. Z. Antarctic Research Expedition, 1929–31," *Geogr. Jour.*, Vol. XXX, No. 2, Aug. 1932, pp. 101–131, plates and folded map. See also Hjalmar Riiser-Larsen, "The 'Norvegia' Antarctic Expedition of 1929–1930," *Geogr. Rev.*, pp. 555–572, figs. 1–18, folding map. See also W. H. Hobbs, "Conditions of Exceptionable Visibility within High Latitudes, Particularly as a Result of Superior Mirage", *Ann. Assoc. Am. Geogr.*, Vol. XXVII, Dec. 1937, No. 4, pp. 237–239.

[21] James Clark Ross, *A Voyage of Discovery and Research, etc.*, Vol. I, pp. 218–219.

[22] R. F. Scott, *Voyage of the Discovery*, Vol. I, pp. 161, 170–171.

[23] Hobbs, *Conditions of Exceptionable Visibility*, p. 234, fig. 3.

[24] W. H. Hobbs, "A Remarkable Case of Polar Mirage," *Science*, Dec. 1, 1939, pp. 513–514.

east of 154° E., and in mistaking the off-shore high shelf-ice to the westward of 100° E. for the land behind it, the Antarctic discoveries of Wilkes have now been confirmed. The large map which accompanied my monograph of 1932 on "Wilkes Land Re-

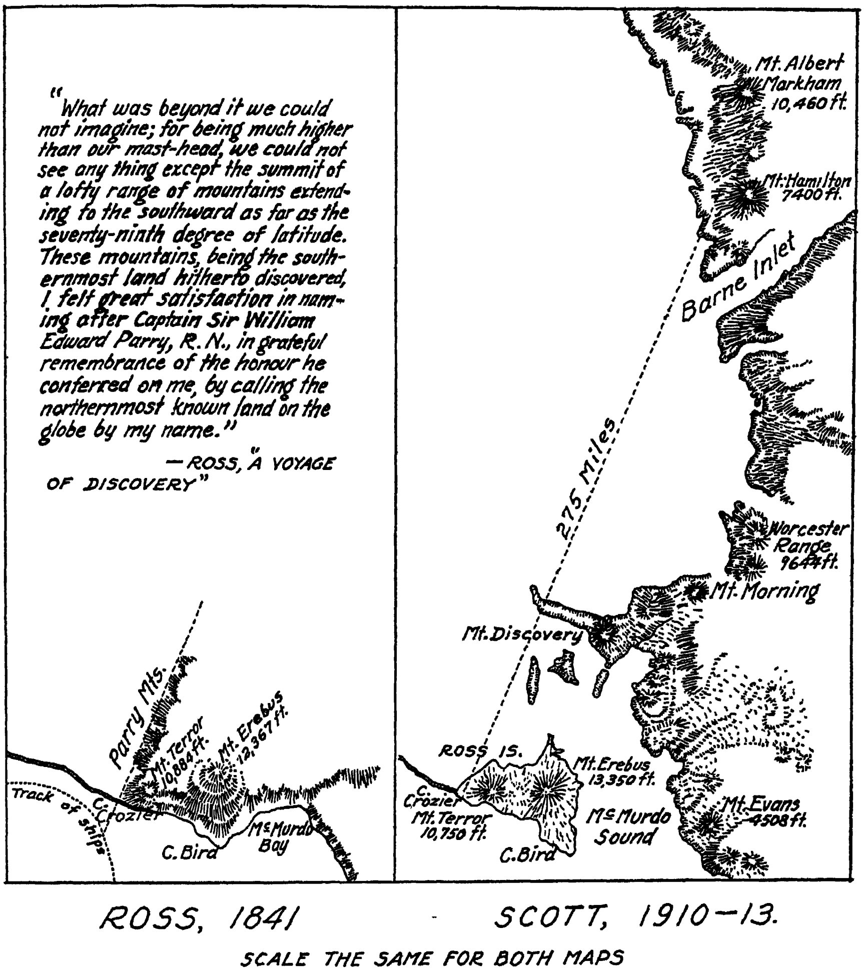
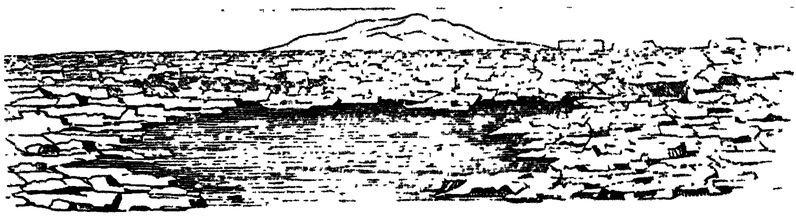


FIG. 11. Comparison of the map by Ross of the "Parry Mountains" in 1841 and that of Scott in 1910–13. Lines have been added to show that the "Parry Mountains" seen by Ross must have been a range at a distance of about 275 statute miles.

discovered" on which all landfalls within this extended coast have been plotted together, shows how well Mawson's explorations have confirmed Wilkes's discoveries three-quarters of a century earlier.

Even the doubtful "appearances" of land to the eastward of Cape Hudson, and especially Ringgold's Knoll, may perhaps be explained by mirage in the same way as Cape Hudson itself; for salients of the Antarctic coast (Oates Land and King George Land) have since been discovered in positions along the extension of sights to these doubtful landfalls. The line of sight extended over the position of Ringgold's Knoll on Wilkes's chart intersects a sharp salient of the Oates Land coast (see Fig. 7). However, this does not constitute a proof.

Outside of Britain, and France also for the earlier period, the recognition of Wilkes's discoveries has been very general, and some British geographers, particularly the more distinguished, have not hesitated to place themselves in opposition to the official



SKETCH OF LAND AND FIELD-ICE.

FIG. 12. Sketch by Wilkes of Ringgold's Knoll made January 16th, 1840, from the *Vincennes*. Reproduced from Wilkes's "Narrative" (Vol. II, p. 294). (From *Geogr. Rev.*, October, 1932.)

British attitude.[25] After the appearance of Mawson's reports in 1914 a few standard atlases other than British in origin reduced Wilkes Land to Mawson's five degrees of coast line.[26] Most reference works, however, continue to use Wilkes Land for the

[25] Thus: Sir John Murray (*Geogr. Jour.*, Vol. III, Nov. 27, 1893, map); J. A. Bartholomew and A. J. Herbertson (*Atlas of Meteorol.*, 1899); Sir John Scott Keltie (map in London *Graphic* of August 10, 1901); Dr. H. R. Mill (*Siege, etc.*, 1905, pp. 247–248 and map at end, also article "Polar Regions" in 11th Ed. *Cyclopædia Brittanica*, 1911); and Dr. R. N. Rudmose Brown ("The Polar Regions," 1927, p. 25).

[26] Thus: Andree's *Handatlas*, ed. of 1921, for the Antarctic regions, though on the World Map the old usage was retained and later return was made to earlier practice (see below); a small Swedish atlas (Svensk *Varldsatlas*, Stockholm 1930, Pl. 7); Vivien de Ste. Martin et Schrader, *Atlas Universel de Géographie* (no Wilkes Ld.); and E. Debes, *Grosse* (Columbus) *Handatlas*, 1937. Likewise, the *Atlante Universale*, Bergamo, 1927 (Pl. LXXII), *Great Soviet World Atlas*, 1937, and Zimmerman (*Géographie Universelle*, Vol. X, 1930, p. 342). The American atlases of Gram and Hammond also followed Mawson. Rand McNally in the 1939 and 1940 editions of their atlas restricted Wilkes Land to five degrees of coast, but are returning to their earlier practice in the next edition now in preparation.

major part at least of the coast as laid down by Wilkes, and this has been notably true of those which have appeared within the last two decades.[27] Now in 1939 the Australian Commonwealth has accepted the broader coastline.[28]

[27] *British:* Keith Johnston, *Royal Atlas,* 1887; Bartholomew and Herbertson, *Atlas of Meteorol.,* London and Edin., 1899; John Scott Keltie, *London Graphic,* Aug. 10, 1901; *The Harmsworth Atlas and Gazetteer of the World,* London, 1907 (90°–155° E.); Dept. of External Affairs, Australia, *Map of the Antarctic,* 1939 (102°–136° E.).

French: *Larouse du XXe Siècle* (Encyclopaedia), 1933, Vol. VI, p. 648, map; Grandidier, *Atlas des Colonies Françaises,* Paris, 1934 (89°–151° E.); Vidal de Lablache, *Atlas,* 1938.

German: Berghaus, *Atlas der Geologie,* Perthes, Gotha, 1892 (100°–155° E.), Fricker, 1900 (*Antarctic Regions,* map. opp. p. 1), Neumayer, 1901 (*Auf zum Sudpol,* map at end); Debes, *Neuer Handatlas neben alle Teile der Erde,* 1905 (102°–142° E.); Meinardus, 1914 ("Aufgabe und Probleme . . . in der Antarktis," *Geog. Zeit.,* Vol. XX, map on p. 21); Filchner, 1923 (*Zum Sechsten Erdteil,* map on p. 3 as of 1910, but in map at end he presents the Mawson view as a revision); Ullsteins *Weltatlas,* Berlin, Velhagen und Klasing (Ambrosius and Frenzel), *Das Bild der Erde,* 1930; v. Drygalski, 1931 (*Antarktis,* map plate XXI); Herders *Welt- und Wirtschaftsatlas,* 1932 (110°–140° E.); Stieler's *Handatlas,* Editions of 1925 and 1932–34 (also Haack's Wall Maps), 1939 (100°–140° E.); *Der Grosse Brockhaus,* 1934 (Vol. XVIII, map opp. p. 328); *Der Grosser Weltatlas,* 1935 (map 1, 115°–135° E., map 2, 115°–150° E.); Andree's *Handatlas,* 1937 (100° E. to 135° E.); Edition of 1938 the same; Knours *Weltatlas,* Berlin, 1939 (90° E. to 135° E.).

Italian: *Atlante Methodica de Geographia Moderna,* Novara, 1921 (102°–155° E.); *Grande Atlante Geografico,* 3rd ed., Novara, 1927; *Atlante Internazionale del Touring Club Italiano,* 3rd ed., Milan, 1929 (Pl. III), also 1938 (95°–140° E.); *Enciclopedia Italiana di Scienze, Lettere ed Arti*—Trecanni, 1929 (Vol. III, map on p. 440); Pennesi and Almagià, *Nuovissimo Atlante di Geografia Fisica e Politica,* Turin, 1931.

Swedish: M. Roth, *Atlas för Skolan,* Stockholm, 1874; Otto Nordenskiöld, 1905 (*Antarctica,* pp. 68–69), Otto Nordenskjöld, 1909 (*Die Polarwelt, etc.,* p. 79); *Nordisk Familjebok, Konversationslexikon och Realencyklopedi,* Stockholm, 1915 (Vol. XXI, p. 1171); *Nordisk Världsatlas,* Stockholm, 1926 (Pls. I and XXXVII); *Världsatlas,* Stockholm, 1934.

Norwegian: O. J. Skattum, 1911 (Sydpolforskning, *Norske Geog. Selskabs Aarbok,* Vol. XXII); Refsdal, *Skole-Atlas,* Oslo, 1925; Bj. Aagaard, 1932 (*Fangst og Forstning i Sydishavet,* Vol. II, map at end); Isaacsen, 1932, *Hvalfangernes Assuranceforeening,* Atlas over Antarktis og Sydishavet, Sanderfjord [1936?]; Christensen, 1938 (*My Last Expedition to the Antarctic 1936–1937,* map inside of cover).

Danish: Knud Rasmussen, 1933 (*Heldenbuch,* map on p. 257); Danish Atlases generally use plates made in Germany and hence follow German practice.

Dutch: W. J. Huberts, *Nieuwe Geograph. Atlas,* Groningen, 1870; J. Kuyper's *Nieuwe Atlas der Wereld,* Amsterdam, 1864; Beekman en Schuiling, *Schoolatlas,* 1894; A. A. Beekman, *Schoolatlas von de Geheele Aarde,* 3rd ed., 1903; Bos-Niermeyer, *Schoolatlas der Gehele Aarde,* Wolters, Groningen, 1936.

Swiss: *Atlas für Schweiz. Mittelschulen,* Eds. 1911 and 1915.

Spanish: *Enciclopedia Universal Ilustrada,* 1930 (Vol. LXX, p. 257).

American: Bradley's *Atlas of the World,* Phila., 1888 (103°–150° E.). Edwin Swift Balch, 1902 (*Antarctica,* map at end); *Century Atlas,* 1913; *New International Cyclopedia,* 2nd ed., 1927 (Vol. I, map on p. 686); Nelson's *Perpetual Loose-Leaf Encylopedia,* Article by W. M. Davis on Antarctic Exploration, 1930; American Geographical Society, Map of the Antarctic in 4-sheets, 1928, and map to accompany

article "Wilkes Land Rediscovered", 1932 (*Geogr. Rev.*, Vol. XXXII, map opp. p. 654); Goode, *School Atlas,* 1932; National Geographic Society, 1922 (*Nat. Geog. Mag.*, Vol. XLII), Map of the Antarctic, 1932 (Vol. LXII, October), Map of the World, 1935 (Vol. LXV); Lincoln Ellsworth, 1939 (*Nat. Geog. Mag.*, vol. LXIX); Rand McNally, *Premier Atlas,* 1915; *Intern. Atlas of the World,* 1925.

By a ruling of the United States Geographic Board all official Government maps on which the Australian sector of the Antarctic continent is represented show Wilkes Land extending between the longitudes of 96° E. and 155° E.—59 degrees including Cape Hudson at the east and Termination Land at the west (Decision of November 6, 1912, reaffirmed in Sixth Report, 1890–1932, Washington, 1933, p. 818).

[28] The Commonwealth government have assigned the following limits in longitude to the "Lands" in the Australian Antarctic Territory: . . . Wilkes Land, 102°–136° E. (*Geog. Jour.*, Vol. XCIV, No. 3, Sept. 1939, p. 207).

THE CONTRIBUTIONS OF WILKES TO TERRESTRIAL MAGNETISM, GRAVITY AND METEOROLOGY

COMMANDER F. W. REICHELDERFER

Chief, United States Weather Bureau

(Read February 23, 1940, in Centenary Celebration of the Wilkes Exploring Expedition, 1838–1842)

Introduction.—Tables of meteorological observations afford but little information and give a very indefinite idea of the changes that have taken place in the natural phenomena to which they relate, even to those who are familiar with them; they require some elucidation to bring the results at once under the eye, and to the mind: in order to effect this object, a series of illustrations have been constructed, which exhibit all the results of the observations of the Exploring Expedition, at sea as well as in port.

With the foregoing introductory paragraph, Charles Wilkes began the volume [1] in which he published in 1851 the meteorological work of the United States Exploring Expedition, 1838–1842. The quotation is an appropriate introduction to this review of Wilkes' contributions to terrestrial magnetism, gravity and meteorology. In this short paper the voluminous material which the Wilkes Expedition collected in these fields during its forty-six months of exploration can best be suggested by a few illustrations, and the scientific results which came after many years' study of data collected during the cruise can be presented only briefly. The most careful selection of illustrations can scarcely do justice to the vision of the men who, more than a century ago, planned this expedition in the interest of commerce and science, and no brief recital can pay due tribute to the patience, industry, courage and determination which carried the work to fruition years after completion of the Expedition.

Other speakers here have described the aims, organization and route of the Expedition, and have mentioned the dangers, hardships and intrigues which it encountered at home and

[1] *United States Exploring Expedition, 1838 to 1842*, Vol. XI, Meteorology, by Charles Wilkes, U. S. N., member of the American Philosophical Society, etc., pp. v and vi. (Printed by C. Sherman, Philadelphia, from copy of limited edition sent to Prof. Louis Agassiz by Charles Wilkes, now in Harvard University Library.)

abroad. It is my purpose to review briefly what is known of the instruments used for observations in geo-magnetism, gravity and meteorology, to indicate the observational procedures which were followed, and to summarize the most outstanding results in these fields. I shall necessarily say little about geo-magnetism and gravity, fields which should properly be left to the specialists who have spent years in the effort to collect and analyze the data planned by Wilkes for publication in Volume XXIV, Physics.

The official appointment of Wilkes as Commander of the Expedition was dated March 20, 1838. The squadron sailed from Hampton Roads, Va., on August 18. In this period of less than five months it took almost heroic measures for the vessels to be made ready for the long cruise and for the commander to perfect his plans and organization and to assemble personnel and equipment. Wilkes set about the task with characteristic determination and thoroughness. Much of the instrumental equipment had already been procurred. The meteorological instruments, for example, were obtained in 1836, soon after Congress authorized the Expedition. Some came from Europe, and others, according to Wilkes, were "prepared by Fisher, of Philadelphia, with great care and accuracy." Most of the instruments were selected by Wilkes himself before he was officially assigned as the commander, for he says,[2] "The instruments I was supplied with were procurred by myself in Europe; they were made by the best English, French and German artists. A description of these will be given in the volume on Physics"; and "The magnetical instruments were by both English and French makers. Results have been had throughout the cruise, and will serve to give a magnetic chart of the world; these will be published in the volume on Physics." This volume was never published. If the manuscript was ever completed most of it has been lost. Through the efforts of Dr. George W. Littlehales[3] fragments of the original records were recovered about 1906[4] and deposited in the Hydrographic Office, Navy Department. These have now

[2] *The Narrative of the United States Exploring Expedition*, by Charles Wilkes, Vol. I, pp. xx and xxi; 363, etc. (Published by LEH and Blanchard, Philadelphia 1845.)

[3] "The South Magnetic Pole," by G. W. Littlehales, *Bulletin of the American Geographical Society*, Vol. XLII (1910), No. 1, pp. 1 to 8.

[4] Letter from Dr. G. W. Littlehales to Dr. Wm. Bowie, Washington, D. C., dated July 27, 1939.

been placed among the United States Archives. Search has been made of these original papers, as well as among the Wilkes collection in the Library of Congress,[4a] with the purpose of obtaining detailed descriptions and sketches of the instruments, but nothing important has been discovered beyond the indication that emphasis was placed upon procuring only the most accurate equipment and the fact that frequent reference is made to pendulum observations for gravity and to magnetic compass and dip needle observations for terrestrial magnetism. A list of the instruments procured by Wilkes for the Expedition is contained in the historical records of the American Philosophical Society, but no details or characteristics are given.

Some of the meteorological instruments are briefly described in Volume XI. Standard marine barometers were used aboard ship. For the observatory, which was set up ashore whenever the Expedition remained in port, a barometer, manufactured by Traughton and Simms, London, was used. It is described as an ivory float gauge type with tube diameter 0.35 inch and its error on the Royal Society's standard equal to 0.007. It was used as the standard during the cruise, being read to thousandths of an inch, and was found in "perfect order" upon the return of the Expedition, when it was again compared with standards in Washington. Wilkes gives a table of corrections for the various marine barometers used on board the vessels of the squadron. He refers to the impracticability of applying a correction for capillary attraction, and to his belief that the mercury had been boiled (to free it of air) just before the barometers were filled. His air and water thermometers had 18-inch scales and were graduated on metal and ivory to half degrees. They were easily read to tenths. He also had self-registering spirit thermometers of Six's design. The hygrometers used were those of "Pouillet (a capsule), Daniels, the silver cup, and the wetted bulb". A DeWitt raingage was installed at the end of the spanker-boom, where it was free from the drip of sails or rigging. It consisted of a cone nine inches high with six-inch base, the rain catch being measured "immediately after rain" by a rod graduated to hundredths. There were no wind measuring instruments, the force and direction being obtained in "the usual way by the different

4a Original records in the Historical Files of the Navy Department, the Library of Congress and the U. S. Archives.

observers'' from appearance of rigging, canvas and sea. These comments on instruments are included as an indication of the attention given to quality of observations. Wilkes seems to have been well satisfied with the scientific equipment of the Expedition, although he states, referring to the condition of the vessels in his squadron, that ''we were anything but well equipped for such a cruise,'' an estimate which was later confirmed by the unseaworthy conditions developed in some of his vessels.

Remembering that 1838 was before the days of stenographers and typewriters, the records contain a remarkable number of pages, many in Wilkes' handwriting, covering transactions on everything from supplies of pork, beef, etc., to damage to ship's gear, local police records and arrangements for return passage of surveyed seamen. They include a ship's order curbing the alarming increase of eccentric mustachios among the ship's officers, and another reserving the ship's library for its intended use against the tendency for it to become a writing and recreation room. In keeping with his naval training, Wilkes laid down in concise instructions the procedure for taking scientific observations, and issued general orders requiring all officers to keep journals, which were to be forwarded weekly to the Commander of the Expedition. Mere perfunctory entries were not acceptable. Wilkes expected his officers to display genuine scientific curiosity toward every new thing they encountered, whether it was natural phenomenon, social custom or trade statistic. He writes that the daily journal was a part of their duty ''which I consider as paramount to all others.'' He goes on ''1st. The duties devolving upon all the officers of the Expedition are altogether of a public nature: . . . (The objective) can only be effected by keeping full and complete memoranda of all observations, made at the time, and entered in the journals. . . . The kind of journal required is not a mere copy of the log-board, but it is a diary, in which will be noticed all that relates to public information, being a record of all objects of interest, however small, which may take place during the cruise, in the scientific or any other department: and the views of the officer ought to be briefly expressed concerning things that may come under his notice. . . . A casual memorandum or observation, believed at the time of little importance, may lead to important and satisfactory results. . . . I trust I need not remark that the above

relates entirely to public transactions. With private affairs I have nothing to do: they are, and always should be deemed sacred, and, consequently, will form no part of the records." Wilkes had received instructions from the Secretary of the Navy to "take all occasions, not incompatible with the great purpose of your undertaking, to extend the bounds of science and promote the acquisition of knowledge," and he set about to do this in a most thorough and comprehensive fashion.

In Fig. 1 is shown a transcribed copy of the "General Instructions Relative to Observations."[2] It will be noted that the naval medical officers were assigned the duty of making weather

GENERAL INSTRUCTIONS RELATIVE TO OBSERVATIONS

The surgeon and his assistants will take the meteorological observations at 3 P.M., 9 P.M., 3 A.M., and 9 A.M.; viz, the standing of barometer, thermometer, and hygrometer.

The temperature at the masthead, and that of the water, wind, weather, and the force of the wind, the quantity of rain, &c.; the officers of the watch will note and make any remarks of their own, regarding facts that may have occurred, (during their watch,) in the meteorological journal: all astronomical and atmospherical phenomena, it is desired may claim attention, and be noted under their respective heads. Astronomical phenomena, such as shooting stars, zodiacal lights, aurora borealis, the height of their arcs, their colours, &c., measured and the direction they take in the heavens. Atmospherical phenomena, such as rainbows, halos, waterspouts, lightning, appearance of the clouds, rain, the Magellanic clouds, to be noted when first observed; in short, any unusual appearance connected with the weather.

Of the sea, all phosphorescent lights, fishes, and all substances adhering to weeds, must not fail to claim attention, and specimens of them obtained. Fish caught must be preserved till opened in the presence of an officer, and their stomachs carefully examined, and if any thing is found, it must be taken care of.

Things and animals that might in ordinary cases be deemed troublesome and useless, are not to be lost sight of, but are to be picked up for examination.

Every opportunity of trying the current must be taken advantage of, and marked.

Astronomical observations, viz., lunar distances of the stars, east, and west of the moon, of the sun, and of the planets, to be frequently taken.

Observations for chronometers must be taken daily, mornings and afternoons, when the weather will permit; azimuths and amplitudes, at least once or twice a day, in the morning, or in the afternoon, and the ship's head noted at the same time.

Any of the officers (among whom are considered the scientific gentlemen) will on all occasions promote the objects of the Expedition by procuring any article referred to in the foregoing instructions, or aiding in carrying into effect the same. And the officer of the deck is authorized to stop the ship's way, and perform any evolution with a view of carrying into effect the above, in which case he will report the same immediately to me, if time does not permit his doing so previously.

It is necessary for the sea-officers to make themselves thoroughly acquainted with the heavenly constellations, in order to be efficient in noting the course of meteors, &c.

Charles Wilkes,
Commanding Exploring Expedition.

U. S. Ship Vincennes,
At Sea, August 25th, 1838.

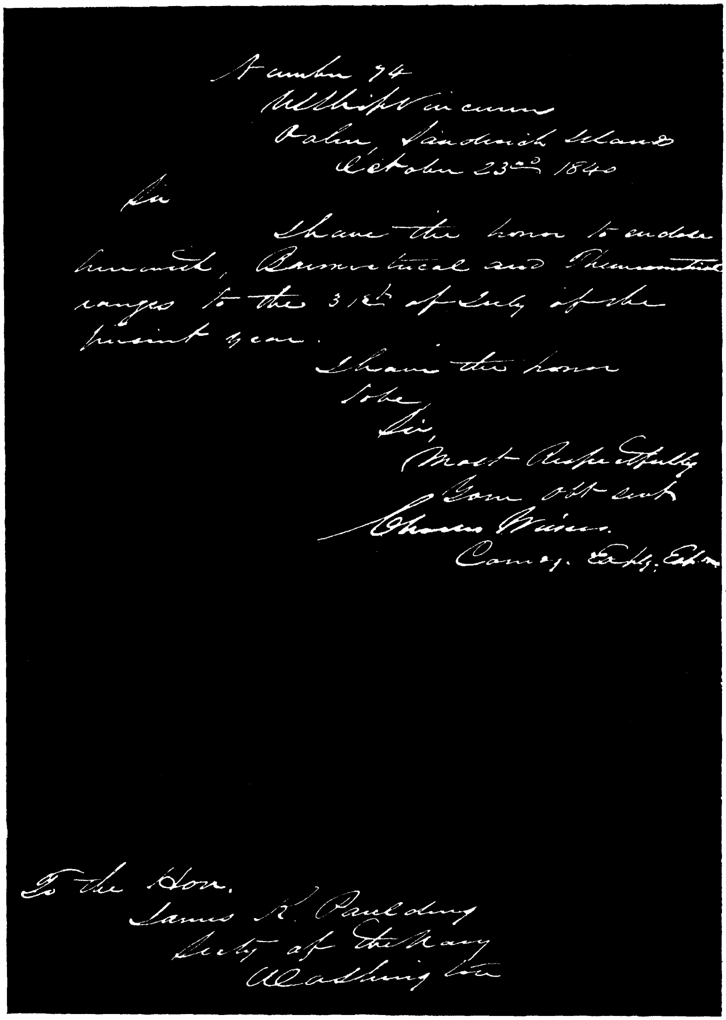
FIG. 1. Transcription of Wilkes' "General Instructions Relative to Observations" from the *Narrative,* Vol. I, p. xx.

observations for the records. Although there was a special staff of scientists, totalling about twelve, attached to the Expedition, none of them was a specialist in the fields of terrestrial magnetism, gravity and meteorology. The observations in these sciences were taken by the regular ship's officers, assisted at times by members of the civilian scientific staff. In his special acknowledgments,[4] Wilkes expresses appreciation for the work of Lieutenant Overton Carr, Midshipmen Henry Eld and Simon Blunt, and Mr. James Howison, captain's clerk, in their observatory duties and pendulum observations, and to Assistant Surgeons J. L. Fox and Silas Holmes for their zeal in the field of natural history as well as attention to their medical duties and meteorological observations.

The weather record began with the departure of the Expedition from Hampton Roads. For purposes of comparison, Wilkes had arranged for meteorological observations at Cambridge, Mass., by Wm. Bond, Astronomer, Harvard College, and at the naval depot, Washington, by Lieut. J. M. Gillis during the entire period covered by the Expedition. The vessels in the squadron recorded their weather observations four times daily (3:00 and 9:00 A.M. and P.M., civil time), and more frequently when conditions were unusual. The temperature at the mast head was taken in connection with these observations every six hours. The temperatures of air and water were taken hourly, at sea and in port. When anchored, thermometers were sunk to get water temperature at different depths and observations of temperature of springs and caves were taken as indication of mean temperature of the climate. A standard tabular form was used, amplified by descriptive remarks in the journals which were kept during the whole cruise.[5] These were assembled by Wilkes and published in summarized and graphical form in Volume XI.[1] To illustrate the extent of the meteorological records, a number of photographic copies are shown here. These include a letter from Wilkes to the Secretary of the Navy transmitting barometric and thermometric data (Fig. 2); a copy of an original meteorological record made on the *U.S.S. Relief* (Fig. 3); three copies of diagrams showing pressure and tem-

[5] Synopsis of the Cruise of the United States Exploring Expedition, etc., delivered before the National Institute by Charles Wilkes, June 20, 1842. (Printed by Peter Force, Washington, 1842.) Pp. 42–43.

perature conditions observed underway on three portions of the cruise, inspection of which indicates that Wilkes' observations contain errors due to difficulties of instrument exposure, a problem which still troubles the modern meteorologist (Figs. 4, 5 and



Number 74
U.S. Ship Vincennes
Oahu, Sandwich Islands
October 23rd 1840

Sir

I have the honor to enclose herewith, Barometrical and Thermometrical ranges to the 31st of July of the present year.

I have the honor
to be,
Sir,
Most Respectfully
Your obt servt
Charles Wilkes.
Comg. Expl. Expdn

To the Hon.
James K. Paulding
Secty of the Navy
Washington

FIG. 2. Photograph of original letter from Wilkes to the Secretary of the Navy transmitting meteorological data.

6); copies of original sketches of Aurora Australis; and Parhelia found in Wilkes' journal (Figs. 7 and 8); Wilkes' description of Parhelia (Fig. 9); and (Fig. 10) Photo of wood cut of *Vincennes* in Antarctic.

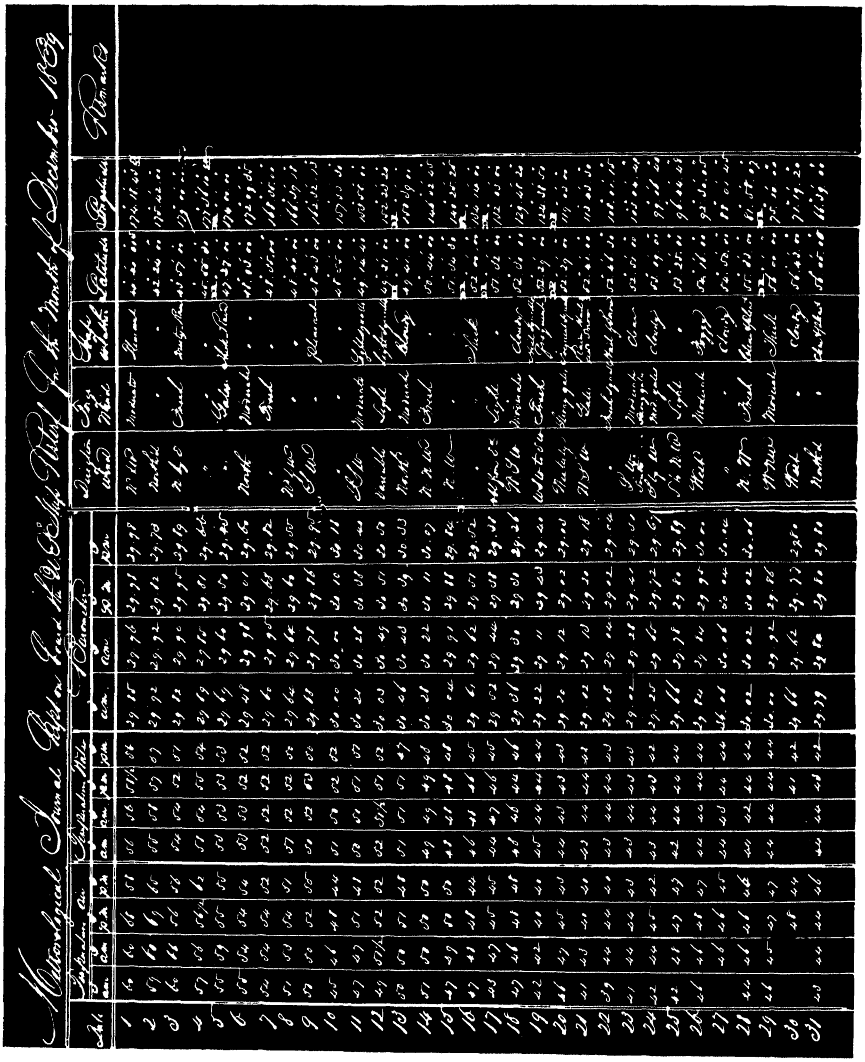


FIG. 3. Photograph of original log sheet of the *U.S.S. Relief.*

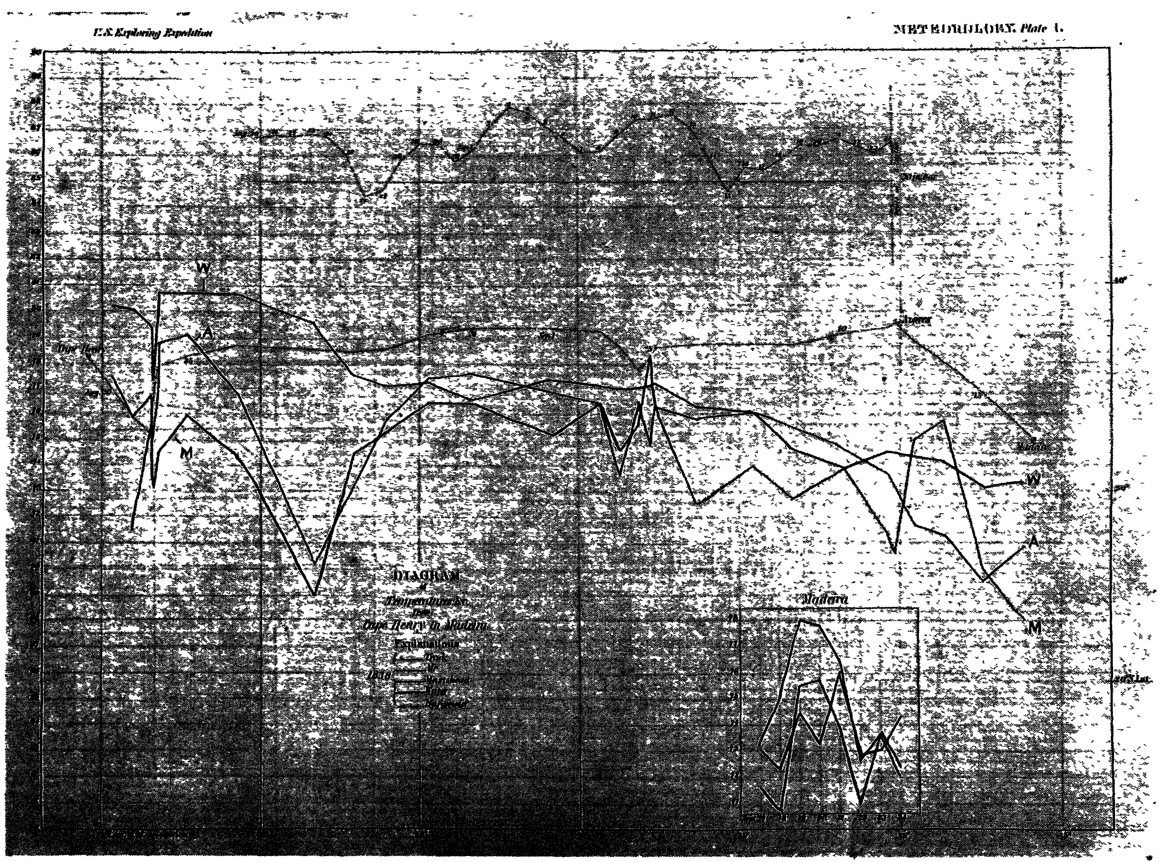


FIG. 4. Temperatures, C. Henry to Madeira. Graphs starting at left—Top = water T. Middle = air T. Bottom = masthead $T°$. Note differences at start indicates error in deck $T°$ perhaps through radiation.

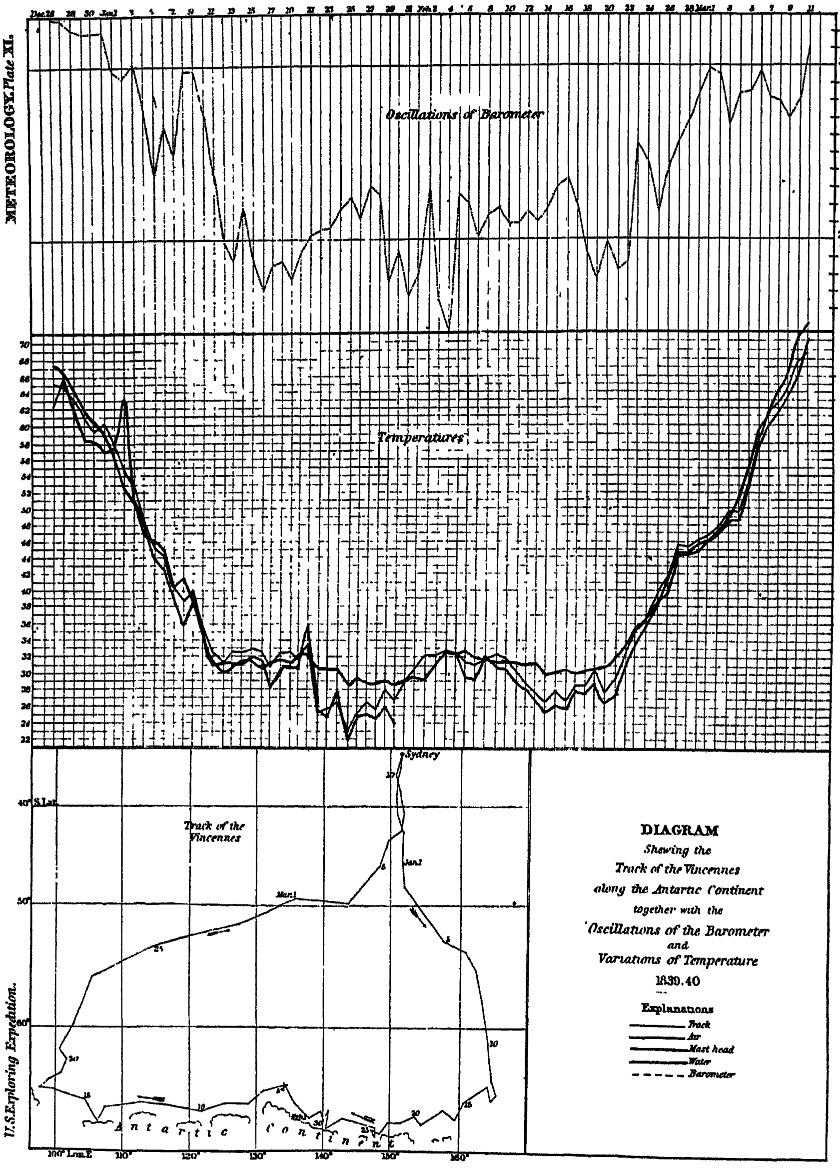


FIG. 5. Temperature and barometer, Antarctic.

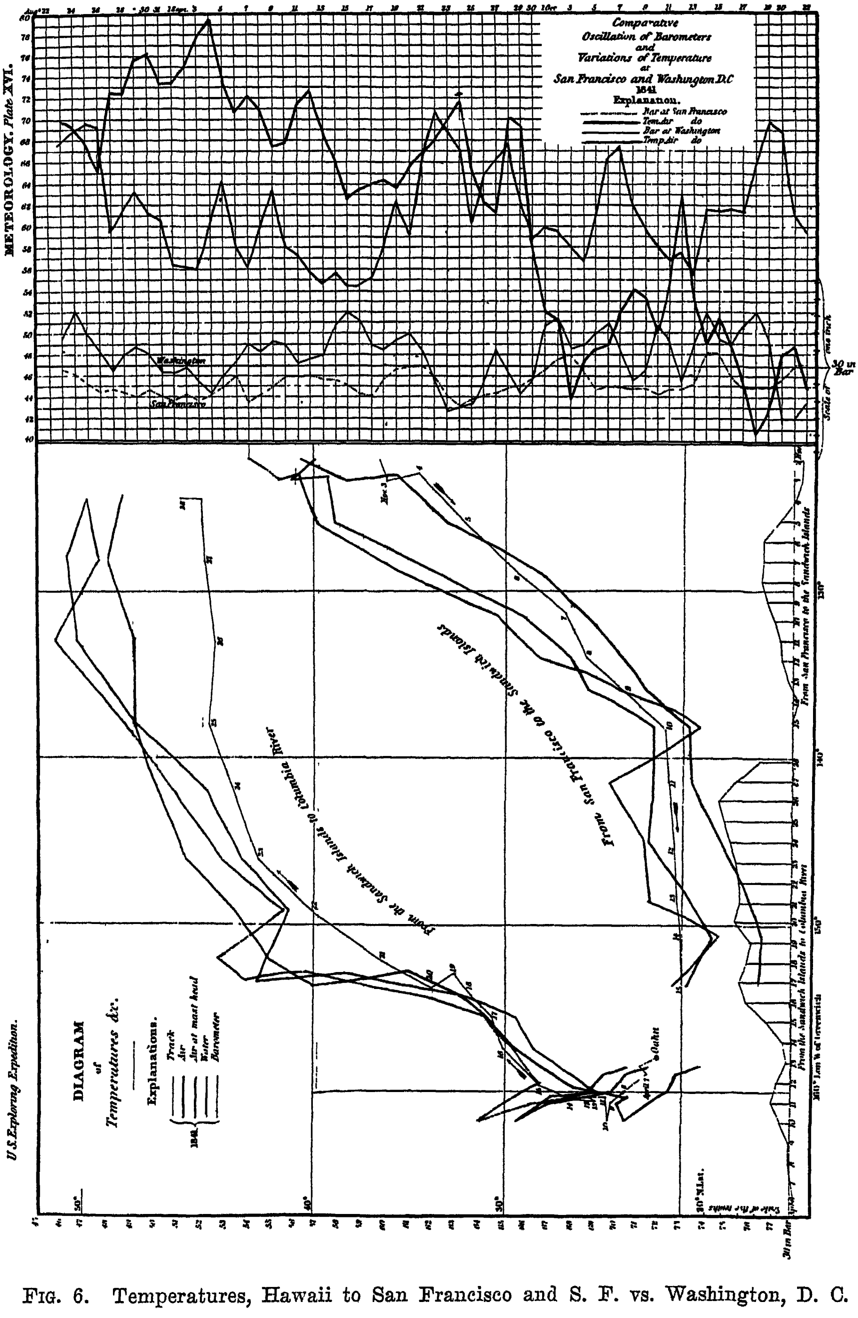


FIG. 6. Temperatures, Hawaii to San Francisco and S. F. vs. Washington, D. C.

Before passing on to general remarks about the meteorological results of the Expedition, the observations in geo-magnetism and gravity should be referenced. Magnetic observations were made at sea whenever conditions permitted, and ashore at every port where the squadron anchored for an appreciable time. The first extensive magnetic and gravity observations were made at Rio de Janeiro at the observatory which

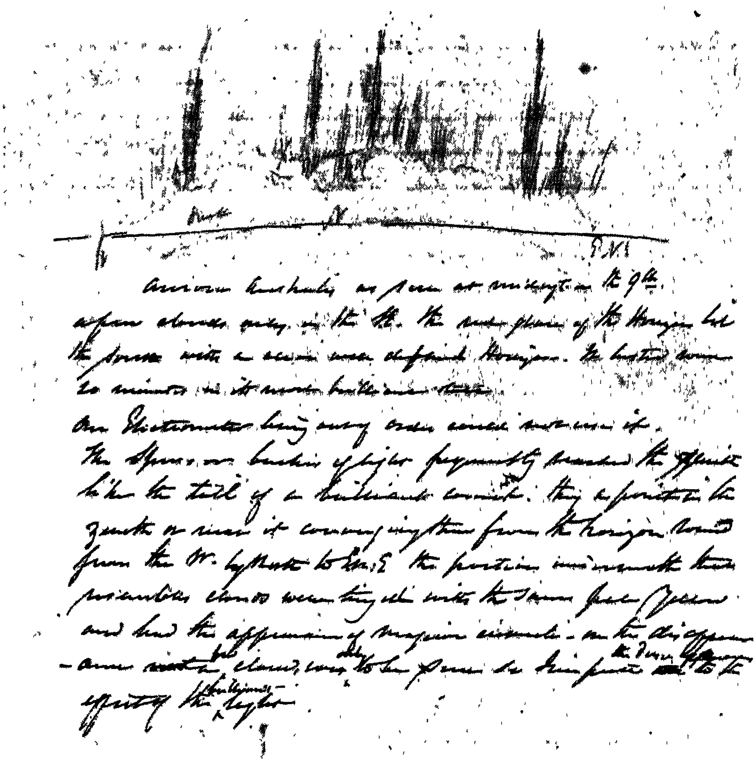


FIG. 7. Photo of original sketch of Aurora Australis, U. S. E. E. records.

was set up on an island in the harbor. Later, similar extensive observations were made at San Francisco and Honolulu. Of his magnetic and gravity observations Wilkes states,[5] "At all important points of the cruise an observatory has been established. . . . In magnetism, observations have been made at fifty-seven stations, for dip and intensity; and at every point where we have remained a sufficient time, those for diurnal variation. The dip

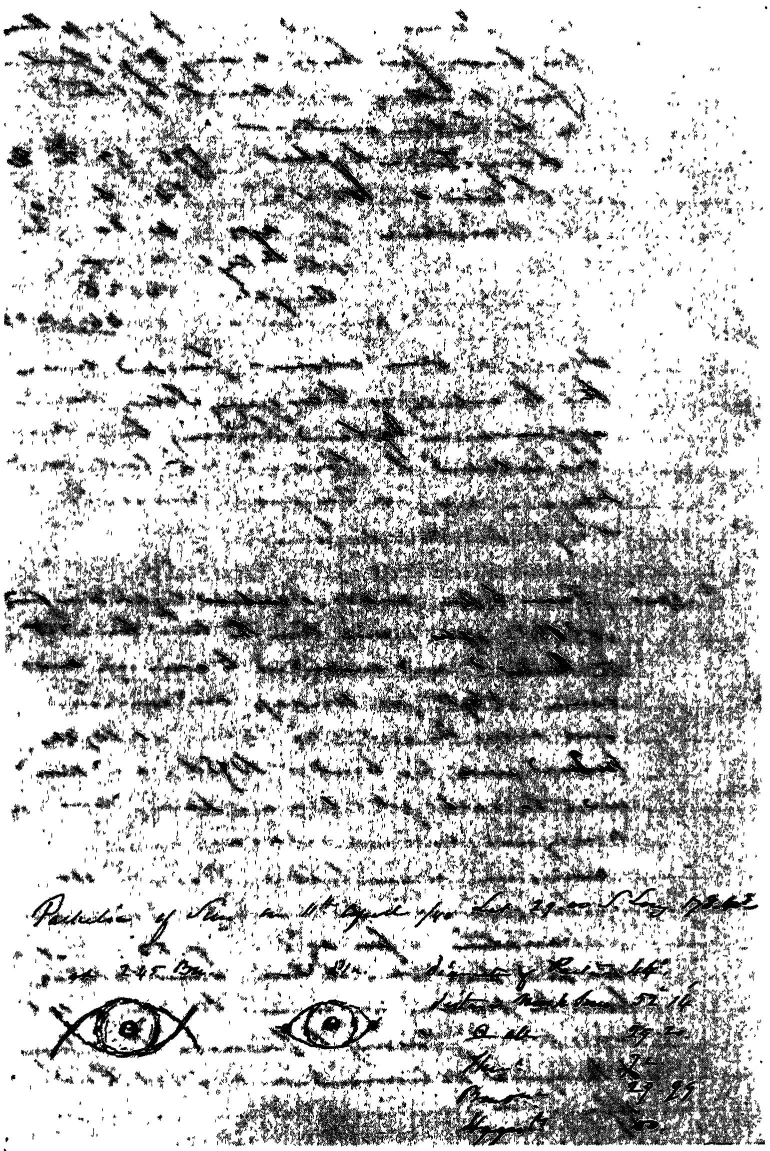


FIG. 8. Photo of sketch of parhelia, original journal. Writing in upper half shows through from reverse side of sheet.

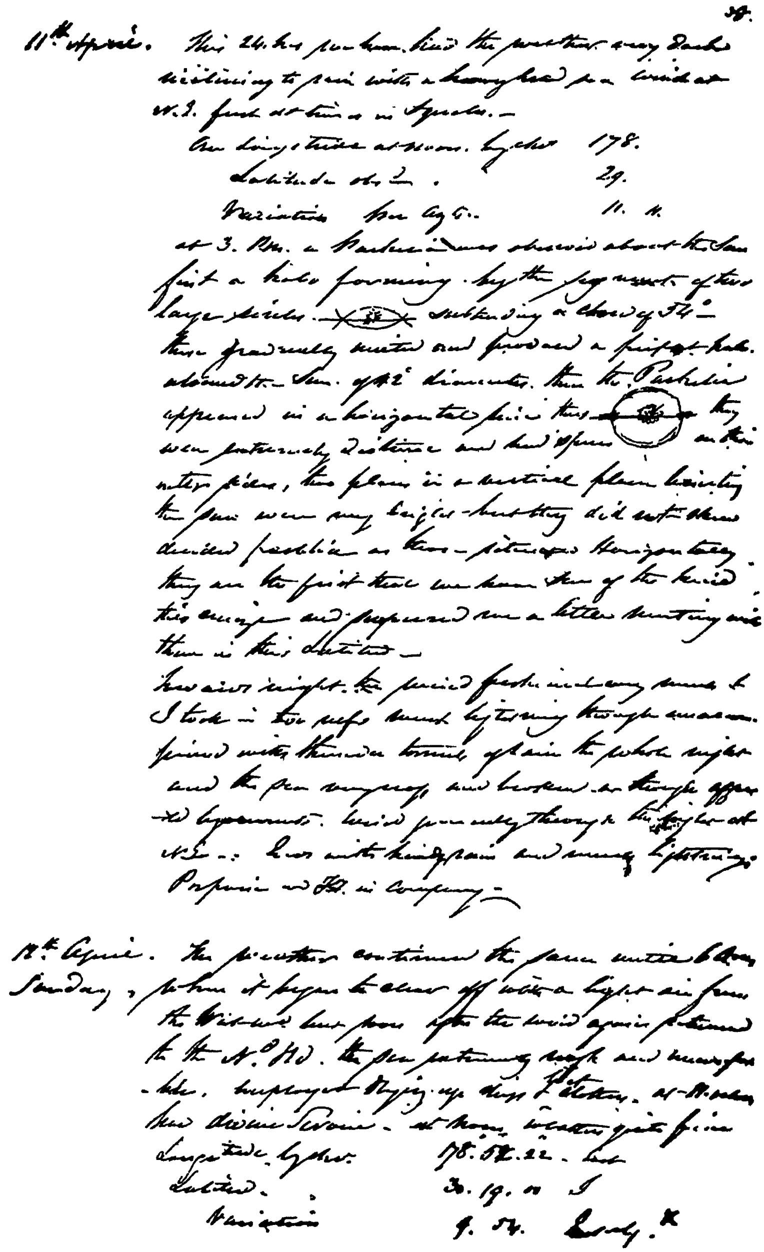
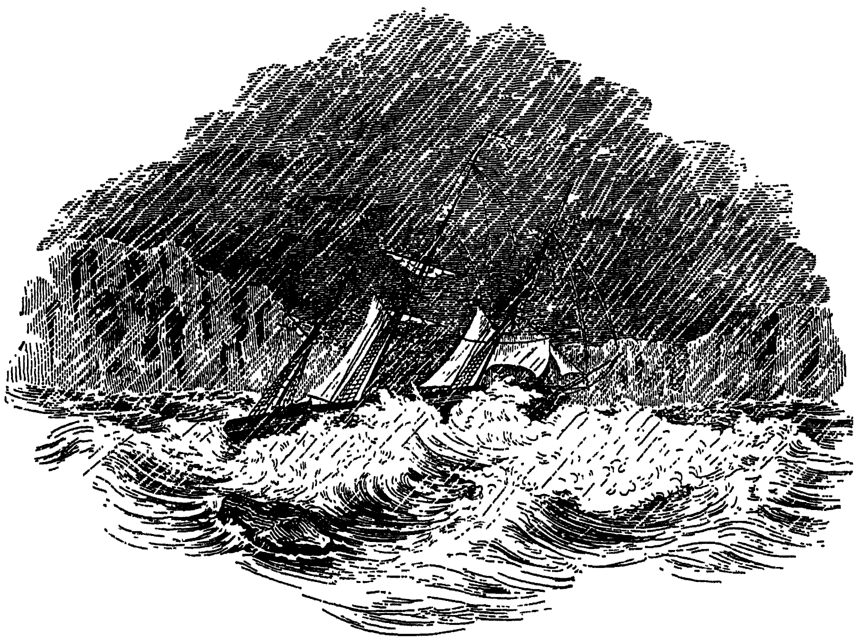

11th April. This 24 hrs [illegible] the weather very dark [illegible] inclining to rain with a heavy head sea [illegible] wind at N.E. fresh at times in squalls.—
Our longitude at noon by chro. 178.
Latitude obs.d 29.
Variation per az. 11. 4.
at 3. P.M. a Parhelion was observed about the Sun first a halo forming by the segments of two large circles [illegible] subtending a chord of 34°— these gradually united and formed a perfect halo around the Sun of 42° diameter. then the Parhelia appeared in a horizontal line thus [illegible] they were extremely distinct and had spurs on their outer sides, those placed in a vertical plane [illegible] the pair were very bright—but they did not show [illegible] parhelia as those situated horizontally. they are the first that we have seen of the kind this voyage [illegible] them in this latitude—
[illegible]
12th April. The weather continued the same until [illegible] Sunday [illegible] [illegible] from the Westward [illegible] the wind again returned to the N.o [illegible] the sea [illegible]
Longitude by chro. 178.58.22.
Latitude. 30.19. [illegible]
Variation 9. 54. [illegible]

Fig. 9. Photo of original sketches and description of parhelia from records of Wilkes Expedition.

has been observed at sea frequently, and the ship's head always kept north and south whilst the observations were making. Very many attempts have been made to observe the intensity at sea, both by horizontal and vertical vibrations, but I have never been able to satisfy myself with the results, whatever others may have done. The only instrument with which I believe it is possible to succeed in getting intensity observations is that of Fox, with which I regret I was not supplied. The term days, whenever possible, have been attended to. Observations for varia-



VINCENNES AMONGST ICE-BERGS.

FIG. 10. Photo of wood cut of Vincennes in Antarctic.

tion have been taken twice a day, when the weather would admit, during the whole cruise. Barlow's plates were used with the compass. All islands that afforded suitable positions for results in this department were landed upon, and series of observations made for dip and intensity. For the determination of the Southern Magnetic Pole, we have variation observations from 35° easterly variation to 59° west, between the longitudes of 97° and 165° east, nearly on the same parallel of latitude, which will give

numerous convergent lines through that space for its determination. Our greatest dip was 87° 30′. The summit of Mouna Loa, thirteen thousand four hundred feet above the level of the sea, was among the magnetic stations. The pendulums have been swung at six stations, one of these at the summit of Mouna Loa, and another at its foot."

These magnetic records, apparently never compiled for publication as intended in Volume XXIV and for a long time lost, were eventually recovered in part. They have been studied and results published at different times by Littlehales [3,7] and Ennis.[6] Wilkes' data constituted a real contribution to knowledge of secular changes in terrestrial magnetism, gave an indication of the location of the south magnetic pole, which Wilkes took to be at 70° S., 140° E., and enabled Littlehales to prepare a map of magnetic declination and inclination in a portion of the ocean south of Australia. Wilkes intended to publish a magnetic chart of the world in Volume XXIV. Ennis in 1934 states,[6] "The results of the United States Exploring Expedition constitute a real monument to Commander Wilkes and his able assistants, and the return of Admiral Byrd from Wilkes' 'Antarctic Continent' may signalize the unofficial centennial celebration of this great and fruitful organization."

Perhaps the least in results came from the pendulum observations of the Expedition. The original data were mislaid for so long that subsequent observations replaced them, and, unlike the magnetic observations, these gravity data by Wilkes were not important for determination of secular changes. Since the volume in which the gravity data were to appear was never completed, it appears that no analysis of these observations has been published.

In reviewing the meteorological results of the Expedition, one is impressed by their far reaching influence and by the information still to be gleaned from Wilkes' records in the light of modern observations from the regions crossed by the Expedition. Our understanding of the meteorology of those portions of the globe is still far from complete. Some of Wilkes'

[6] "Magnetic Results of the United States Exploring Expedition," by C. C. Ennis, *Atmospheric Electricity*, Vol. 39 (1934), No. 2, pp. 91–101.

[7] "The Navy as a Motor in Geographical and Commercial Progress," by G. W. Littlehales, *Bulletin of the American Geographical Society*, Vol. XXXI (1899), No. 2, pp. 124–129.

comments are of interest in relation to modern studies of temperature inversions in the upper air. In a number of cases he relates mirages and other optical phenomena which he observed to the temperature inversions shown in his readings of the masthead thermometer. For many of the phenomena which he saw, Wilkes arrived at an explanation satisfactory to him. In Volume XI he gives his ideas of the formation of certain types of fog and dew. In Volume XVII, Hydrography, and his later booklet on "The Theory of the Winds"[9] he present his views on the general circulation of the atmosphere. These theories have not in general stood the test of time and later observational facts, but they illustrate the independent and searching mind of their author and have served to stimulate discussion and study which have led to sounder theories. It is scarcely to be expected that a man with the ability and excellent general qualifications of Wilkes should also be a profound physicist and mathematician, even in the science of his day. Perhaps his greatest scientific error was in trying to demonstrate that the earth's rotation has no effect upon atmospheric circulation, a fact not generally understood at that time. His "Theory of the Winds" contains also novel but unsound explanations of the distribution of moisture in the atmosphere and the relation of electrical discharges to hurricane formation and movement. The portion giving "Sailing Directions" for a voyage around the world proved to be far more enduring in accuracy and value.

Wilkes lost no opportunity to add to his collection of meteorological data by obtaining copies of climatic records wherever he could get them on his cruise, some from official sources, others from personal journals of citizens in the localities where he anchored. These, together with his own records, have become an important part of our climatological knowledge. He was a pioneer in charting sailing routes and his observations still are represented in the averages shown on the Pilot Charts published monthly by the Hydrographic Office, charts which, during the days of sailing vessels, led to enormous savings in time of ocean passage and to great benefits to shipping of all nations. The interest created by the work of the Expedition encouraged scien-

[9] Charles Wilkes, *Theory of the Winds,* accompanied by a map of the world, showing the extent and direction of the winds, to which are added sailing directions for a voyage around the world, Philadelphia, 1856, pp. 116, 3 charts.

tific studies and progress which were among pioneering steps essential to the development of the scientific and service activities now carried on by several government bureaus and private scientific institutions.

ADDITIONAL SOURCE MATERIAL

Rear Admiral W. R. Gherardi, U. S. N., The Hydrographic Office of the Navy, Special Notice to Mariners. One Hundredth Anniversary, 1830–1930. Washington, 1930, pp. 18.

James D. Hill, Charles Wilkes—"Turbulent Scholar of the Old Navy," *Proc. U. S. Naval Institute*, Vol. 57, No. 7, July 1931, pp. 867–887, portrait and illustrations.

Louis N. Feipel, "The Wilkes Exploring Expedition, its Progress through Half a Century, 1826–1876," *ibid.*, 40, pp. 1323–1350.

Bess Styer Hurst, "One Hundred Years Ago," *Proc. U. S. Naval Inst.*, Vol. LXIV, No. 429, Nov. 1938, pp. 1624–1627.

THE REPORTS OF THE WILKES EXPEDITION, AND THE WORK OF THE SPECIALISTS IN SCIENCE

HARLEY HARRIS BARTLETT

Director, Botanical Garden, University of Michigan

(Read February 23, 1940, in Centenary Celebration of the Wilkes Exploring Expedition, 1838–1842)

THE Reports of the United States Exploring Expedition of 1838 to 1842, known as the Wilkes Expedition, are a monument in the record of American science. The inception of the Expedition in the first place marked America's assertion of its scientific coming of age, and the publication of the narrative and the scientific reports, even though attended by difficulties and delays of all kinds, some of which were inevitable and some quite unnecessary, was finally carried almost to completion in a manner entirely creditable to the authors.

In appraising the value to science of the Expedition, one must admit that the influence of the official reports was greatly reduced by inadequate and ineffective distribution. There were other and greater values that came about from the Expedition through the development of scientists themselves, and through the organization of scientific institutions to perpetuate the scientific work begun by the Expedition. The greatest value was that the enormous coordinated effort that the few scientists of a hundred years ago put forth enabled America to take a place with the leading European nations as a partner in the development of world science.

Before proceeding, then, to what may prove to be a wearisome review of the reports themselves, I wish to discuss the historic and scientific setting of the Expedition. I shall also tell something about the authors of the reports, what they were doing before they participated in the work of the Expedition, and what influence their labors in a great project may have had upon their later careers and upon the course of scientific affairs in America. My task in reviewing the results of the Expedition has fortunately been simplified by the assignment of several of the more

important topics to other speakers. I shall have nothing to say about the geographic discoveries or Dana's work on volcanic islands and coral reefs, nor need I deal with the reports written by Wilkes himself.

At the time the Expedition sailed, in 1838, it was primarily scientific in character, whatever its antecedents may have been, and quite regardless of what even some of the scientists may have thought, at times when they conceived their individual work to have suffered through their subjection to naval discipline.

In making the claim that the final aims of the Expedition were primarily scientific I know that I lay myself open to argument. There had been much public discussion of the objectives of the Expedition and disagreement about them before Wilkes and his "scientific gentlemen" ever set sail and it continued after they returned. The truth is that the relative importance of several objectives changed greatly during the decade of discussion, part of it very acrimonious, that intervened between the inception and execution of the project. The chief promotor of the Expedition, Jeremiah N. Reynolds, sought "national glory" however it might be attained, whether by geographical and scientific discovery, by display of maritime power, which would show that America challenged the pretensions of older countries to rule the waves, or by the peaceful and utilitarian charting of "obstacles to navigation", in behalf of the far-flung whale and seal fisheries, and of expanding commerce. Reynolds's ideas underwent a gradual change and when they were finally in a large measure carried out, without his participation, the reality bore little resemblance to the initial dream. As a matter of fact, the historical background of the Expedition had a "lunatic fringe".

Jeremiah N. Reynolds, the chief promotor of the Exploring Expedition, is now so little known that his name has often been given as "John" instead of "Jeremiah". The best account[1]

[1] Almy, Robert F. J. N. Reynolds: a brief biography with particular reference to Poe and Symmes. *The Colophon*, n.s., 2: 227–245, 1937.

Professor Almy of Miami University, the author of this biography, spared no pains to search out whatever local record and tradition in Ohio had preserved regarding a native son whose career seems to have been followed with especial interest in his home state. For calling my attention to Almy's article I am much indebted to Miss Mary E. Cooley of Mt. Holyoke College, who is writing a biography of Charles Wilkes.

of his life says: "Reynolds followed a gleam for nearly twenty years, an imperial dream of a United States anchored 'on the axis of the earth' and turning once around in twenty-four hours. Following this mirage he chilled his bones in the 'farthest south' of the day. It carried him still later to the speaker's rostrum of the House of Representatives. But it caused him in the end to lose his temper—noisily—and to pull his house down about his ears. The net effect of this final tantrum has been an inclination by posterity to remember him vaguely by the wrong name and to consider his case mainly by virtue of his connection with more enduring personalities."

Reynolds grew up as a land lubber in the vicinity of Wilmington, Ohio, and was not an officer of the U. S. Navy, as at least one account of his life said he was. He had, however, served as a special agent of the Navy Department during the summer of 1828 to gather data from ship captains and owners regarding conditions in the South Seas. He had also been secretary to Commodore John Downes on the U. S. Frigate "Potomac," from 1832 to 1834. Moreover, he was scarcely a "scientist," as John Quincy Adams called him, and certainly not a "Doctor". His education consisted of three years, 1819–1822, at Ohio University, where he did not complete his course. When he first attracted public attention, it was as an advocate of the quaint "theory" of Captain John Cleves Symmes, Jr., that the earth was made up of five concentric spheres with a hollow core and polar openings so wide that a voyager "might pass from the outer side of the earth over the rim and down upon the inner side a great distance before becoming aware of the fact at all". The idea of concentric spheres was of course nothing new. It had come down through the ages, but it would seem that the time when it could reasonably have found advocates should have passed at least a couple of centuries before. Human credulity, however, is so unaccountable that, according to Almy, when Symmes petitioned Congress in 1822 to send a government expedition for testing his theory, to such an unbelievable extent did his ideas meet with favor that he enlisted twenty-five advocates in the House of Representatives who voted "aye" to the proposal. I fail to find an official contemporary confirmation of this incident, but there is no reason to question it, in view of the

reception by Congress of petitions from various sources in the following year.[2]

Thus the idea of the Expedition originated on the lunatic fringe, but Symmes was soon to drop out of the picture. He became seriously ill in 1827, and died in 1829. Reynolds had not been Symmes's first advocate. Almy tells us that the earliest exposition of the "theory" was by a disciple, James McBride,[3]

[2] The Debates and Proceedings in the Congress of the United States. 17th Congress, 2nd Session. Washington: Printed and Published by Gales and Seaton. 1855.

[Monday, Jan. 27, 1823.] "Among the petitions presented today was the following, presented by Mr. J. T. Johnson, of Kentucky: '*To the honorable the Senate and House of Representatives in Congress assembled:* The petition of the subscribing citizens respectfully showeth, that, in our opinion, both the national honor and public interest may be promoted by the equipment of an exploring party, for the purpose of penetrating the Polar regions, beyond the limits at present known; with a view, not only of making new discoveries in geography, natural history, geology, and astronomy, but of opening new sources of trade and commerce.

" 'And it is our further opinion, that Captain John Cleves Symmes, late of the United States Army, who professes to have originated a new theory of the earth, which may be verified by a voyage to the North, will be a suitable person (assisted by men of science and experience) to be intrusted with the conduct of such an expedition.

" 'Independently of the truth or error of Symmes's theory, there appear to be many extraordinary circumstances, or phenomena, pervading the Arctic and Antarctic regions, which strongly indicate something beyond the Polar circles worthy of our attention and research.

" 'We, therefore, pray Congress to pass a law granting an exploring outfit, in conformity to our memorial; and thereby at once subserve the cause of philosophy and the earnest wishes of your constituents.'

"Mr. Johnson moved to refer it to the Committee of Foreign Relations.

"Mr. Farrelly, of Pennsylvania, moved to lay it on the table.

"Mr. Johnson hoped it would not be laid on the table. The memorial had many respectable signatures, and perhaps, on further examination, it might turn out that something useful might grow out of the investigation of it.

"The motion to lay the memorial on the table was negatived.

"The question recurring on referring it to the Committee on Foreign Relations —Mr. Archer suggested the propriety of referring it, in preference, to the Committee on Commerce, the object of the memorialists being probably to establish a commerce with the interior inhabitants.

"The question to refer the memorial to the Committee on Foreign Relations was decided in the negative—56 to 46.

"On motion of Mr. Little, the memorial was then ordered to lie on the table." (Cols. 698–9.)

In February of the same year similar petitions from Charleston, South Carolina, Greenville and Drake counties, Ohio, and Huntington, Pennsylvania (col. 792), suffered the same fate, as did three additional petitions from Ohio which specifically asked for the outfitting of an expedition to explore the polar regions, under the conduct of Captain John Cleves Symmes (col. 928).

[3] [McBride, James] "Symmes's Theory of Concentric Spheres; demonstrating that the earth is hollow, habitable within, and widely open about the poles." By a Citizen of the United States. Cincinnati, 1826. See also: Clarke, P., "The Symmes' Theory of the Earth," *Atlantic Monthly,* 21: 471–480, 1873. Peck, John Weld, "Symmes' Theory." *Ohio Archeol. and Hist. Publications,* 18: 28–42, 1909.

of Hamilton, Ohio, who wrote under the *nom de plume* "Citizen of the United States", a name which Reynolds himself later (1837) adopted in his anonymous newspaper controversy with the Secretary of the Navy, Mahlon Dickerson. Reynolds had often publicly avowed in his lectures that he believed the Symmes "theory" and could not attempt to deny that he had done so when Dickerson twitted him about it, saying that the naval officers were disinclined to serve in an expedition in which a disciple of Symmes would have undue influence, since he might approach "too nearly to the verge of this said opening of sixteen degrees around the pole, or [undertake] some other movements to test the truth of his strange theories". Justly enough, in view of the record, Dickerson was profoundly distrustful of Reynolds's science.

Reynolds had, as a matter of fact, relinquished his advocacy of Symmes's theory, and according to Almy, published a series of three articles in the "National Intelligencer of Washington" in the form of remarks on a review of McBride's book, defending the less extreme proposition that there was apparently an icy barrier, toward both poles, beyond which "the nearer the pole, the less ice".[4] As we shall see, his ideas inevitably must have been modified by his contact, in 1828, with American whalers and sealers who had attained a "farthest south" in the Antarctic. At the time it was also generally credited that James Weddell had attained 74° 15′ south in ice-free water, after passing ice fields.

In 1827, while temporarily a resident of Baltimore, Reynolds continued to press Symmes's idea of an Antarctic Expedition. He seems to have been given hope of receiving financial aid from private sources, but soon turned to memorializing Congress for a government subsidy. He then brought political pressure to bear upon Washington, and by January 1828 had so far advanced his cause as to get the attention of Congress.[5]

[4] These articles, reprinted during Reynolds's visit to the Antarctic and South America, constituted his first booklet, "Remarks on a Review of Symmes' Theory, which appeared in the American Quarterly Review, by a 'Citizen of the United States' ". Washington, Gales and Seaton, 1832. (Not seen; referred to by Almy. We find from the printed catalogue card of the Library of Congress that there was an earlier edition of this reprint, in 1827.)

[5] Journal of the House of Representatives, 20th Congress, 1st Session, Washington: Printed by Gales & Seaton, 1828.

Jan. 21 [1828]. The Speaker presented a letter addressed to him by J. N. Reynolds, accompanied by several memorials of citizens of the United States, recom-

Events now moved fast. Secretary Southard approved the project, in reply to an inquiry of the Committee on Naval Affairs dated March 3, 1828. He said: "We now navigate the ocean, and acquire our knowledge of the globe, its divisions and properties, almost entirely from the contributions of others. By sending an expedition into that immense region, so little known to the civilized world, we shall add something to the common stock of geographical and scientific knowledge. . . ."

On March 25, 1828, the committee reported a bill to authorize an expedition.

mending to the favorable consideration of Congress, the importance of affording some efficient aid in fitting out a small expedition to explore the immense and unknown regions in the Southern Hemisphere; which letter and memorials were referred to the Committee on Naval Affairs (p. 197).

20th Congress, 1st Session. [Doc. No. 88.] Ho. of Reps. Letter from J. N. *Reynolds to the Speaker of the House of Representatives* upon the subject of An Antarctic Expedition, accompanied with Petitions from Inhabitants of several States, praying the Aid of Government in carrying the same into effect. January 22, 1928. Read, and referred to the Committee on Naval Affairs. Washington: Printed by Gales & Seaton. 1828. [pp. 4.]

Reynolds's letter transmitted four memorials, the first from Albany, New York, the second from Charleston, South Carolina, signed by the Mayor, the President of the Chamber of Commerce, "and by a very long list of respectable citizens", the third from Raleigh, North Carolina, and the fourth from Richmond, Virginia, all "recommending to the favorable consideration of Congress, the importance of affording some efficient aid in fitting out a small expedition to explore the immense and unknown regions in the Southern Hemisphere". It also transmitted a resolution adopted by the House of Delegates of the State of Maryland of which the preamble stressed the importance of "enlarging and improving the boundaries of knowledge". The resolution was: "That we do highly approve of the views of the said memorialists, believing that a polar expedition, if properly conducted, could scarcely fail in adding something to the general stock of national wealth and knowledge, and to the honor and glory of the United States."

20th Congress, 1st Session. [Doc. No. 201.] Ho. of Reps. Petition of Citizens of New Bedford, praying that a Naval Expedition may be undertaken for the Exploration of the North and South Pacific Ocean, and other seas visited by whale ships and others. March 17, 1828. Referred to the Committee on Naval Affairs. Washington. Printed by Gales & Seaton. 1828. [pp. 3.]

"The petition of citizens of New Bedford, Massachusetts, engaged in the whale fishery to the Pacific Ocean, Respectfully represents: That, of late years, it has been customary for ships employed in the Pacific Whale Fishery, to pursue their voyages into seas but little known, and but partially explored, abounding with islands and reefs, either heretofore undiscovered or undescribed, or so erroneously laid down upon existing charts as to mislead the navigator as to their true position and bearings: That, owing to the scanty information at present afforded on points so essential to the safety of navigation, whale ships visiting those seas are necessarily exposed to the most imminent perils: Wherefore your petitioners beg leave respectfully to request, that a naval expedition may be fitted out by the Government of the United States, for the purpose of exploring such portions of the North and South Pacific Ocean as present the greatest exposure to these hazards, and are most generally resorted to by whalemen; and of making such accurate observations and surveys of the islands, reefs, etc. that therein exist, as to furnish a sure and correct guide to future navigators."

Expressions of approval included a letter from Commodore John Downes, as whose secretary Reynolds was later to write the "Voyage of the Potomac". He was soon authorized to visit New England coastal cities as government agent "to get preliminary information at first hand principally from conversations [with whalemen and sealers] logbooks and journals". His report met with the approval of President John Quincy Adams and Samuel L. Southard, Secretary of the Navy. Southard, under date of November 27, 1828, said:[6] "With a view to give the most useful character to the enterprise, it is important that persons skilled in the various branches of science should partake in it. Correspondence has therefore been held with scientific men, and some selections have been made, and others are now making, by the Department, of astronomers, naturalists and others, who are willing to encounter the toil, and will be able to bring home to us results which will advance the honor and promote the interests of the nation."

Under date of February 5 and 6, 1829, President John Quincy Adams requested Secretary Southard to make a detailed report to the Senate upon what plans had been made for the expedition and what expenses already incurred. Southard replied at length, making this statement (p. 40): "In ordering the officers, care was taken to select such as were believed to be not only good seamen and navigators, but also distinguished for enterprise and science [since] . . . it was necessary that the officers should . . . partake in all the scientific researches."

An expedition was authorized by resolution of Congress, but when the Jackson administration came in the project was vetoed by the President, through motives of economy, rather than disapproval. As a result, it was several years before the Reynolds report again came to attention and was officially twice published,

[6] 24th Congress, 1st Session. [Senate Doc. No. 262.] March 21, 1836. Read and ordered to be printed. Mr. Southard made the following Report, with S[enate] Bill No. 175. Gales & Seaton, print. [Washington, 1836.] [pp. 87.]

Reynolds's Report on "Islands and Reefs" was reprinted in this document (pp. 55–87) with omission of nearly all the notes upon high Antarctic latitudes. There are also numerous additions, indicating that Reynolds had presumably had an opportunity to revise his report in the year that intervened between its being called for and printed by the House of Representatives and reprinted by the Senate. In this connection it appears that Secretary Dickerson was not treated altogether with frankness, since his identical letter of transmittal is prefixed to the modified report with no indication that changes had been made before printing and eight years after submission of the original to Secretary Southard.

by the House and by the Senate. The two versions are not at all alike, and certain very interesting passages regarding the Antarctic are to be found only in the House edition.[7] The report states that in obedience to Secretary Southard's orders of June 30 [1828] Reynolds had "repaired without delay to New London, Stonington, Newport, New Bedford, Edgartown, Nantucket, and other places where information might be found of the Pacific Ocean and South Seas. The whaling captains were ready to communicate such knowledge as they had treasured up or recorded in their numerous voyages. The owners of whale ships were equally anxious to do all in their power to assist me. . . . I shall now proceed to give you a list of the discoveries of our enterprising and careful navigators in those seas, in as tabular a form as may be consistent with a clear view of the extent and importance of these discoveries. The English charts, and those of other countries, are as yet very imperfect. Much of their information has been obtained from loose accounts from whalers, who were careless in some instances, and forgetful in others, and which were seized with greediness by the makers of maps and charts, in order to be the first to make these discoveries known. . . .

"The information I have collected, if not perfectly accurate, is certainly the most so that can be found. It has been drawn from purely original sources; nothing has been received at second hand. I have examined the logbooks, journals, maps, and charts of the navigators themselves, and in most cases have questioned them personally. . . .

(p. 26.) "In frequent and familiar conversations with these practical men, who have spent so many years of their lives in these high latitudes, I have been enabled to draw out a great deal of information in relation to the manner of conducting a vessel with safety through the ice, and the proper season of the year to make the attempt to reach high latitudes, with a world of useful hints, and observations of a kindred nature. These I do not deem it necessary to give in detail, but have recorded them in my

[7] 23rd Congress, 2nd Session. [Ho. of Reps. Doc. No. 105.] Navy Dept.: Letter from the Secretary of the Navy, transmitting a report of J. N. Reynolds, in relation to islands, reefs, and shoals in the Pacific Ocean, etc. January 27, 1835. Referred to the Committee on Commerce. Gales & Seaton, print. [Washington, 1835], pp. 28. Secretary Mahlon Dickerson's letter of transmittal gives the date of Reynolds's report, originally addressed to Samuel L. Southard, Secretary of the Navy, as September 24, 1828.

private notes for future use. I have also been able to ascertain, with a good deal of precision, the portion of the southern hemisphere where these attempts to reach a high latitude have always proved ineffectual. And they have communicated to me, also, where their experience has fully shown that vessels may advance with no great difficulty into very high latitudes, if not to the 90th degree itself. From all which, as well as from answers received to a circular letter addressed to many whom I could not see, I have been enabled to make the following estimate:

That they have been beyond 70° S. latitude in a few instances, in which latitude they experienced moderate weather, a clear sea, and no land or ice to the south. They all agree that the ice to be met with is first formed and attached to land, and that the greatest impediment to navigation from ice will be found from 62° to 68° S., except in those meridians where they have not been able to go far south at any time. They have seen lands to the east of the Shetlands, but give no account of any animal or vegetable productions on any of them.

The southern part of the New South Shetlands extends farther than anyone has yet penetrated. The shores are bold, and in many places afford spacious harbors, which look as if they might extend far into land, like Hudson's or Baffin's Bay.

In latitude 63° S., and 63° W. longitude, from the island Pisgah, our sealers have sailed along a high and rugged coast, tending S.W. to 75° S. longitude and 66° S. latitude. Captain Pendleton, of Stonington, Connecticut, one of the most practical and intelligent sealers I met with, and who has spent many years in the South Sea fur trade, is strongly of the opinion that there are many valuable discoveries to be made in the seas southwest of the Shetlands.

. . . On the northern part of Palmer's land, and in latitude 66° S. and about 63° W. longitude, Captain Pendleton discovered a bay, clear of ice, into which he ran for a great distance, but did not ascertain its full extent south. . . .

Captain Morrill, who sailed from this city (New York) on a sealing voyage, while he commanded the brig Wasp, between the years 1822 and 1825, sailed between the latitude of 59° 30′ and 69° 15′ south, from 117° east to 110° west longitude; discovered several islands, and saw many indications of land, but had not time to run for it. On the meridian 46° west, he fell in with land, and coasted it from 60° 47′ to 71° south, and does not know how much further it extends.

Enough has already been given of what I have collected, to show how much remains to be done in that portion of the globe; and enough also, to prove to the department that it is in possession of more information of those seas than the Admiralty of any other nation, however commercial, for those seas are truly our field of fame. Too much credit cannot be given to our whalers, sealers, and traffickers in those seas for

the information they have acquired, and the liberality, generally speaking, with which they have imparted it. But, after all their exertions, justice to ourselves as a great people requires that this mass of information should be reviewed, analyzed, classified, and preserved in careful literary labors for the benefit of mankind.

That this may be accomplished in your administration of the marine of our country, and under your auspices and especial care, to the satisfaction of the public, and the honor of our country, is my ardent wish. It is a desideratum for which I have labored, and am ready to labor while my arm has a sinew or my heart a pulse. Very respectfully, Your obedient servant, J. N. Reynolds. City of New York, September 24, 1828."

Before President Jackson ordered the indefinite suspension of plans for the Antarctic Expedition, Reynolds had already arranged for chartering a Stonington brig, the "Seraph," Benjamin Pendleton, Jr., Captain. Congress had so far authorized Reynolds to act officially that a claim was eventually recognized as due the owners of the "Seraph" for failure to carry out the Government's engagement with Captain Pendleton.[8]

In October 1829 Reynolds accompanied Captains Nathaniel Palmer and Benjamin Pendleton in a combined voyage of sealing and discovery to the south.[9] This voyage greatly modified Reynolds's ideas about the Antarctic and led him to place the emphasis thereafter upon the importance of discoveries in tropical latitudes in the South Seas. There is less information about his voyage with Captain Pendleton than one could wish. What scant knowledge there is, derived from several sources, several of which have recently been cited by Hobbs,[10] indicates that it was a genuine adventure. He drew upon his experiences for literary material, but never published the book which he promised.

He utilized for an article in the *Knickerbocker Magazine* the

[8] 21st Congress, 1st Session, House Rept. no. 418. [Petition of] Benjamin Pendleton, May 31, 1830. Reprinted in: American State Papers, Class VI, Naval Affairs, Vol. III, p. 684, 1860.

[9] According to Almy, the *New Bedford Mercury* for Oct. 30, 1829, reported: "The brig Seraph, Capt. Pendleton, sailed from Stonington the 21st, to join her consort, the Annawan, Capt. Palmer, off Block Island, whence they will proceed on their exploring voyage."

[10] Hobbs, Wm. Herbert, The discoveries of Antarctica within the American Sector, as revealed by Maps and Documents. *Trans. Amer. Philos. Soc.*, n.s. 31, Pt. I, 1939. (See especially p. 53.)

Hobbs comes to the conclusion that this expedition encountered no land except the previously known South Shetlands, but there seems to be a bare possibility that information may have been withheld for Reynolds's promised book, which was never published.

legend of Mocha Dick, the great white whale [11] which he took down in his journal from the forecastle yarn of a New York whaler "in the very cruising grounds of the white whale himself—that is, between the islands of Mocha and Santa Maria, off the coast of Chile". This has recently been republished.[12] Reynolds likewise published, according to Helen S. Wright [13] (quoted by Almy), an excerpt from his journal [14] relating the adventures of himself and a companion, J. F. Watson, while marooned on a barren islet of the South Shetland group. This account resembles one in the *History of Clinton County, Ohio,*[15] Reynold's boyhood home, where, Almy says, "the inhabitants in their current newspapers still cherish the memory of him as the most romantic figure ever to emerge from their quiet district". Here may have remained relatives or friends who had access to Reynolds's own journal, or to information derived from it, and who may have woven into it more of fancy and romance than the facts would have justified. Almy quotes, however, this definite statement from the *History:* "They [Reynolds and his companions] at length arrived in sight of land, which they afterwards discovered to be a southern continent, which seemed completely blockaded with islands of ice."

So far as the "Annawan" and the "Penguin" are concerned, the historical investigations of Hobbs have shown that these vessels stopped at various places in the South Shetland group, where Dr. Eights, the naturalist, conducted investigations, but that no other Antarctic land whatever was seen. As to the "Seraph", under Captain Pendleton, there is little or no information. As Almy says, the assertion in the *History of Clinton County* that Reynolds "discovered" the southern continent can-

11 Reynolds, J. N., Esq. "Mocha Dick: or the white whale of the Pacific: a leaf from a manuscript journal." *The Knickerbocker,* or *New York Monthly Magazine,* 13: 377–392, May, 1839.

12 Reynolds, J[eremiah] N. (with pictures by Lowell LeRoy Balcom, ed.). *Mocha Dick; or, the White Whale of the Pacific.* New York, Charles Scribner's Sons, 1932. "Balcom has unfortunately added to the web of errors around Reynolds's life by calling him 'a United States naval officer'. Reynolds was a civilian, a fact which later counted against him. Mr. Balcom represents the Penguin in a woodcut as a full-rigged ship; the Penguin was a schooner." (Almy, *l.c.,* p. 241.)

13 Wright, Helen S. *The Seventh Continent: a History of the Discovery and Explorations of Antarctica.* Boston, R. G. Badger, 1918.

14 Reynolds, J. N. Leaves from an unpublished journal. *New York Mirror,* April 21, 1838.

15 *The History of Clinton County.* Chicago, J. H. Beers and Company, 1882. (See pp. 580–585 for a brief account of Reynolds's life, prepared by a friend, Mrs. R. B. Harlan, of Wilmington, Ohio.)

not be sustained. The fragment of his journal which Reynolds published as *Mocha Dick* indicates that at least part of the time he was with Captain Palmer on the "Annawan", not with Captain Pendleton.

Since Reynolds had long wanted to be the "historiographer" of an Antarctic voyage, we may be sure that his journal, if it could be found to be still in existence, would turn out to be a most interesting document, but the chances are slight that it will ever come to light. His complete disillusionment as to the conditions in the Antarctic may have led him to withhold what he knew, for he was a most prolific writer.

Edgar Allen Poe seems to have owed much to Reynolds for some of his most imaginative tales.[16] As Almy points out, the contrasting points of view with regard to the mysterious polar regions presented by Poe's earlier and later tales correspond exactly to the evolution of Reynolds's views with regard to polar conditions, which came about through association with the New England captains, even before he went to the Antarctic himself, from advocacy of Symmes's crazy theory of concentric spheres to the mere expression of what seemed to him a reasonable possibility, that the polar climate might be mild beyond the ice rim.

In October, 1832, Reynolds was picked up at Valparaiso, Chile, by Commodore Downes, who was returning from a mission of chastising the Achinese on the northwest coast of Sumatra for acts of piracy. Forwarned of a hostile outcry against him in certain quarters at home and wishing to be prepared for prompt publication of his vindication, Downes employed Reynolds as private secretary to write the narrative of the circumnavigation, which was published in 1834.[17]

[16] *Ms. Found in a Bottle* and *The Narrative of Arthur Gordon Pym.*

[17] Reynolds, J. N., *Voyage of the United States Frigate Potomac, under the command of Commodore John Downes, during the Circumnavigation of the Globe in the years 1831, 1832, 1833, and 1834; including a particular account of the Engagemenment at Quallah-Battoo, on the Coast of Sumatra, with all the official documents relating to the same.* "Naval power is National Glory." Illustrated by several engravings. New York: Harper and Brothers, 1835.

There was a preliminary issue of only the Introduction and a few pages of this book (30 pages, according to the Library of Congress catalogue) which Reynolds seems to have reckoned as a "1st edition". The full work, 560 pages, and 10 engravings, including folded panoramic views, seems to have been considered by Reynolds as a second edition, although the title page does not so indicate. The so-called "third edition" was merely a cheaper issue of the full, unchanged text without the engravings and with the title-page altered by the addition of "Third Edition".

On account of his employment with Downes, Reynolds was able to get access to official files, by permission of Secretary of the Navy Levi Woodbury and Secretary of State John Forsyth. The former was to give up his cabinet position within a few months to Mahlon Dickerson, and it seems to have been in connection with the completion of his *Voyage of the Potomac* that Reynolds had his first contact with Secretary Dickerson, whom he was so soon to fix upon as the object of his particular enmity. The compliment was returned, with the result that the feud ultimately threw the control of the Expedition (preparation for which, on a larger scale, was resumed in 1834 as a result of Reynolds's increased acquaintance and influence) from Dickerson to Secretary of War Joel R. Poinsett. Reynolds, in the four years after his return on the "Potomac", seemed obsessed by ideas of "National Glory", in which he thought of himself as largely participating. He dedicated the *Voyage of the Potomac* "to the Honorable the Secretary and Officers of the United States Navy,—believing that, whatever is well done by one [*i.e.*, Commodore Downes] among the band of brothers, is done by all in feeling and principles; for this is the only way of making up the treasury of a nation's glory". On the title-page he put "Naval Power is National Glory", and in the dedication he said: "Our flag should be borne to every portion of the globe, to give to civilized and savage man a just impression of the power we possess, and in what manner we can exercise it when justice demands reparation for insulted dignity."

The one-time advocate of scientific discovery and research had at least for the time being lost his sense of proportion and carried forward his preparations for the great Exploring Expedition uttering the most jingoistic sentiments. Curiously enough, it turned out that the "naval clique", as Reynolds called his imaginary enemies, were the ones who finally sent the Expedition to sea with scientific objectives.

There were sundry favorable committee reports,[18] and, ere long, a bill was passed authorizing the outfitting of the great Exploring Expedition. Reynold's influence was at the crest, and if

[18] 23rd Congress, 2nd Session, House Rept. no. 94, Feb. 7, 1835. Pearce, James A. Report from the Committee on Commerce, praying that an Exploring Expedition to the Pacific Ocean and the South Seas may be authorized by Congress. (Includes memorials by various persons requesting the fitting out of an Exploring Expedition.)

he had been patient, especially if he had not aroused the mistrust and antagonism of Dickerson, he might have managed matters in pretty much his own way. In 1836 he seems to have had every prospect of being not a mere "historiographer", which was originally his ambition, but the head of the civilian scientific corps. His campaign had reached a climax on the evening of April 2, 1836, when Congress met to listen to the great plan that would reflect glory on the country and profit its maritime ventures.[19] On May 14 an appropriation of $300,000 was made, and President Jackson instructed Dickerson that "prompt measures be taken to prepare and complete the outfit".

With Reynolds the prospective "scientific" head, there was hope in some quarters that Nathaniel Hawthorne might find a way out of numerous troubles that beset him by applying for the post of "historiographer". He did so, and was supported in his application by Franklin Pierce.[20] However, all of Reynolds's high hopes were dashed as a result of the continuation of his hysterical attacks upon Secretary Dickerson. In 1837 and 1838 he published a series of anonymous letters (everyone knew who wrote them) in the *New York Times* and the *New York Courier and Enquirer*,[21] which were answered by Dickerson. Acrimonious in the extreme, Reynolds wrote: "I resolved to arraign the Secretary at the bar of public opinion regardless of all consequence to myself".

The consequences were swift and relentless. Dickerson dropped Reynolds, "acidly insinuating", as Almy says, "that Reynolds regarded the expedition as his private affair, and that his claim to the role of scientist was not a little pretentious".

At this juncture, Joel R. Poinsett was requested to take over the official responsibility for the conduct of the Expedition.

[19] Reynolds, John N. Address on the subject of a surveying and exploring expedition to the Pacific Ocean and South Seas, delivered in the Hall of the House of Representatives on the evening of April 3, 1836, with correspondence and documents. N. Y., Harper, 1836. 300 pp.

[20] Hawthorne, Julian. *Nathaniel Hawthorne and his Wife: A Biography.* Cambridge, Printed at the University Press. 1884. (The correspondence will be found in Vol. I, pp. 152–164.)

[21] Reynolds, John N. and Dickerson, Mahlon. Exploring Expedition, Correspondence between . . . under the respective signatures of "Citizen" and "Friend to the Navy," touching the South Sea Surveying and Exploring Expedition; wherein the objects of the enterprise, and the causes which have delayed its departure, are canvassed. Originally published in the *New York Times* of July, August, and September, 1837, and in the *New York Courier and Enquirer* of December and January, 1837–38, n.d., n.p., pp. 151.

Only two weeks before the time of sailing, Reynolds in deep depression humbly applied to Poinsett for permission to accompany the expedition as a volunteer, at his own expense. The answer, dated August 1, 1838, was a bitter blow. It read: "Your desire to accompany the expedition is natural, and, under ordinary circumstances, your having, in some measure, originated the design, would give you a strong claim to be indulged in your wishes; but all subordinate considerations must yield to the paramount one of conducing the expedition to a successful issue."

Reynolds [22] in a new volume set before the public his side of the affair. His books probably were in part responsible for the unfriendly and unappreciative reception given Wilkes upon his return in 1842, for the public agreed with Edgar Allen Poe that Reynolds had been "most shamefully" treated, and was not disposed to give an unprejudiced hearing to the report of accomplishments, however great, in which Reynolds had been robbed of participation.

The final formulation of the objectives of the Expedition fell to Poinsett and to James Kirke Paulding, Dickerson's successor. An examination of the background of these men shows clearly why the emphasis was put so predominantly upon natural science and geographical exploration, and why "National Glory", as measured by naval display, was relegated to the background, as building up commercial prestige was likewise.

Both Poinsett and Paulding were widely cultured men who had fallen under the influence of a "thirst for the Natural Sciences that seemed ready to pervade the United States like the progress of an epidemic", as Amos Eaton said, who was one of the greatest American pioneers in botany and geology. According to G. Brown Goode [23] "in 1826 there were in existence more than twenty-five scientific societies, more than half of them especially devoted to natural history, and nearly all of recent origin". Many of the new societies started with great vigor, then dwindled away and disappeared, but in not a few instances their work was important while it lasted, and was perpetuated

[22] Reynolds, John J. *Pacific and Indian Oceans; or, the South Sea Surveying and Exploring Expedition; its Inception, Progress, and Objects.* New York, Harper and Bros., 1841. 10 + 516 pp.

[23] Goode, George Brown. "The beginnings of American science: the third century." *Proc. Biol. Soc. Wash.*, 4: 9–94, 1888. (Reprinted in *Memorial to G. B. Goode,* Report U. S. Nat. Mus. for 1897, part 2, pp. 407–466. Washington, 1901.)

through the young men that they inspired, the libraries and museums that they founded, and the scientific projects which they initiated. To each of them, a few leading spirits were so important that after the older ones had passed away and the younger had been drawn away to some greater field of activity, there was not enough life left to keep a society alive. So most of the smaller societies experienced dissolution, but not without having given their strength to greater ones.

One of them was the Literary and Philosophical Society of South Carolina, founded in 1813.[24] Its leader was Charleston's distinguished botanist, Stephen Elliott, and it contributed greatly to the movement for public museums in the United States. One of its strongest and most eminent personalities was the diplomat and politician, Joel R. Poinsett.

With a European education and a background of extensive travel, Poinsett took great interest in exotic natural history, an interest which must have been stimulated soon after his return from a diplomatic mission to Chile, by the purchase and installation in the Charleston Museum of an extensive collection made in Guadaloupe by Dr. Felix L'Herminier. Thomas Sumpter, United States Minister to Brazil, sent the Museum valuable collections of minerals, birds and insects from Chile and Brazil, setting a precedent for Poinsett's own later gifts to the Charleston Society and to the American Philosophical Society of specimens collected by himself in Mexico. Poinsett saw the advantage that could accrue to science from the National Exploring Expedition, and, as Secretary of War, in 1838 took over from the disgruntled and disgusted Dickerson the launching of a project that had suffered as much from its friends as from its enemies.

James Kirke Paulding succeeded Mahlon Dickerson as Secretary of the Navy from 1838 to 1841. He had previously (1825–1838) been Navy Agent at New York and was therefore conversant with the aims and accomplishments of another society very similar to that of Charleston, namely, the United States Naval Lyceum, which was established at the Navy Yard near New York in 1833. Deploring that, for want of a general place of deposit, the collections of specimens made by naval officers in

[24] For historical data throughout this paper I have drawn heavily upon Max Meisel's *Bibliography of American Natural History: The Pioneer Century, 1769–1865* (3 vols., Brooklyn, 1924–1929), a work indispensable to all who concern themselves with the history of the natural sciences in this country.

foreign service had been scattered and had become almost a total loss to the cause of science, the officers of the Navy and the Marine Corps undertook to build up a permanent library and museum, and to further the cause of natural history. The Lyceum remained more or less active until 1891, when the museum and library were transferred to the Naval Academy. It can hardly be doubted that the existence of the Lyceum gave many young officers an inclination toward work in the natural sciences. Among the participants in the work of the Lyceum were the officers of the U. S. ship "Peacock", which in 1833 had recently returned from a cruise in the China and Indian Seas. Paulding is chiefly remembered as a humorist and essayist, and for his joint authorship with his brother and Washington Irving of *Salmagundi*. He was not himself a naturalist.

Poinsett undoubtedly and almost certainly Paulding early had in mind that the successful conclusion of the Expedition might result in the establishment of a National Museum that would become a center of scientific research. Poinsett, from at least as early as 1838, gave the matter much thought, and was aided in his plans not only by Paulding but by several residents of the District of Columbia, including Peter Force, the Mayor of Washington, and not a few others.

On May 15, 1840, when the Exploring Expedition was about half through its cruise, "The National Institution for the Promotion of Science" was organized in Poinsett's house at Washington, with only eight persons present. Its objects were to promote science and the useful arts and to establish a National Museum of Natural History. Two directors were chosen to hold office until the annual meeting in January, 1841. They were Poinsett and Paulding, Secretaries of War and of the Navy, respectively. Poinsett became the first president of the Institution and served until 1844. It is quite clear that one important reason for the founding of the National Institution (later "Institute") was that the time was drawing near when there would have to be a repository in Washington for the collections of the Exploring Expedition.

When part of the collections actually arrived in Washington in advance of the return of the Expedition, the National Institute, by virtue of an informal arrangement with Secretary Paulding, took over their care under a somewhat divided au-

thority, shared by the Institute and the Commissioner of Patents, for the only space available in a government building was in the Patent Office.

Among early applicants for the curatorship of the Expedition collections was young Spencer F. Baird of Carlisle, Pennsylvania, whose naturalist-brother William wrote Spencer from Washington in July 1841: "I have been to the Patent Office for a little while in the evening after dinner. The collections of specimens which have been sent home are enormous. . . . Although three or four persons are engaged in stuffing and mounting birds and cleaning shells, minerals, etc., not more than one or two birds are finished in a day." Spencer applied to the National Institute for a position, and a year later had the support of Audubon, who wrote in his behalf under date of July 30, 1842: "Knowing, as I do, Spencer F. Baird, Esq. as a young gentleman well qualified to assist in the arrangement, description, etc., of the specimens of natural history brought home by the Exploring Expedition, and deposited in the National Institute at Washington City for the purpose of being published and thereby rendered useful to the world of science, I take great pleasure in recommending him as a most worthy, intelligent, and industrious student of nature, both in the field and the museum, and I would feel great satisfaction in hearing that our government had employed him in this national and important undertaking."

Dall,[25] from whose interesting biography of Baird this letter has been quoted, commented: "This proposition, however, came to nothing. Political influences prevailed, as one might expect from Congressmen, some of whom had been willing to finance a Symmes expedition. A janitor and preparator in the person of a kindly and well intentioned old man, one or two taxidermists, with a Dr. King as Curator, and a clergyman, doubtless a worthy theologian, were assigned to the work of caring for the collections until the return of Captain Wilkes."

The National Institute has sometimes been tacitly or openly blamed for the destruction and confusion of collections at the hands of this "worthy theologian", who dried many alcoholic Crustacea "for exhibition", impaling small soft types on pins,

[25] Dall, Wm. Healey, *Spencer Fullerton Baird: a Biography, including selections from his Correspondence with Audubon, Agassiz, Dana, and Others.* Philadelphia and London, J. B. Lippincott Company, 1915. (See pp. 78–79.)

discarding the data; who meticulously removed the tin-foil labels from the alcoholic specimens of naked (and some of the shelled) mollusks, one by one, and mixed them together in a bottle, thus making precise citation of localities impossible, etc. However, there is no evidence that the Institute had been responsible for this person's appointment, or that it ever had sufficient influence to determine who should be appointed. Politicians insistently urged the claims of their friends for such appointments, regardless of the qualifications required, and, with appropriations under the control of Congress, even cabinet members had to recognize that the wishes of Congressmen could not be too far disregarded, especially with regard to apparently minor positions. William Baird's letter shows that his brother's application was at least later than some of the appointments as taxidermist, etc., and after the return of the Expedition members of the expeditionary staff were employed as curators.

The whole history of the genesis of the National Museum has been told by G. Brown Goode,[26] and it makes a most interesting tale. Of course provision for taking care of the collections of the Expedition was just one of the objectives of Poinsett's very comprehensive plan [27] which called for establishing just such an organization as the Smithsonian Institution has become. The National Institution was founded at a time when Congress was embarrassed by more advice about what to do with the bequest of James Smithson than it could digest. So Poinsett in 1840 deliberately set up a model institution on lines that Congress might well approve for the future Smithsonian. By placing a model before the eyes of Congress, showing how it would work, and then asking financial support for it, in the form of the Smithsonian endowment, he hoped that he might bring about the transformation of the model into the completed structure. He constantly advocated the fusion of the Smithsonian with the National Institution.

There was strong support for the idea. Dr. Peter Du Ponceau, president of the American Philosophical Society, in a let-

[26] Goode, G. Brown. "The genesis of the National Museum." Ann. Report of the Board of Regents of the Smithsonian Institution for the year ending June 30, 1891. *Rep. U. S. Nat. Mus.:* 273–380, 1892.

[27] Poinsett, Joel Roberts. Discourse on the objects and importance of the National Institution for the Promotion of Science, established at Washington, 1840, delivered at its first anniversary. Washington, Peter Force, 1841. (Not seen.) Reprinted in *Proc. Nat. Inst. Prom. Sci.*, **1**: 1–65 (Bulletin 1), 1841.

ter [28] to the Institution in November 1840, wrote: "Congress can not find a better opportunity to execute the will of the beneficient testator than by laying hold of your institution and making it its own." Ex-President John Quincy Adams, at first advocating another idea, and suspicious of Poinsett, finally wrote in his diary under date of April 17, 1841: "Mr. Poinsett called upon me and now fully disclosed his project, which is to place the investment and disposal of the Smithsonian funds under the management of the American [*i.e.*, National] Institution for the Promotion of Literature and Science. . . . He said that he had at present no other occupation on hand and would be willing to devote two years entirely to organizing the establishment and getting it into full operation. . . . I know not that it could be accomplished more effectively, and think I must acquiesce in this arrangement and endeavor to carry it through."

Already, in February, as Representative Adams might have known, Senators Linn and Preston, both members of the National Institution, had introduced unsuccesful bills to carry into effect Poinsett's proposal. Congress was too confused by conflicting opinion to come to a favorable decision, and too suspicious, it would seem, of the very success of the National Institution.

It was Du Ponceau who suggested that "Institute" would be a more dignified appellation than "Institution", "as expressive of some degree of superiority", and that a change of name might be an auspicious move if the Smithsonian Institution were to be taken over by the National "Institution". Du Ponceau wrote: "My idea would be to call the national establishment the 'National Institute for the Promotion of Science' and the subordinate one the 'Smithsonian Institution,' without more." So, at the time of the incorporation of the society by Congress, July 27, 1842, it became "Institute", but, as Goode remarks, the change did not make it any more prosperous. Likewise, it experienced a serious blow in 1842 through the loss of jurisdiction over the collections of the Exploring Expedition, which it had assumed with insufficient authority through informal agreements which it had with the Navy Department, and which was taken away as the result of the hostile activities of persons who well concealed their identity, but convinced Congress that the Insti-

[28] Du Ponceau, Peter Stephen, "Letter respecting the Institution in general; its organization, plan of bulletin. . . ." *Proc. Nat. Inst. Prom. Sci.*, 1: 10–13, 90–92, 1841.

tute, a private corporation in spite of the fact that its directorate included an ex-President of the United States, members of the cabinet, *ex officio,* with other public men of the highest distinction, "was unworthy of the patronage of the Government" and should not be entrusted with the custody of Government property. As a result, Pickering, who had been Curator of the Expedition's collections under the Institute, resigned, and the special custodianship of the Expedition collection "for the purpose of preparing an acount of it" fell to Wilkes, appointed by the Congressional Joint Committee on the Library, but the general custody to the Commissioner of Patents.

In taking over the functions that had previously been exercised by the Institute, Wilkes, on September 16, 1843, wrote to Col. J. J. Abert, one of the Institute's most zealous officers, ever watchful of its interests, as follows:

> I will now close with a few words respecting the last clause of your letter relative to my feeling any "unkindness" towards the National Institute. It is rather improbable that any unkindness or hostility would exist on my part considering that the labors of the expedition, combined with the exertion of your gifted president (Mr. Poinsett) were the origin of it [*i.e.*, the Institute] and that in all probability it may one day become the depository of the large and valuable collection of the exp'g expedition, therefore I cannot but feel deeply interested in its welfare—everything compatible with the performance of my public duties will always be done to accommodate and assist its rise and progress.

The Institute lost prestige very seriously as a result of this strategic defeat by enemies who now busied themselves in spreading the statement that now that the Institute had lost control of the collections of the Expedition, it had nothing left that would be worthy of acceptance by the Government.[29] This was

[29] Goode (*l.c.*, p. 318) says the enmity arose in Mr. H. L. Ellsworth, Commissioner of Patents. "Mr. Ellsworth was evidently bent upon dislodging the National Institute from the Patent Office. To effect this he pursued the not altogether ingenuous course of belittling the Institute, its work, and the extent of its cabinet, and laying claim to the official possession of more important collections of models, fabrics, manufactures, which, in accordance with the act of 1836, reorganizing the Patent Office, he designated as the 'National Gallery,' a name which he also applied to the great hall in which all the collections were deposited. The Commissioner of Patents was evidently legally in the right, and the Institute found itself bereft not only of its command of Government collections, but also of its hall." Mr. Goode doubtless knew, but refrained from telling, which particular Commissioner of Patents it was who, between 1850 and 1860, got rid of the Government's collection of vertebrate fossils, including bulky Megatherium bones, by sending them to a bone mill in Georgetown where they were ground into commercial fertilizer. "Once for thought, they now became food for the farmer's crops!"

far from true, for the Institute had brought together an unbelievably large mass of valuable scientific materials and books in the short time it had been active and, by the conditions of its charter, was "in reference to all its collections in reality a trustee for the United States".

By 1843 the Institute had actually spent so much for books and for expenses of scientific exchanges and organization that it was in a desperate financial state. How closely the activities of the Expedition had been built into its structure is shown by the fact that when Wilkes replaced Pickering in the curatorship of the collections, James Dwight Dana, Horatio Hale, J. P. Couthouy, and W. D. Brackenridge of the Expedition's scientific corps were also serving the Institute in a curatorial capacity. As Goode said: "It was not to have been expected, however, that its authority should have remained long unquestioned, and in the end its lot was that which very frequently befalls those who, out of disinterestedness, undertake, unasked, to forward the interest of others . . . [for] the merit of the Institute was turned to its misfortune, and its 'voluntary zeal' was thought totally unworthy of recognition."

It was therefore unsuccessful when its financial needs were forcibly called to the attention of Congress in June, 1844.[30]

Poinsett did not admit defeat until the end of 1844. He had attempted to demonstrate the breadth and national significance of his conception by convening the first national scientific convention under the auspices of the National Institute at Washington, April 1–10, 1844. The metings were an imense success, and the Institute met with the wholehearted commendation of the country's leading educators and scientists. Again it hopefully petitioned Congress for financial support, which was not forthcoming, in spite of the fact that a special committee made a very earnest and commendatory favorable report,[31] and that aid

[30] The general content of the petition, which was put in final form by Mr. J. R. Ingersoll, was determined at a meeting held December 23, 1843, in the office of the Secretary of State. Goode (*l.c.*, p. 294) writes: "That the society was regarded at that time as one of national importance is shown by the presence at the meeting of Mr. Upshur, the Secretary of State, who took an active part in the proceedings; the Hon. John Quincy Adams, who presided; Sanator Levi Woodbury, late Secretary of the Treasury, who agreed to represent the meeting in Congress; the Hon. J. R. Ingersoll; Senator R. J. Walker; besides Col. Peter Force, Col. Abert, Col. Totten, Lieut. Maury, and the officers of the society."

[31] 28th Congress, 2d Session. Rep. No. 130. Ho. of Reps. National Institute (To accompany bill H.R. No. 615). February 15, 1845. [4 pp.]

might have been given by settling upon the Institute the Smithsonian bequest.

Poinsett had advocated museum activities for the Smithsonian from at least 1838, and was the first who did so. He convinced President Tyler that the National Institute should be identified with the future National Museum and the future Smithsonian Institution, and that the Government should "continue to it a fostering care". Promptly in 1842, when the Expedition returned, he had a group of curatorial experts at work upon the Government collections that Goode said was "a force fully as effective as thirty years later, in 1873, when [he] first became acquainted with the operations of the Smithsonian Institution".

Poinsett and his friends had done all in their power to accomplish a very worthy end. In the very beginning they had attempted suggestion, by paraphrasing or actually quoting the terms of the Smithson bequest in describing the purposes of the National Institute. Later they were frankly outspoken about their wishes. Goode believed that the Institute had made the mistake of being too successful, and had excited jealousy. At the most critical time, in 1844, it had ex-President John Quincy Adams, then in Congress, enrolled among its most sincere friends [32] but neither his strong advocacy nor that of many others

Mr. Barnard, from the Select Committee, to whom was referred the memorial of officers of the National Institute, reports: "The institution . . . was incorporated by act of Congress on the 27th day of July 1842, by the name of the National Institute for the Promotion of Science.

"The Secretaries of State, the Treasury, War, and the Navy, the Attorney General and the Postmaster General are to be directors *ex officio*, and the residue of the officers are to be chosen annually from the resident members. . . .

"The institute was immediately organized under the charter, and has ever since been in active and successful operation. It became possessed of valuable collections possessed by the Columbian Institute, a society whose charter had expired, and the National Institution, a voluntary association previously organized. . . . Although the institute is a private corporation, it has been, to a certain extent, recognized as a national institution. . . . Foreigners and strangers have thus come to consider the institute as an appurtenance of the government. . . .

"There is another point of view in which the institute is worthy of the favorable regard of Congress. [Then follows commendation of its meetings.]

"The select committee report, therefore, a bill in behalf of the institute."

32 Adams closed an address to the scientific convention of 1844 with these words:

"I avail myself of this occasion to express my regret that, having taken an humble part in the establishment of this Institution from its first foundation, under the auspices of Mr. Poinsett, I have been able to contribute so little to its promotion and advantage, and to add my heartfelt satisfaction at the prosperity, which, by the untiring exertions and fervent zeal of its executive officers, it has attained. I

only less distinguished availed to move Congress, which killed Poinsett's hopes, only to build anew, before long, on essentially the same plan.

After the collapse of his scheme, Poinsett withdrew from the presidency of the Institute, which he had held from 1840 to 1844. The society rapidly declined. It had, however, succeeded in acquiring many scientific collections and the nucleus of a scientific library, housed in the Patent Office, which, after the demise of the Institute, the charter of which expired in 1862, were transferred to the Smithsonian. Other functions of the National Institute had already by that time been duplicated by the Smithsonian. Thus, eventually, Poinsett's great idea came to fruition and the Smithsonian Institution was organized about as he had hoped it would be in the first place, except that the new institution had lost one function of leadership, anticipated by the initial great national scientific meeting of 1844, namely, organization of American science, a function never assumed by the Smithsonian, and soon taken over by the American Association for the Advancement of Science and the national societies in the various fields of science.

I hope that I have made clear the bearing of all this history on the Wilkes Expedition. Without Poinsett's keen interest in natural history and in furthering the museum movement, the Expedition might really have ended as an ephemeral gesture in furtherance of the whale fishery and the expansion of trade. Certainly Reynolds, intoxicated with the idea of making a pompous display of sea power, may be suspected of having lent little more than lip service to the cause of science in connection with his later advocacy of the Expedition. He had learned too much about the Antarctic by personal experience to wish to have the Expedition go there. He had become sensitive about his early belief in "Symmes's hole", and nothing annoyed him more, during his controversy with Dickerson, than the Secretary's insistance that Antarctic exploration should continue to be, as Reynolds had first wished it to be, a major objective of the Expedi-

believe it eminently deserving of the fostering care and liberal patronage of the Congress of the United States, and could anticipate no happier close to my public life than to contribute, by my voice and by my vote, to record the sanction of the nation's munificence to sustain the National Institute devoted to the cause of science."

tion. Reynolds wanted "national glory" and wished very much to participate personally in it.[33]

[33] One may read between the lines that Dickerson took a mean delight in tormenting Reynolds about the Antarctic, now that his opponent seemed to be much more concerned about the Expedition staying away from it than exploring it. Reynolds's exasperation was shown in his letters to the *New York Times*. He seems to have had information (through private advices from someone in Secretary Dickerson's office) that a board of naval officers had been instructed to make approximately these plans for the expedition, namely, "to explore the seas of the Southern Hemisphere, more particularly in high latitudes, and in regions as near to the South Pole as may be approached without danger; to make, in the regions thus to be explored, all practicable surveys and observations, with accurate descriptions of the same, so far as they may be connected with the geography or hydrography by which the interests of commerce and navigation may be promoted".

Angrily Reynolds comments· "Perhaps you may have dropped a word about science at the close, and intimat‹ ‹ that the vessels might, during the cruise, go north of the line; though for what purpose you do not say.

"This, if I am not misinformed, is the breadth, and length, and altitude of your instructions, if not the very words.

"Why did you forget—no, why did you omit the major part of your subject? the great commercial interests among the islands of the Pacific, and the thousand ways in which those noble interests might be examined, extended, and secured by this expedition? Have not the memorials from Nantucket, New-Bedford, New-London, Salem, and other great commercial places, given you any light upon the subject? Have they not, 'in thoughts that breathe and words that burn', told you, through Congress, the difficulties, the dangers our fisheries have to encounter in those seas? Have they not, in the deep impassioned feeling of their hearts, implored their country to look after their brethren in bondage on desolate or savage islands? And you, in your instructions to this gallant board, have mentioned but little more than the object of getting as near as possible to the South Pole, *and there to make surveys for the benefit of commerce!* This same plan of misrepresenting the objects of the expedition was tried by its opponents last winter before Congress, and failed. Do you expect to be more successful in urging the same plea before this board?" (From letter dated at New York, June 29, 1837.)

Dickerson retorted (letter of August 25, 1937) that "Citizen's" vehemence was uncalled for except by his own imagination, that although five-sixths of the time would be spent in low latitudes, nevertheless "the results of the examinations in high latitudes, in this one-sixth of the time, will be looked to with more intense interest than any others of the whole cruise. The 'Citizen' is now endeavoring to divert the attention of the public from these examinations in high southern latitudes . . . he has determined to convert this surveying and exploring expedition into an expedition for the protection of commerce; the objects of surveying and exploring being considered by him of secondary importance. In this he will fail . . .

"But if the 'Citizen' shall succeed in changing the character of this expedition from peaceful to warlike, he will still be perplexed with difficulties. Suppose, in clearing the decks . . . for action, what a scene of confusion—skeletons and bones of animals of all kinds, testaceous, crustaceous, vertebrated and invertebrated; heaps of molluscous treasures; alligators stuffed, 'and other skins of ill-shaped fishes', must all, all be tumbled into the ocean without reserve, *'rari natantes in gurgite vasto'*.

"The 'Citizen' had much better permit this expedition to remain what it was intended to be from the beginning, a peaceful surveying and exploring expedition. . . .

"Terror is the favorite means of the 'Citizen' for carrying his points. His threats are awful. . . . Don't, Mr. Giant; do not, in your wrath and fury, play the part of the terrible lion,

Poinsett's whole disposition, after he took over the Expedition, was to make it truly scientific in its major objectives, and it would be absurd, in view of the record of himself and Paulding, to believe that they were enemies of the expedition or enemies of science, as Reynolds and his proponents alleged they were. One of the amazing things about the expedition was that it stirred up such lasting and vitriolic personal prejudices and animosities. Reynolds and his friends originally said they wanted a scientific and exploring expedition. When exactly that was finally equipped and dispatched, their ire knew no bounds.

If it had not been in anticipation of the enormous collections in natural history that would have to be taken care of, there would have been no immediate need to plan and build up the National Institute. Without the Institute, no place would have been provided for the collections, which, if dissipated to institutions throughout the country for study, would never have been reassembled to form the substantial basis for a National Museum. Wilkes was eventually to be harshly criticized for wanting reports written in Washington, then "a city without books", but by adhering as inflexibly as he could to an arbitrary policy that the collections were to remain in Washington he performed a great service to the nation.

So without the Expedition there would have been no National Museum, at least not until some later emergency should have aroused a need as critical as that created when Poinsett anticipated the receipt of the Expedition specimens. There might even have been no National Museum yet! That this suggestion is not preposterous is indicated by the fact that only very lately have we had centralized and properly administered National Archives.

Goode has emphasized the importance of the fact that a "Memorial of the Friends of Science who attended the April meeting of the National Institute", signed in 1844 by nearly forty representative scientific men and college presidents from all parts of the United States, especially commended the extent and value of

'Who roared so loud, and looked so horrid grim
His very shadow durst not follow him.'

"If the 'Citizen' will only put off the lion's skin, and cease, by his awkward attempts at roaring, to frighten folks who do not know him, the affairs of the exploring expedition will go on much more smoothly."

the museum material. He said: "This endorsement of the museum work of the Institute is very cordial and comprehensive, and very significant; is indicative of a decided growth in public opinion in regard to museums—a growth largely due in the first instance to the suggestions and later to the fostering care of Mr. Poinsett and his society, the National Institute. . . .

"The influence of the National Institute upon the history of science in the United States, and particularly in educating public opinion and the judgment of Congress to an application of the proper means of disposing of the Smithsonian legacy, can not well be over-estimated.

"If the Smithsonian had been organized before the National Institute had exerted its influences, it would have been a school, an observatory, or an agricultural experiment station.

"In 1846, however, the country was prepared to expect it to be a general agency for the advancement of scientific interests of all kinds—as catholic, as unselfish, as universal as the National Institute."

To this I would add that if the plan of the Smithsonian Institution grew out of Poinsett's Institute, the Institute itself became a necessity because of the Expedition.

However much credit should go to Reynolds for promoting the Expedition in the first place, and however much we may regret that he had to bear the disappointment of not being connected with it, we can realize, in the light of history, how fortunate it was for the advancement of science in America that the final activities of the expedition were determined by Poinsett, one of the truly remarkable characters of our early national life.

Secretary Paulding wrote the "Instructions" for the conduct of the Expedition. Included in that document [24] we find the names of the scientific staff, most of whom wrote final reports. Asa Gray had been originally chosen as Botanist, but withdrew in order to take on other duties at a time when it appeared that the sailing of the Expedition might be indefinitely delayed. He finally, however, participated energetically and effectively in the preparation of the botanical reports. The other appointees were as follows:

34 Printed in Vol. I of Charles Wilkes's *Narrative of the United States Exploring Expedition.*

Horatio Hale, Philologist
Charles Pickering, Naturalist
Titian Ramsay Peale, Naturalist
Joseph Pitty Couthouy, Conchologist
James Dwight Dana, Geologist
William Rich, Botanist
William D. Brackenridge, Horticulturist
Joseph Drayton, Artist
Alfred T. Agate, Artist.

All of the civilian scientists except Couthouy and Rich wrote reports which were published. Peale's, to be sure, was "withdrawn'" but not until a sufficient number of copies had been distributed so that it must be considered as having been published. Charles Wilkes, Commander of the Expedition, wrote the *Narrative,* and the two special reports on Meteorology and Hydrography.

Collaborators who did not go on the expedition, but worked up part of the materials, were Augustus Addison Gould, who wrote the volume on Conchology in place of Couthouy; John Cassin, who wrote a volume on the mammals and birds, to replace Peale's; Asa Gray, John Torrey, William Starling Sullivant, Edward Tuckerman, Jacob Whitman Bailey, W. H. Harvey, M. A. Curtis, and M. J. Berkeley, all of whom worked on the plants, utilizing Brackenridge's, Rich's and Pickering's collections; Charles Girard, who published on the Herpetology. Another collaborator was Louis Agassiz, whose two volumes on Ichthyology were never published.

How eagerly American naturalists looked forward to the publication of the reports of the Expedition, and what at least one of the greatest of them thought the publication policy should be, we learn from a letter written by Audubon which I quote from Dall's admirable biography of S. F. Baird. It was addressed to his "dear young friend" Baird, who was eventually to help complete the publications by undertaking the "superintendence" of Girard's report on the reptiles and batrachians. Dated July 30, 1842, the part concerning the Expedition is as follows: "I have some very strong doubts whether the results of the Antarctic Expedition will be published for some time yet;

for, alas, our Government has not the means, at present, of paying some *half a Million of Dollars* to produce publications such as they should publish, and connected with the vast stores of Information, collected by so many Scientific Men in no less than Four Years of Constant Toil and privation, and which ought to come to the World of Science at least as brightly as the brightest rays of the Orb of Day during the Mid-summer Solstice. Oh, my dear young friend, that I did possess the wealth of the Emperor of Russia, or of the King of the French; then indeed I would address the Congress of our country, ask of them to throw open these stores of Natural Curiosities, and Comply with mine every wish to publish, and to *Give away* copies of the invaluable works thus produced to every Scientific Institution throughout our country, and throughout the World.''

I shall now review most of the published volumes, omitting all reference or all but the briefest mention of those which have been assigned to other participants in this program. In doing so I shall touch only lightly upon the vexed bibliographical puzzles presented by the reports, since there has just been a preliminary publication on the subject by Mr. Daniel C. Haskell,[35] Bibliographer of the New York Public Library, which is to be followed by a full bibliography later in the year. His exhaustive work will give an account of the numerous variants of the volumes, some of which have been discussed by the botanist F. S. Collins.[36] I shall therefore confine my bibliographic efforts to the accurate citation of those particular volumes which have been accessible to me, thus taking a precaution rather than attempting to make a contribution to the subject.

The University of Michigan has a full set of all the Reports that were printed, except one volume, and one part of another, and these are the ones that I have used. Most of the volumes are of the official issue, but some are the variants published for the trade. I have had access to other editions of the *Narrative* and to the unofficial issue of one of the special reports of which the official edition is in the Michigan set.

[35] Haskell, Daniel C., *The United States Exploring Expedition, 1838–1842, and its Publications, 1844–1874.* Bull. N. Y. Public Library, 44: 93–112, 1940.

[36] Collins, Frank S. ''The botanical and other papers of the Wilkes Exploring Expedition.'' *Rhodora,* 14: 57–68, 1912.

THE NARRATIVE, VOLS. I–V, AND ATLAS

The bibliographic difficulties presented by the reports of the Exploring Expedition begin with the *Narrative,* by Wilkes [37] himself, in five volumes and atlas. The official edition was so small that few have ever seen it. The unofficial issuance of another edition was practically forced upon the author, if he wished to insure effective publicity, by the unreasonable restriction placed by Congress upon the size and manner of distribution of the official edition, which was the only one originally provided for. Chapter 204 of the Acts of 1842 provided for the publication of only 100 sets. No definite plan seems to have been made for their distribution until after the actual printing was completed of the official edition of the *Narrative,* which presumably took place between November, 1844, and February, 1845. Since Wilkes's introduction was dated November, 1844, and the joint resolution of Congress, approved February 20, 1845, provided for the distribution of 63 of the 100 copies that had been ordered,

[37] United States / Exploring Expedition. / By Authority of Congress. [Half-title.]

Narrative / of the / United States / Exploring Expedition. / During the years / 1838, 1839, 1840, 1841, 1842. / By / Charles Wilkes, U.S.N., / Commander of the Expedition, / Member of the American Philosophical Society, etc. / in Five Volumes, and an Atlas. / Vol. I. / Philadelphia: / Printed by C. Sherman. / 1844.

Vol. I, lxvi + 455 pp., 8 pl., 15 vignettes, 67 woodcuts, 1 map.

Vol. II, xvi + 505 pp., 14 pl., 14 vignettes, 46 woodcuts, 3 maps.

Vol. III, xv + 463 pp., 11 pl., 10 vignettes, 50 woodcuts.

Vol. IV, xvi + 574 pp., 16 pl., 3 vignettes, 35 woodcuts, 1 map.

Vol. V, xv + 591 pp., 15 pl., 5 vignettes, 48 woodcuts, 4 maps.

Atlas. / Narrative / of the / United States / Exploring Expedition. / During the years / 1838, 1839, 1840, 1841, 1842. / by Charles Wilkes, U.S.N., / Commander of the Expedition, / Member of the American Philosophical Society, etc. / in Five Volumes, and an Atlas. / By Authority of Congress. / Philadelphia: / Printed by C. Sherman. / 1844.

[Contents.]

Chart / of the World / Showing the Tracks / of the / U. S. Exploring Expedition / in 1838, 39, 40, 41 and 42. / Charles Wilkes Esq., / Commander. [Tinted with water color.]

Chart / of the / Antarctic Continent / Showing the Icy Barrier Attached to it. / Discovered by the / U. S. Ex. Ex. / Charles Wilkes, Esq. / Commander / 1840. [Not tinted.]

Chart / of the / Viti Group / or / Feejee Islands / by the / U. S. Ex. Ex. / Charles Wilkes, Esq. / Commander / 1840. / Eng. by Sherman & Smith, N. Y. [Not tinted.]

Map / of the / Oregon Territory / by the U. S. S. Ex. Ex. / Charles Wilkes Esq. / Commander / 1841. / Columbia River / Reduced from a Survey / made by the / U. S. Ex. Ex. / 1841. / J. H. Young & Sherman & Smith New York. [Tinted to show the regions occupied by the several Indian tribes.]

Map / of / Part of the Island of / Hawaii / Sandwich Islands / Showing the Creaters and Eruption / of / May and June 1840 / By the / U. S. Ex. Ex. / 1841 / Sherman & Smith Sc. N. Y. [Untinted.]

but took no cognizance of the need for any additional number beyond the few that were to be held in reserve, it may fairly be assumed that Wilkes acted upon his own initiative in 1844 when he secured for himself the copyright for the *Narrative,* and arranged for the publication of a trade edition.[38]

[38] The Congressional resolution of February 20, 1845, regarding distribution of the reports, provided for the disposal of 63 of the 100 copies, as follows: 58 copies to be distributed by the Secretary of State, one to each of the United States; to the Governments of France, Great Britain, and Russia, two copies each; one copy each to Sweden, Denmark, Prussia, Austria, Bavaria, The Netherlands, Belgium, Portugal, Spain, Sardinia, Greece, Tuscany, The Ecclesiastical States, the Two Sicilies, Turkey, China, Mexico, New Granada, Venezuela, Chili, Peru, The Argentine Republic, Brazil, Texas, The Sandwich Islands, and to the Naval Lyceum in Brooklyn, New York. 1 copy each to Charles Wilkes, William L. Hudson, and Cadwallader Ringgold. 2 copies to the Library of Congress.

Subsequent resolutions provided for further distribution, as follows:

July 25, 1845; 5 copies to Alexandre Vattemare for exchange with foreign governments.

July 15, 1846; 1 copy to the State of Florida and 1 copy to each new State admitted to the Union.

March 2, 1849; 2 copies of Vols. 6 and 7 to Russia, to replace copies lost at sea, and 1 full set to Ecuador.

February 20, 1850; 1 copy to Oregon, 1 copy to Minnesota, 1 copy to each territory thereafter organized, 1 copy to Switzerland, 1 copy to the Naval Academy at Annapolis, Vols. 13 and 14 to Great Britain.

The Library of Congress has no record of the actual distribution that may have been made under these resolutions, but the interesting fact comes out that there seems never to have been any legal provision for the several authors to receive even a single copy of the official edition of their respective volumes.

I have drawn these facts regarding distribution from a memorandum by Mr. F. W. Ashley, Chief Assistant Librarian, transmitted by the Librarian of Congress to Dr. W. W. Bishop, Librarian of the University of Michigan, under date of September 25, 1933.

One's natural curiosity about Alexandre Vattemare is satisfied in part by the following information regarding the exchange relations of the National Institute, taken from a Congressional document.

"The Institute has attracted the favorable notice of many foreign governments, and of literary and scientific societies and individuals throughout the civilized world, and is in the constant receipt of interesting communications and valuable contributions from these various sources. In this connection the committee cannot forbear to express their high sense of the great value and importance of the zealous, able and disinterested labors of M. Alexandre Vattemare, of Paris, who has devised and put in operation a system of exchanges which promises to be of incalculable use as a means of facilitating the diffusion of knowledge and the general distribution of the most important productions of nature and of art. The zeal of this enlightened and disinterested individual has been of essential service to the Institute, and many highly valued contributions from continental naturalists, authors, and artists, are the fruit of his suggestions.

"The interest which M. Vattemare and other liberal and intelligent persons in various parts of the world have manifested in the prosperity of the Institute, has occasioned contributions so numerous, that the association (which has no funds, except such as are derived from a small annual tax upon the resident members) is unable to defray the charges of transportation, and has been compelled to request persons desirous of sending objects from abroad, to defer their transmission until funds can be provided. . . ."

As already noted, Wilkes got a personal copyright on the *Narrative.* This led an anonymous reviewer (perhaps Asa Gray, who kept closely in touch with the publications) to remark in the *North American Review* that since Wilkes had been well rewarded by the nation for his labors, he should not be permitted to copyright and profit by literary property, which, if the nation did not reserve it, should by rights belong equally to all the officers whose confiscated diaries and notes had been drawn upon to make the *Narrative.* In defense of Wilkes, it may be said that he probably hadn't a glimmering of an idea that the officers had any rights to their diaries, since it was he who had ordered them to be written, in accordance with his own instructions from the Secretary of the Navy.[39]

However high-handed Wilkes's action with regard to the copyright may have seemed, his plan for a trade edition resulted in a more liberal distribution of the reports than Congress was willing to provide for, and was therefore very commendable.

Wilkes's fellow officers and the scientific staff naturally took umbrage at his having personally secured a copyright on the *Narrative,* to which they all contributed, and a reading of the official orders pertaining to the Expedition (as they are printed in the *Narrative*) explains why. All officers of the Expedition were required to keep detailed journals which were submitted weekly to the commanders of the several ships and by them as often as circumstances permitted passed on to the commander of the Expedition. These journals were drawn upon by Wilkes for the extraordinarily lengthy *Narrative,* in which was incorporated not only all the factual material that Wilkes wished to make use of under his own name as author, but also an enormous

28th Congress 1st Session Rep. No. 539. Ho. of Reps. National Institute. (To accompany bill H.R. No. 417.) June 7, 1844. Report: The Joint Committee on the Library. Also: 28th Congress, 2nd Session. Rep. No. 130. Ho. of Reps. National Institute. (To accompany bill H.R. No. 615.) February 15, 1845.

Vattemare continued to further international exchanges of scientific publications and specimens for several years after 1845, as we know from stray mention of his activities in connection with the early history of the international exchange that was inaugurated by the Smithsonian Institution under Assistant Secretary Baird, and has continued to the present.

[39] As a matter of fact, even Wilkes was considered by the President to have no especial rights in the matter. Haskell's recent article states that President Tyler assigned to Robert Greenleaf, translator in the State Department, the duty of drawing up the reports and writing the journal of the expedition. Wilkes's friends in Congress blocked this appointment, and the Joint Committee on the Library finally drew up the plan for publications which was adopted.

amount of presumably documentary or published material which was acquired by various members of the expedition in the countries visited and which was utilized with apparently a minimum of acknowledgement of sources.

The appropriation by Wilkes of all the notes and journals may have been the reason why Wilkes tried to insist upon the reports being written in Washington, where all the records would be available. More than one of the scientists made very caustic comments about being expected to write reports in Washington, then a city almost without scientific books, and one of the scientists, in flagrant violation of a ukase to which he objected, took his original records to Europe.

Wilkes's general order under date of September 13, 1838, with regard to journals, required all the officers of the Expedition "to bestow their constant and devoted attention to all incidents, facts, or occurrences, which may present themselves. . . . This can only be effected," he said, "by keeping full and complete memoranda of all observations, made at the time, and entered in the journals. . . . The kind of a journal required . . . is a diary . . . of all objects of interest, however small, which may take place during the cruise, in the scientific or any other department. . . . I wish particularly to avail myself of the results and observations of all, to avoid the possibility of passing over any subject without full examination and remark".

Wilkes certainly made generous general asknowledgment to the other officers and to the scientists for the use of their literary materials, and probably did not even realize that each of the "scientific gentlemen," at least, might have preferred, much less that he had a right, to make the original publication of his own observations. The commander must be given credit for very promptly editing a vast amount of material, for his *Narrative* appeared only two years after the return of the Expedition, in spite of the mental turmoil and distraction that Wilkes must have experienced as a result of his court-martial and the unexpected blast of public disapproval and antipathy that broke out upon his return.

The volumes that followed the *Narrative* were not copyrighted. When the first of the strictly scientific reports appeared there was another review (this one was anonymous also, but was ac-

knowledged later by Asa Gray [40]) in the *North American Review,* in which the publication policy of Congress was severely condemned. Gray said: ". . . we are bound to call public attention to a serious error in respect to the mode, or rather the amount of publication, which, unless corrected in season, must render them forbidden fruit to nearly all the scientific world. We know something of the interest with which the appearance of these volumes is awaited, not only by the comparatively few laborers who represent the rising science of our own land, but especially by their numerous European brethren. Let our readers imagine their surprise and our mortification, when they learn that the edition ordered by the 'collective wisdom of the nation', or the more concentrated intelligence of the library committee of Congress, which has charge of the subject, is restricted to *one hundred copies!* It would be hard to contrive a more effectual plan for defeating the very object of publication. When it is remembered that much the larger part of this five score of copies will probably be absorbed in presents to foreign cabinets and to the State governments, it will be evident that few indeed are likely to be accessible to those who can really appreciate or profit by them. . . . We shall be the last to find fault with these beautiful volumes, printed on fine paper, with the utmost luxury of type and amplitude of margin. Still, if it be a question between an edition of a hundred splendid but inaccessible copies, and an adequate one in a cheaper form, surely no reasonable person, not even Congress 'can long debate which of the two to choose'. But no change is necessary in this respect, except the ordering of an additional impression of three hundred or five hundred copies, to be placed on sale—just as the charts of the Expedition are sold—at a price which will barely reimburse the additional cost. . . . Such a plan has, we believe, been recommended to the consideration of the library committee of Congress by the leading scientific societies of the country—with what success we have not yet learned. We can only add our protest against the present ill-advised scheme, which is preposterous on the score of economy, since nothing whatever is saved by it, and which, if persevered in, will be truly disgraceful to the country.

[40] [Gray, Asa] "United States Exploring Expedition. 1. The Zoophytes; by James D. Dana, A.M. . . . 2. Ethnography and Philology; by Horatio Hale, A.M. . . ." *North Amer. Rev.*, 63: 211–236, 1846.

"It has occurred to us, as we turned the leaves of these sumptuous volumes—though we like not to entertain the thought, that a pitiful pride may have had something to do in limiting the number of copies, so as designedly to give them the adventitious value of great rarity; that the library committee may have wished to imitate the equivocal patronage to science of some sovereigns, such as an emperor of Austria in the last century, for instance, who caused the works of Jacquin to be published in magnificent style, but in a very small number of copies, chiefly for distribution as presents, and then destroyed the plates that imperial gifts might not subsequently be cheapened.

" 'These are imperial arts, and worthy kings', perhaps in a former age,—though even royal patrons have now grown wiser; but they are quite unworthy of republican imitation."

Congress did indeed consider adopting a resolution to accede to the request for a more adequate edition, but did nothing officially beyond winking at an arrangement by which the printers, at their own responsibility and risk, printed 150 extra copies of Vol. VI, Hale's *Ethnography and Philology* and Vol. VII, Dana's *Zoophytes*. This matter came to Gray's attention in time for him to comment upon it in a footnote in his review, but he still protested at "the want of any guaranty for the continuance through the series of this unauthorized impression", and also at the insufficient number of copies.

Hale's "Ethnography and Philology", Vol. VI

Horatio Emmons Hale's volume entitled *Ethnography and Philology* [41] was the first of the special reports to appear after

[41] In this and all references to the official edition which follow the citation of the half-title is omitted, and may be understood to be the same as in the *Narrative, i.e.*, to have the words "By Authority of Congress". Likewise all of the title page preceding the volume number is omitted, since it is the same in all volumes. The atlases have no half-titles. The title-page of the Atlas to Vol. VII is cited in full, and those of subsequent atlases are shorted by the omission of that portion which is identical in all.

Vol. VI / Ethnography and Philology. / By / Horatio Hale, / Philologist of the Expedition. / Philadelphia: / Printed by C. Sherman. / 1846. [xii + 666 pp.]

Chart / of / Oceanic Migration / by H. Hale. / U. S. Ex. Ex. / 1838, 39, 40, 41, & 42. / Eng. by Sherman & Smith.

A chart / representing the Isles of the South Sea / according to / the Notions of the Inhabitants of / O-taheitee / and the Neighboring Isles, chiefly / collected from the accounts of / Tupaya. / E. Yeager Sc.

Ethnographical Map / of / Oregon / showing the Limits of the Tribes / and their / Affinities by Language. / By E. Hale / U. S. Ex. Ex. / 1844.

Unofficial, or "author's" edition:

the five volumes of *Narative*. It was not the first work of its author, a youth who graduated from Harvard College in 1837 after a college career of unusual distinction. As a boy of seventeen at about the time of his entrance to Harvard College,. Hale published his first scientific paper entitled "Remarks on the Language of the St. John's or Wlastukweek Indians, with a Penobscot Vocabulary". Asa Gray tells us that "he exhibited uncommon powers in the acquisition both of literature and science, and his industry was remarkable. His aptitude for learning languages made him known, even at that early period of his life, to the most distinguished philologists of our country. The late learned president of the American Academy was among his warmest friends. When the Exploring Expedition was fitting out, Mr. Hale, though still an undergraduate, was selected for the place of philologist; and the result shows that probably a better selection could not have been made. He engaged in the duties to which he was thus honorably appointed, with a zeal and ability which have produced the most valuable results. . . . The journals of voyagers, the writings of the missionaries, the researches of philologists into the nature and character of the languages spoken throughout the extensive groups of the Oceanic islands, manuscript vocabularies and grammars, have all been examined, sifted, and combined with the results of personal study and observation. Mr. Hale has thus succeeded in giving a certain classical completeness to his work, which makes it a model for future laborers in the same or in similar fields of research. The style of this volume is marked by rare excellences, and those of the highest order. It is elegant, terse, compact, and business-like, to a remarkable degree. . . . As a contribution to general philology it will stand in the foremost rank, unless the foolish economy of the government in limiting the number of copies published should unfortunately operate to exclude its valuable contents from the general fund of philological knowledge, and to defraud Mr. Hale of the reputation which is justly his due".

Hale made use of the phonetic modifications of the fundamental Polynesian vocabulary to trace the migrations of the

United States / Exploring Expedition. [Half-title.]

United States / Exploring Expedition / during the years / 1838, 1839, 1840, 1841, 1842. / Under the command of / Charles Wilkes, U.S.N. / Ethnography and Philology. / By / Horatio Hale, / Philologist of the Expedition. / Philadelphia: / Lea and Blanchard. / 1846.

Pagination and charts exactly as in the official edition.

Polynesian race, and arrived at conclusions which have not since been improved upon, although they have been only too often neglected, as, for instance, by Kern in his classical studies of Fijian. Among the remarkable features of his volume will be found vocabularies for islands which, after nearly a century, have still not been superseded, and for which new material will never be forthcoming, a grammar of Fijian which is remarkably in advance of its time, in that it presents an analysis of the language unmarred by any preconceptions, based upon European philology, of what such a language ought to be like, but isn't, and the first extensive comparative dictionary of the Polynesian dialects, which led him to a clear conception of the phonetic changes that had come about during the dispersal of the Polynesian peoples. In his striking argument regarding migrations, he used the name Hawaii as the type word, after presenting evidence that all its modifications were in accordance with the rules governing the phonetic differences between the dialects.

Contemporary opinion of Hale's *Ethnography and Philology,* as voiced by a younger contemporary, Brinton, was that it was "indispensable to one who would acquaint himself with Polynesian and American ethnography, the two fields in which it is strongest".

In 1856 Hale entered the practice of law at Clinton, Canada. He published on American Indian linguistics and ethnography until nearly the close of his life and was honored by the learned societies of both the United States and Canada.

Dana's "Zoophytes", Vol. VII

A hundred years ago the Cœlenterata were called Zoophyta, a name which was soon to go out of style and be superseded by Radiata. The zoologists have in turn frowned upon the latter name, for science is fickle in matters of nomenclature, and has to adopt new, even if no better, styles. James Dwight Dana's volume on the Zoophytes [42] marked remarkable progress in its

[42] Vol. VII. /Zoophytes. / By James D. Dana, A.M., / Geologist of the Expedition, / Member of the American Academy of Arts and Sciences of Boston, / Academy of Natural Sciences of Philadelphia, etc. / With a Folio Atlas of Sixty-one Plates. / Philadelphia: Printed by C. Sherman. / 1846. [pp. 740.]

Atlas. / Zoophytes. / By James D. Dana, A.M., / Geologist of the Expedition, / Member of the American Academy of Arts and Sciences of Boston, / Academy of Natural Sciences of Philadelphia, etc. / By Authority of Congress. / Philadelphia: C. Sherman, Printer. / 1849. [pp. 12, 61 plates (mostly colored).]

field. Asa Gray's contemporary review commended Dana most heartily for making his volume a complete monograph of all the species known. Dana, by doing so, became the exponent of all that was known not only about coral animalcules but also about the Actiniæ, those organisms, such as sea-anemones, which, although related to the coral zoophytes, make no coral. Of four hundred and eighty-three species of corals collected on the Expedition, only two hundred and fifty-four, or a little more than half, had been previously known. Moreover, of those previously known very few had been examined in a living state, for which reason Dana found it necessary to amplify the descriptions extensively. He had undertaken work, it turned out, on a group that had been so superficially known that as a result of the new information he "detected numerous errors in the received systems or suggested changes of fundamental importance. In making out the report, it was found impossible, in many genera, to describe the newly discovered species without giving new and more definite characters to the old, and the genera themselves sometimes required a modification of their limits, and changes in their associations".

Gray pronounced Dana's work one that would long remain the standard authority "upon one of the most curious and attractive, though formerly the most obscure and difficult departments of the animal kingdom".

Gray continued: "Nor should it be forgotten, in our estimate of Mr. Dana's labors, that his scientific reputation hitherto has principally rested on his mineralogical writings, that the special field assigned to him was the geology of the Expedition, upon which his reports are still to be made, and that it was only in the course of the voyage, owing to the withdrawal of a zealous member of the scientific corps to whom this department was originally consigned, that the subject of zoophytes fell into his able hands".

One of the notable facts about Dana's book was that he made the hundreds of drawings for the beautiful Atlas himself, with the exception of a few that were contributed by Mr. J. Drayton, one of the Artists of the Expedition. It is one of the great merits of the reports, not only of Dana's volumes but of all the rest, that the individual plates are all marked to indicate both artist (or draughtsman) and engraver. By the middle of the century

it was beginning to be common practice for engravers to work for companies, and for the plates to be signed by the firm rather than the individual. At the time the illustrations for the Wilkes reports were being made, American engravers had attained a high degree of technical excellence. This had largely come about through certain large publishing enterprises which employed and gave training to many engravers at once. Thus, we know from the signatures on the plates that at the very beginning of the century, when mechanical methods were still relatively unused, and most of the work on the plates was done by hand, the execution of the plates for any large work taxed the resources of the nation.

With reference to the American edition of Rees' *Encyclopedia,* published by Thomas Dobson, of Philadelphia, 1794–1803, Stauffer [43] says: "This publication appeared in so many volumes, and the amount of illustrative work was so great that the names signed to the plates included about all the engravers then actively employed in the United States."

Among the many engravers who worked on the Atlas for Dana's *Zoophytes* were Prud'homme [44] and J. N. Gimbrede.[45]

Dana's career was founded upon the United States Exploring Expedition just as surely as Darwin's was upon the Voyage of the "Beagle". He was only twenty years old when he left Yale College, before graduation, because of the opportunity for appointment to an instructorship in the Navy, which enabled him to visit the Mediterranean on the ship "Delaware". After his return in 1836 he was assistant to Professor Benjamin Silliman at Yale, but soon received appointment as geologist and mineralogist for the Expedition. He was attached to the ill-fated "Peacock" which was wrecked at the mouth of the Columbia River. However, he and others travelled overland to San Francisco,

[43] Stauffer, D. McN. *American Engravers upon Copper and Steel.* The Grolier Club, New York, 1907. [2 volumes.]

[44] John Francis Eugene Prud'homme came to the United States in 1807 with his parents from the Island of St. Thomas, West Indies, where he was born in 1800. He was apprenticed about 1814 to his brother-in-law Thomas Gimbrede to learn engraving, and was signing his own work by 1821. He did portraits in stipple. After 1852 he was a bank-note engraver, from 1869 to 1885 with the U. S. Bureau of Engraving and Printing. (See Stauffer, *op.c.*, Vol. I, p. 215.)

[45] Joseph Napoleon Gimbrede learned to engrave with his uncle J. F. E. Prud'homme, and was in business as an engraver at New York from 1841 to 1845. He was the son of Thomas Gimbrede, best known as an engraver of portraits in stipple, who was drawing-master at West Point. (See Stauffer, *op.c.*, p. 100.)

where they rejoined the remainder of the Expedition. He returned by way of the Cape of Good Hope on Wilkes's own ship, the "Vincennes", and still a member of the Expedition Staff after the return, spent thirteen laborious years in Washington writing up his reports. The first was the *Zoophytes,* the second the *Geology,* the third, in two volumes of text with atlas, the *Crustacea.*

One can hardly help being struck by the parallelism in the early lives of Darwin and Dana. Each found his great opportunity in a naval exploring voyage, each specialized and published voluminously on the taxonomy of the Crustacea, each found his major geological interest in volcanic and coral islands, each was an invalid much of his life, and each was a gigantic figure in science. Each had the capacity to be absorbed in innumerable details without ever losing sight of their broad bearings. The opportunity it gave Dana to become one of the greattest American leaders in science was undoubtedly one of the Exploring Expedition's greatest benefits to the nation.

Peale's Mammalogy and Birds, Vol. VIII

Titian Ramsay Peale lacked the advantages of formal higher education, but came of a family of amazing versatility and ability. His artistic and scientific background was excellent indeed. Two of the ladies of the family were artists of distinction in their day. His father, Charles Willson Peale, according to Stauffer, was executing respectable mezzotint engravings as early as 1787. It was the accident of obtaining a mastodon skeleton that led the elder Peale, burning with a new enthusiasm for science, to begin his private museum, which was built about his first acquisition. It became bigger and better for years, was conducted in a highly educational way, and had the approbation and patronage of the American Philosophical Society and of the Government itself, for it became the depository for many specimens collected on early government explorations in the West. Sharing in the family project was naturally more attractive to a boy of 17 than entering the cotton-spinning business, as he had a chance to do, and so he joined his brother Rubens Peale in taking care of the museum. It was more than they could manage, however, fell into financial difficulties, and descended steadily in status until it became necessary to append it to a musical show and dance hall

in order to pay expenses. Its final end as a Philadelphia enterprise came in 1849 when P. T. Barnum bought it, presumably for a circus sideshow, and it is supposed to have been destroyed in a conflagration.

Titian Peale's appointment to the staff of the Exploring Expedition saved him from being dragged down in the ruins of the Museum. He had long been improving himself in both art and science, and association with some of America's greatest scientific men had given him excellent training to justify his appointment to the Exploring Expedition. In 1818, he had been with Thomas Say, the father of American conchology, William Maclure, the first prominent American geologist, and George Ord, an eminent zoologist, in an expedition to the coast of Georgia and eastern Florida. In 1819 he went as assistant naturalist and painter on the United States Expedition under Maj. Stephen H. Long to the Upper Missouri, and made many sketches that were used in the report of that expedition. On this expedition he was Thomas Say's assistant. He exhibited four water-color paintings of animals in the exhibition of the Pennsylvania Academy of Fine Arts in 1822. In 1824 he was sent to Florida by Charles Lucien Bonaparte, nephew of the Emperor Napoleon I, to collect specimens and make drawings for Bonaparte's *American Ornithology* (4 vols., 1825–33). The colored plates of Vols. I and IV of this work were done by Peale. He continued to exhibit his natural history paintings, and between 1824 and 1828 prepared part of the plates for Thomas Say's *American Entomology.* In 1832 he visited the interior of Colombia for natural history collecting and the following year published his *Lepidoptera Americana.* He was therefore not ill-prepared for his work as naturalist of the Expedition and his performance as an author would have held up well had he not had the competition of at least three men of extraordinary genius, namely, Dana, Hale, and Pickering. His talents were more in accord with those of the other gentlemen of the Expedition. After returning Peale was prompt in publishing his report,[46] but unfortunately it met with disapproval by Wilkes, and after partial distribution (in

[46] Vol. VIII. / Mammalia and Ornithology. / by / Titian R. Peale, / one of the Naturalists of the Expedition. / Member of the American Philosophical Society, of the Academy of / Natural Sciences of Philadelphia, etc., etc. / Philadelphia: Printed by C. Sherman. / 1848. pp. i–xxv, [17]–338.

accordance with the law) the remaining copies were destroyed, to be replaced by Cassin's substitute volume.

It is immediately apparent if one examines Peale's volume carefully that Peale was careless in such matters as agreement in gender between generic names and specific adjectives. Other little inconsistencies are to be found. He failed to utilize voluminous records of mammals and birds that had been made by Charles Pickering, the insertion of which made the chief difference between Peale's volume and Cassin's.

Only a professional ornithologist would be competent to say if Cassin's descriptions were really much superior to Peale's. Peale followed the good old tradition of a Latin diagnosis, in which he did not skimp. In fact, if latinity be any test of scholarliness, Peale's volume is the superior. The defects are obviously those of haste rather than of knowledge, of inexperience in editing and proofreading. After the lapse of a century it is easy to be more indulgent about Peale's faults than his contemporaries were. His book has blemishes to be sure, but not such serious ones that Wilkes or the country had any reason to be ashamed of it. However, it was a time when Americans were very sensitive about any scientific or literary shortcomings that they might have. Anything European was *a priori* good: anything American probably bad. In a different connection G. Brown Goode referred to the national humility of Americans at that time as follows:

> It is almost ludicrous at this day to observe the grateful sentiments with which our men of science welcomed the adoption of this American method [the application of the telegraph to longitude determination] in the observatory at Greenwich. American were still writhing under the sting of Sydney Smith's demand "who reads an American book?" [47] and the narrations of those critical observers of National customs, Dickens, Basil Hall, and Mrs. Trollope. The continental approval of American science was like balsam to the sensitive spirits of our countrymen.

Although the numerous species it contains are to be considered validly published, the Peale volume is so excessively rare that for the convenience of naturalists I am presenting as an Ap-

[47] In 1937 a reader's letter of inquiry to the editor of the *Colophon* asked where the famous sneering remark about "an American book" originated. The editor answered: "An insular and provincial fellow named Sydney Smith is reputed to have made this enquiry about a century ago . . . in a review of Adam Seybert's 'Statistical Annals of the United States,' published in the *Edinburgh Review*." (*Colophon*, II, part 2, pp. 292, 303.)

pendix a list of the new names proposed by Peale, indicating how they were disposed of in Cassin's replacement volume. In these days of photostats and bibliofilm service it will be easy enough, for anyone who wishes, to have reference to this rarest volume of the Expedition reports, and by the indexing of Peale's book I hope that zoologists may be led to give it the attention that so great and serious an effort deserved.

Peale's opinion of the objects, conduct and results of the Expedition is recorded in a long manuscript in the Library of Congress, which, I am informed by the librarian of the National Museum, was published in a periodical now very rare and not very accessible in printed form.

It appears that Peale wrote an introduction for Cassin's replacement of his volume which was suppressed by the committee in charge of publication. This manuscript introduction is in the possession of the American Museum of Natural History and is to be published in connection with Mr. Daniel C. Haskell's forthcoming bibliography of the Wilkes Expedition. This publication may be expected to clear up the reasons why Wilkes considered Peale's volume so unsatisfactory.

That personal relations between Cassin and Wilkes were not always too cordial we know from a letter from Cassin to S. F. Baird, dated March 12, 1851, that is to say, three years after the publication of Peale's volume and seven years before his own. It presumably refers to the breaking off of an agreement with Wilkes about completing the illustrations or possibly rewriting Peale's volume, and reads in part: ". . . now that the Exploring Expedition has fallen through we must turn our attention to something else—

"I am unconditionally discharged from that business, and the only civil letter Wilkes ever wrote to me conveyed that information,—. . . I shall publish nothing relative to Peale's book for at least a year—. . . ." The omissions from this letter, which may have been of great bibliographic interest, were made by Baird's biographer Dall, from whom the quotation is taken. Copies of Peale's manuscript, however, have apparently had some distribution, and one of them which was made available to the bibliographer Max Meisel [48] is referred to by him.

It was through Peale's influence that the Academy of Natural

[48] See footnote 24.

Sciences of Philadelphia came into the possession of much of the anthropological collection of the Wilkes Expedition, although there is some in other museums.

Peale was employed from 1849 to 1872 as examiner in the United States Patent Office at Washington, and spent his declining years from retirement to his death in 1885 at the age of eighty-five in working upon his collections at the Academy of Natural Sciences in Philadelphia.

The best account of his life is that in the *Dictionary of American Biography*.[49]

Cassin's Mammalogy and Ornithology, Vol. VIII bis

Cassin, younger by 14 years than Peale, was chosen by Wilkes to prepare a revision of Peale's report.[50] Like Peale, he was a Philadelphian. He divided his time between business and ornithology, and since the former demanded so much of his time, he was distinctly one of those who came to be known as the "closet naturalists" as opposed to the workers in the field. His great opportunity in science came through his friendship with Dr. Thomas B. Wilson, patron and later president of the Philadelphia Academy, who procured for the Academy not only the largest collection of birds in the world, but likewise every piece of ornithological literature, no matter how scarce, that could be obtained. Thus Cassin was enabled to acquire a knowledge of world bird literature unexampled in America.

According to Witmer Stone, it was in 1846, when Peale was well along with his report on the mammals and birds of the Expedition, that Dr. Wilson, a devoted member of the Philadelphia Academy of Natural Science, purchased for the Academy at a cost of 50,000 francs the great ornithological collection of Victor Mossena, Duc di Rivoli and Prince d'Esling. This collection contained some 12,500 specimens, and its acquisition, as Thoreau said in his Journal (November 21, 1854), "was greatly to the

[49] S[ellers], H[orace] W[ells]. "Titian Ramsay Peale." *Dict. Amer. Biog.*, 14: 351–352, 1934.

[50] Mammalogy / and / Ornithology. / by / John Cassin, / Member of the Academy of Natural Sciences of Philadelphia; of the American Philosophical Society; / of the National Institute; of the Natural History Society of Charleston; of the Lyceum / of Natural History of New York; of the Natural History Society of Montreal; / Corresponding Member of the Zoological Society of London; Honorary / Member of the United Society of German Ornithologists, etc. / With a Folio Atlas. / Philadelphia: J. B. Lippincott & Co. /1858. pp. viii + 466.

credit of American energy and enterprise''. Dr. Wilson later added to his gift the Gould collection of Australian birds as well as notable smaller collections, so that when the whole assemblage was formally presented to the Academy it contained some 600 type specimens of Gould, Cassin, Bonaparte, Temminck, La Fresnay, Vieillot, Lesson, and other ornithologists whose work provided a basis for the ornithology of the regions visited by the Exploring Expedition. Ideally, much of this vast collection should have had attention in connection with the preparation of the report on the birds of the Wilkes Expedition.

P. L. Sclater, the eminent ornithologist of the British Museum, said in 1850 that the collection of birds at the Philadelphia Academy was at that time the best in the world, superior to any in Europe.

Stone remarks that ''such a collection as this, brought together at such an early date, was indeed wonderful, and though John Cassin worked at it until the time of his death, and described some 200 new species, there remained probably several hundred doubtfully named birds which subsequent and more extended collections have shown to be distinct species''.

Elliott Coues termed Cassin ''the only ornithologist this country has ever produced who was as familiar with the birds of the Old World as with those of America'', yet, good as his work undoubtedly was, as judged by the standards of the careful workers of his time, it is perhaps open to question whether, in the light of the ideas of species that prevail today, Peale's less discriminating procedure might not have led him as near to the truth as Cassin's more laborious investigations.

Cassin's business was the management of one of the principal engraving and lithographing establishments of the day. His first connection with the Exploring Expedition may have been somewhat in the way of business, for he was entrusted at first only with the task of straightening up the illustrations for the Atlas to accompany the Peale report, but was soon afterward entrusted with the rewriting of the text of the report itself.

The table of contents of the Peale volume called for a larger atlas than finally appeared with the Cassin report. It may be that Peale, on account of his father's experience as a professional engraver and his own artistic inclinations, was too critical to make satisfactorily rapid progress in finishing the Atlas. At

any rate, as brought together under Cassin's supervision, the engravings show great diversity in style and perfection. Some hand-engraved plates of merit seem to have been hastily completed by mechanical methods, then coming into vogue and not always skillfully handled. Careful inspection reveals a good many plates of mixed handiwork. Since the plates of this Atlas are among the more ambitious, and represent the work of a number of artists and engravers, it would be interesting to give them more than this casual mention.

Charles Pickering's "The Races of Man", Vol. IX

In the scientific staff of the Wilkes Expedition the field of physical anthropology was represented by Charles Pickering, whose volume,[51] in marked contrast to the others by specialists, presents in a brief four pages his chief conclusions, namely that there were eleven races of man. He then falls into a most slovenly, discursive and unsystematic account of these races, adopting an arrangement of chapters partly geographical, and including under the geographically appropriate chapters a medley of narrative and miscellaneous observations upon all sorts of subjects. The final chapters, several of which have no bearing whatever upon the Expedition, although they bear upon his theme, are more systematically organized, and deal mostly with introduced animals and plants of countries not even visited by the expedition, such as Equatorial Africa, Southern Arabia, India, and Egypt. His information on these countries was derived from travels which he made privately after his return from the Exploring Expedition, and were clearly, from their style and subject matter, an addition to the original manuscript, showing clearly the trend of his interests in the plants and animals that accompany man in his migration, whether in domestication or as hangers-on. His style in these final chapters begins to show the bookish abstruseness that characterized his later writings, and their content indicates the beginning of his preoccupation with the history of cultivated plants which was eventually his chief

[51] Vol. IX. / The Races of Man: / and their / Geographical Distribution. / By / Charles Pickering, M.D. / Member of the Scientific Corps attached to the Expedition. / Philadelphia: / Printed by C. Sherman. / 1848. [pp. i–viii, 9–447.]

[Colored Map] Geographical Distribution / of the / Races of Men. / By / C. Pickering, / U. S. Ex. Ex. / 1845. /

12 colored plates, representing human types.

interest. His important place among the students of human taxonomy was, however, established by his work on the Wilkes Expedition. He was the first to break away boldly from the idea that all men were comprehended under three, or at most four, races. It is now almost forgotten that in Pickering's time most scientists as well as educated men in general accepted, more or less without question, an idea based upon the Bible that the races of man were three, the white, the yellow, and the black, these being respectively, the descendants of Noah's sons Cham, Sem and Japheth. Linnaeus, to be sure, had recognized four, by distinguishing the American Indians from the yellow Asiatics, but his classification was brought into accord with common belief by Cuvier, who considered the Americans as Mongolians.

During Pickering's own lifetime the approved classification of man came to be that of Blumenbach, who recognized five races, the Caucasian, Mongolian, Æthiopian, American, and Malayan.

In the literature available to Pickering there was some movement toward classifying the Australian race as distinct and coordinate with the three (or four) others that were currently recognized. Pickering tells us very honestly in his account of the visit of the Expedition to New South Wales how few Australians he was able to see, but he didn't really need to see many (or more than one, in close view!) to be convinced how very different they were from the rest of mankind. So he set up the Australian as one of the prime divisions. Although Pickering relied to a shocking extent upon the memory of general impressions, not at all upon measurements nor even in many instances, as he confesses himself, upon notes, he said: "In the end all difficulties vanished, and I was enabled to arrive at satisfactory conclusions. . . . I have seen in all eleven races of men, and though I am hardly prepared to fix a positive limit to their number, I confess, after having visited so many different parts of the globe, that I am at a loss where to look for others. They may be enumerated conveniently enough in the order of complexion. . . .

a. White

1. Arabian . . .
2. Abyssinian . . .

b. Brown
3. Mongolian . . .
4. Hottentot . . .
5. Malay . . .
c. Blackish-brown
6. Papuan . . .
7. Negrillo . . .
8. Indian or Telingan . . .
9. Ethiopian . . .
d. Black
10. Australian . . .
11. Negro. . . .''

There are very close fore-shadowings, in certain parts of this classification, of views held by much later anthropologists, some of whom arrived at their conclusions quite independently, since they give no credit to Pickering, although others cited his work and used some of his nomenclature. Pickering accepted the idea that most of the American aborigines were a branch of the Mongolian race. He had, however, had a very broad concept indeed of the Malayan race, and included under it many of the American Indians of Western America from California southward to Guatemala, those of the Isthmus of Panama, of an area in northern Colombia, and all those of the West Indies. This idea is distinctly in the discard, although some late anthropologists, Dixon for instance, have thrown doubt upon the idea that all the Amerinds are to be uncritically combined into one racial group. Pickering's eventually accepted conclusions were very important. He clearly distinguished the Papuans, in an inclusive sense, covering not only the coastal people of New Guinea, but the Melanesians, which is quite in accord with modern opinion. He set the Asiatic and Philippine Negritos aside from other races, as the Negrillos, quite in accordance with most modern views. (His term, Negrillo, is by Deniker and others transferred to the African pygmies, who were of course unknown to Pickering, but the pygmies are made by Deniker one branch of a Negrito race, of which the other branch comprehends the Philippine Negritoes, the Andamanese, the Semang of the Malay Peninsula, etc., that is to say, Pickering's original Negrillos.)

Pickering separated the Dravidians (under the name Te-

lingans) from the Indo-Europeans(whom he called the Arabian race) and this conclusion was perhaps the most original and indicative of his power as an independent thinker of all, for anthropologists were very slow to shake off the shackles imposed by general acceptance of the absurd claim of some Hindus that members of the Brahmin caste, whether brunette whites in the Northwest of India or blacks in the Dravidian provinces, are all of one race.

Pickering's separation of a race including Bushmen and Hottentots from the black races has had support from Huxley, Deniker, and practically all later anthropologists. Pickering made a distinction between Nubians or Ethiopians (he used both names) and the remaining African Negroes. Modern classification confirms the validity of this distinction. Schurtz, for instance, recognizes the "Aethiopische Rasse" of North Africa as one of three Old Races ("Alte Rassen") as opposed to Chief Races ("Hauptrassen") and Blend Races ("Mischrassen") and considers the Nubians as the best type of the Ethiopian. Pickering's "Negro Race" includes such peoples as the Bantu, and his Abyssinian Race is the Berber-Hamitic (Berberische Mischrasse of Schurtz).

Charles Pickering (one of the distinguished descendants of Timothy Pickering of Revolutionary fame), an M.D. of Harvard in the class of 1826, settled in Philadelphia in 1827. Here he engaged actively in the work of the Philadelphia Academy of Natural Sciences, of which he had already been elected a corresponding member. He served on the botanical and zoological committees and held the offices of librarian (1828–33) and curator (1833–37). The first scientific paper [52] that he presented to the American Philosophical Society indicated that in 1827, within a year of leaving college, he already had the deep interest in the geographic distribution of plants which dominated his later career.

Pickering was appointed naturalist to the Exploring Expedition and served throughout the cruise. His *Geographical Distribution of Animals and Plants* (Vol. XIX of the Expeditionary Reports—the unofficial issue incorrectly called Vol. XV) was followed by *Plants in their Wild State,* not published officially

[52] Pickering, Charles. "On the geographical distribution of plants." *Trans. Amer. Phil. Soc.*, n.s., 3: 274–84, 1827.

but by the Naturalist's Agency of Salem. These works led to the final monumental literary enterprise *The Chronological History of Plants: Man's Record of his Own Existence Illustrated through their Names, Uses, and Companionship,* upon which he labored unceasingly during the sixteen years before his death. It then remained still unpublished, but was edited and seen through the press by his widow. Pickering's work became increasingly meticulous and critical as he became older. In his last great volume he achieved a complete synthesis of the interests which chiefly engaged him during the Expedition—namely, the ancient history and migration of human races as demonstrated by the uses and names of plants. He was one of the first great ethnobotanists.

It must be admitted that Pickering was a keen and discriminating observer, not so systematic in thought and precise in method as his colleague Hale, but nevertheless a scientist many of whose conclusions, even if somewhat "intuitional", have stood the test of time. He would undoubtedly have received more credit for his original views if the editions of his works had been large enough to provide for more adequate distribution.

Gould's "Mollusca and Shells", Vol. XII

The circumstances which led to Augustus A. Gould becoming the author of the report [53] on Mollusca instead of Joseph Pitty Couthouy have been told by W. H. Dall.[54] Couthouy was born in Boston in 1808 and entered the Boston Latin School in 1820. He first went to sea in his father's vessel and seems to have led a seafaring life, for in 1836, the year when he was elected to membership in the Boston Society of Natural History, and presented his first paper on conchology for publication, he was called "Captain". Two years later he presented "a large and splendid collection of foreign shells" to the Harvard Natural

[53] Vol. XII. Mollusca and Shells. / By / Augustus A. Gould, M.D., / Fellow of the American Academy of Arts and Sciences; / of the American Philosophical Society; / Member of the Boston Society of Natural History, etc. / With an Atlas of Plates. / Philadelphia: / Printed by C. Sherman. / 1852. [xv + 510 pp.]

Atlas. / Mollusca and Shells. / By / Augustus A. Gould, M.D. / Fellow of the American Academy of Arts and Science; and of the American Philosophical Society; / Member of the Academy of Natural Sciences at Philadelphia; of the / Boston Society of Natural History, / etc. / By authority of Congress. / Philadelphia: / C. Sherman & Son, Printers. / 1856. [16 pp. + 52 plates.]

[54] Dall, William H. "Some American Conchologists." *Proc. Biol. Soc., Wash.,* 4: 95–134, 1888.

History Society, founded in 1837. In 1838 when the United States Exploring Expedition was in the final stages of organization, Couthouy went to Washington and applied in person to President Andrew Jackson for a position on the scientific corps. When told that the appointments had all been made "the irrepressible young sailor replied: 'Well, General, I'll be hanged if I don't go, if I have to go before the mast'. This pleased 'Old Hickory', who told him, 'Go back to Boston, and I will see if anything can be done for you'. There, a few days after his return, his commission as conchologist of the scientific corps was received. . . . After leaving Samoa his health suffered. Wilkes, who was preparing a narrative of the expedition, demanded that Couthouy should turn all his notes and drawings over to the commander. Couthouy refused, as he considered that his subsequent work would be crippled by the absence of notes and drawings already made, and that as a member of the scientific corps he was entitled to retain his papers until the end of the voyage. He was thereupon suspended by Wilkes and ordered home from Honolulu in 1840 'for disobedience of orders' ". This information about Couthouy is from Dall, who continues:

He had made many valuable drawings and notes, many of which are preserved in the report on the Mollusca and Shells of the expedition. He had numbered his notes with a serial number, and a tin tag, similarly numbered, was attached to the specimen, which was preserved in spirits for future anatomical study and identification. The authorities in Washington had appointed a reverend gentleman who knew nothing of science, with a fat salary, to unpack and take care of the specimens sent home by the expedition. This gentleman, finding that the presence of some lead in the tinfoil tags was whitening the alcohol, carefully removed all the tags and put them in a bottle by themselves without replacing them by any other means of identification. Twenty years ago [Dall wrote in 1888] I saw this bottle of tags on a shelf at the Smithsonian and heard its mournful history. Prominent conchologists, resident in the United States, were favored, for a consideration, with many rare specimens before any of the expedition naturalists had returned. Some of those contemporary with the events have told me of the prizes secured in this immoral manner, unworthy of a true naturalist, though doubtless the temptation was great.

. . . Couthouy found that the shells to which many of his notes related could not be identified, and others had disappeared altogether. He worked over the mass that remained until the return of the expedition, when, to crown all his misfortunes, the pay of the naturalists was reduced forty-four per cent, though low enough previously. For

Couthouy, who had a wife and two children to support, it was the last straw. He declined to attempt the report, and his papers and collections, after sundry vicissitudes, were put into the hands of Dr. A. A. Gould, who bears willing testimony to the value of Couthouy's work.

Turning now to what Gould said in his introduction to the published report, we find:

Mr. Joseph P. Couthouy, the able naturalist of the Expedition in this department, made careful and suggestive notes of all interesting species, and especially, of the new or doubtful species, with the intention of amplifying them after his return. . . . Up to the time of leaving the Paumotu, or Low Coral Islands, these notes were pretty fully written out in form. On arriving at the Samoa Islands, his health obliged him to separate from the squadron; and the numerous notes he had subsequently made were left in an imperfect state. Still these would have been extremely valuable, especially those relating to the land-shells of the Society, Samoa, and Sandwich Islands. But, unfortunately, repeated searches have failed to discover them among the masses of documents pertaining to the Expedition.

Mr. Couthouy was also careful to attach marks, or numbers, to all specimens described or figured, or to which special interest attached; and they were so disposed of as to be readily accessible and readily recognized. The drawings relating to them had been so far finished as to secure the forms and attitudes, more particularly the colours of the animals, all of which greatly change after death, leaving the more permanent features to be copied at leisure. But it had been thought necessary, by the Navy Department, that the boxes sent home in advance of the expedition should be opened, lest, by long packing, the specimens might be injured. Those who preformed this service were not fully aware of the importance of replacing the specimens as originally arranged, and hence much labour and difficulty in identifying them. In many instances, the search was quite fruitless, and, consequently, many drawings and descriptions were altogether rejected, and so far lost.

In spite of Gould's troubles, which were probably as nought compared with poor Couthouy's, Gould was able to report extensively upon several regions where the Mollusca or some groups of them had been previously almost unknown, for example, Tierra del Fuego, the land shells of many Pacific islands, especially the Society and Samoa Islands, and the marine forms of Puget Sound and Oregon. Over 28 members of the Expedition collected Mollusca, and many fine specimens were given by local residents of the places they visited.

An important feature of Couthouy's work was the attention given to naked or unshelled Mollusca, and to the living animals

in all groups. The collections and drawings enabled many new forms to be described. In many genera, Couthouy's and Gould's work added a remarkable number to the species then known; in one genus, *Succinea,* as then delimited, as many new species were described as the sum of all previously known.

Since the Mollusca of the French Expedition of the Astrolabe and the Zelée were in progress of being studied at the same time Gould was at work, he took the precaution to issue preliminary descriptions in the *Proceedings of the Boston Society of Natural History,* and thus secured priority for his new names. To Couthouy he gave credit for the species if names had been proposed and descriptions written by that naturalist. In the field of Malacology the results of the Expedition proved to be important despite the unfortunate loss of Couthouy from the staff and the disagreeable task that Gould had to undertake. In the Atlas the drawings of the Plates are mostly ascribed to J. Drayton and J. P. Couthouy, or to the former alone, or to Drayton and Dougal; the engraving to J. H. Arnold or Dougal, or W. G. Newton.

Because of the precautions that Gould took to secure priority by preliminary publication for the Expedition Mollusca, there is a certain little volume, made up of assembled separata from journals, which forms an indispensible companion to Gould's great official Report, with its magnificent Atlas. This repaged compilation was completed by Gould [55] in 1862 and issued under the title "Otia Conchologica".

Instead of waiting he knew not how many years for the publication of the official report of the Expedition, Gould published currently upon the new mollusks as he described them, in a continued article published on 18 dates running through volumes II and III of the *Proceedings of the Boston Society of Natural*

[55] Gould, Augustus A., *Otia Conchologica: Descriptions of Shells and Mollusks, from 1839 to 1862.* Boston: Gould and Lincoln, 1862. [t.p. + 2 pp. (preface recto and subtitles verso) + 256 pp.]

[Subtitles.]

Expedition Shells; described for the work of the United States Exploring Expedition, Commanded by Charles Wilkes, U.S.N., during the Years 1838–1842 [pp. 1–100].

Shells of the North Pacific Exploring Expedition under Commanders Ringgold and Rogers [pp. 101–178].

Collectanea: Descriptions of Shells and Mollusks reprinted from various Scientific Periodicals, with Revisions. 1839–1862.

Rectifications [pp. 242–246].

History, from July 1846 to December 1850. As the original sheets were printed the pages were consecutively renumbered.

There would seem to be evidence that a brochure of more than 96 but not over 100 pages (presumably 98 pages) was issued with a title-page set up in 1846 when the series was begun, for all of the signatures making up the first 96 pages have similar paper. A signature which begins at page 97 is evenly divided between a part of the Wilkes Expedition article published in 1847 (ending on p. 100 with a table giving actual month and year of publication for each installment) and a similar continued article on the "Shells of the North Pacific Exploring Expedition, Commanders Ringgold and Rodgers", which did not begin until 1859. It would seem that the original brochure of 98 pages on the Mollusca of the Wilkes Expedition had accidentally omitted six species. In order to correct the omission, the signature beginning at p. 97 was reset with the descriptions of the omitted species added at the end, bringing the pagination up to 100. At this time Gould must have determined to continue the volume by adding all of his new species from whatever source, and to provide a new title-page for the entire volume. Thus it became his "Otia Conchologica", completed in 1862. Signatures 14–22 (pp. 105–176) are on heavy paper and seem to have been repaged sheets as set up for the Boston Society of Natural History. This part covers all but a couple of pages of the Ringgold and Rogers article, which is concluded on pp. 177–178, the first two pages of the 23d signature. From page 179 to the end of the Index (p. 256) consists of "Collectanea: Descriptions of Shells and Mollusks by A. A. Gould, reprinted from various scientific periodicals, with revisions". Signatures 23 to 32, inclusive, have numerous revisions of the original texts and constitute not a reprint but a new edition of various descriptions. These pages are especially important in connection with the Mollusca of the Wilkes Expedition, for two reasons: (1) pp. 223 to 236 gave diagnoses of those of his own species which were originally published in his official volume XII of the *Reports,* and had not been previously described in the *Proceedings of the Boston Society of Natural History;* (2) pp. 242 to 246 change the names of many of the species as given in volume XII. He says (p. 242): "Since the time when the printing of the *Expedition Shells* was begun, in 1846, great changes have been made in nomenclature.

Very many new genera and subgenera have been instituted, some of which have been fully established by the structure of the mollusks producing the shells. Meantime, also, the same species has, in many instances, been described by different authors under different names. Some of the more important rectifications, thus becoming necessary, are given below. . . ."

The final pages of *Collectanea* appear to date from 1862, since this is the year given on the title page, and since the last signature printed from the type set for the Boston Society of Natural History was printed in April, 1861. The preface of "Otia" is dated April, 1862.

In discussing the history of American conchology, Dall divided it into three periods. The first, dominated by Thomas Say, extended from 1817 to 1841. The second, of which one of the chief achievements was the elaboration of the collections of the Exploring Expedition, was dominated by Gould himself. It was his especial merit that he advanced the study of the naked Mollusca, and gave the attention that a true biologist should to the whole of the animal, not merely to the shell. It is quite clear that Couthouy's notes and drawings, if satisfactory to Gould, indicate that the collector himself had an advanced biological attitude in his work, and was scientifically worthy of his post.

Augustus Addison Gould, educated as a physician, received his M.D. degree in 1830, but achieved his fame as a naturalist. His first scientific article was published in 1833. His first original taxonomic studies in the Mollusca dated from about 1839, and they were the works for which he was chiefly known. The circumstances under which he took over the work on the Mollusca collected by the Exploring Expedition have been explained. The vast material made available to him led to pioneer studies of geographical distribution, which led to speculations on paleogeography which were distinctly novel. He was joint author with Louis Agassiz of the *Principles of Zoology,* published in 1848 and long one of the most useful works in its field.

Dana's Report on Crustacea, Vol. XIII

Dana, like Gould, had his troubles in writing his reports, especially the one on *Crustacea,*[56] because of the maltreatment to

[56] Vol. XIII. / Crustacea. / By / James D. Dana, A.M., / Member of the Soc. Caes. Nat. Cur. of Moscow; the Soc. Philomathique of Paris; the / Geological Society

which part of his collections were subjected in Washington before the return of the Expedition. Many of his specimens were "prepared for exhibition" by being removed from the alcohol and dried. Many very soft minute forms were impaled on insect pins for drying, and completely ruined. Others that retained their outer characteristics were spoiled for dissection, and many labels were lost. Dana was very disconsolate over these uncalled-for and unecessary mishaps. In addition he lost the results of many months work in the shipwreck of the "Peacock" at the mouth of the Columbia River. Nevertheless his report on Crustacea was one of the best zoological monographs of its period, and remarkable, like Dana's *Zoophytes,* for the beautiful illustrations, drawn or painted for the engraver and colorist by himself. In his prefatory remarks to the Atlas he said: "After the engraving of the Plates of this Atlas was completed, a large part of the original drawings were destroyed by fire in Philadelphia. In consequence of this catastrophe, many of the following Plates, which were to have been coloured from the missing drawings, are issued uncoloured. Some of the figures here engraved represent known species, the colours of which had either not been given by other authors, or only from dead and faded specimens: as the coloured originals are gone, these figures have lost the principal part of their interest. . . . The drawings for this Atlas, issued at this late date, were to a large extent made during the years 1838 to 1842, in the course of the cruise of the Expedition, and in the history of the Science, they would properly have their place in that period.

"In closing my labours in connection with the Exploring Expedition, I take this opportunity to observe that, in the preparation of my several Reports and the Atlases with which they are illustrated, I have had, half a dozen plates excepted, neither the assistance of an amanuensis nor a draftsman. The plates, however, owe much to the artistic skill and taste of Mr. Joseph Drayton, Artist of the Expedition, who has superintended the engraving and printing and contributed in many ways to the beauty of the work." (New Haven, January 1, 1855.)

of London; the American Academy of Arts and Sciences / at Boston; the Academy of Natural Sciences of Philadelphia, etc. / With a folio Atlas of Ninety-six Plates. / Part I. / Philadelphia: / Printed by C. Sherman. / 1852. [viii + 685 pp.]

Part II. 1853. [h.t. + t.p. + pp. 687–1618.]

Atlas. / Crustacea. / By / James D. Dana, LL.D., / . . . / . . . / . . . / By Authority of Congress. / Philadelphia: / C. Sherman, Printer. / 1855. [pp. 27 + 96 pl.]

In a review of Dana's gigantic *Report on the Crustacea* it is again inevitable that the parallelism between the scientific experiences and interests of Darwin and Dana should be pointed out. Darwin got his systematic training in zoology and his ideas of what constituted a species from years of what he felt to be drudgery over barnacles—a group of degenerate Crustacea. Nothing occupied his attention in field work more than corals. Dana, primarily mineralogist and geologist, became zoologist also by force of circumstances. It is quite natural that corals, because of their geological importance, should have occupied his attention, but it seems a curious coincidence that his other group of specialization should have been the Crustacea. When he heard that Darwin was at work on the barnacles, he did little more with this particular subdivision of the Crustacea than to turn over the adult shelled forms to his conchological colleague Gould, and to publish himself certain observations that he had made during the cruise which convinced him that the Cirripedia were not Mollusca.

As late as 1852, when Vol. I of the *Report on Crustacea* appeared, there still remained some difference of opinion among zoologists as to where the barnacles belonged. Some systematists still placed them in the Mollusca. Dana, not knowing J. V. Thompson's paper of 1830, in which the developmental stages were described to prove that the barnacles were "Articulata", arrived at the same conclusions that Thompson had, as a result of studies in the equatorial Atlantic and later off Tierra del Fuego. He found young and adult forms together, and was able to trace the transitions to adult forms. Moreover, he collected exuviae of Cirripedia in the Harbor of Rio de Janeiro, which proved that they were not Mollusca.

Another conviction of Dana's has not stood the test of time, namely his insistence that the Rotifera were lowly Crustacea. Dana was not alone in this belief, in support of which he said that they had "the mandibles and other mouth organs of Crustacea, and some of them resemble certain Entomostraca in general form and in the jointed structure of the caudal extremity. In these species we have, therefore, the lowest Crustacean form under a Radiate type,—the type of the inferior branch of the animal kingdom" (p. 4). I quote this passage because to a modern reader it might seem that Dana was seeking in the "Radi-

ata" a point of departure for the evolution of the Crustacea. If he was groping along the lines of evolutionary thought, however, he was not willing to admit it.

Yet we cannot help being startled by such sentences as these from the pen of a "special creationist". "In the study of these species [of Crustacea] there are actual difficulties in the way of arriving at natural subdivisions with conveniently circumscribed limits. The difficulties arise mostly from the fact that no such limits exist as the systematist often looks for. Nature has made her fields without fences, and although there are some mountain ranges, in general, the blendings among the lower subdivisions in the kingdoms of life are by gentle gradations. The true object of classification consists in tracing out gradations and inter-reticulations among groups. Keeping this in view, we shall not be dissatisfied if the groups laid down are found to shade into one another, instead of standing apart in bold relief" (I, p. 143).

Dana had much in common with Darwin, and since he proved to be the intellectual giant of the Expedition, it is hard for his reader not to read between the lines ideas that Dana for many years was to disavow, although he became, later in life, a confessed evolutionist.

Dana's subsequent history showed that he was a man comparable in intellectual power with any of his great English contemporaries in natural science. He had much the same scientific background as Darwin, had much the same opportunities for observation, interested himself in the same groups, and, one cannot help thinking, might well have come to conclusions with regard to the origin of species similar to those of Darwin and Wallace. His great work on the Crustacea appeared while Darwin was elaborating the theory of evolution, and only seven years before the publication of the *Origin of Species.*

As one turns Dana's pages looking for comments on the broader aspects of species relationship and distribution, he finds a logical development of evidence which requires an evolutionary explanation; then a labored effort to justify more orthodox preconceived beliefs. One may easily ascertain from a glance at some of Dana's earlier studies that he had a strong theological bias and at least up to the time of the publication of *Crustacea* was unable to throw off adherence to the conventional belief in the independent origin of species, much as it was held by Agassiz.

One constantly finds himself expecting Dana to make some downright, unguarded statement of belief in the community of descent at least of closely allied types, but he never actually does so. He catches himself in time and throws in some conventional pietistic allusion that just saves him from being classifiable with Darwin and Wallace as one of the pioneer evolutionists. So far as accuracy of observation is concerned, there is no reason to rate Dana as inferior to Darwin. Neither was he inferior in his power of systematization. However, he could not bring himself to go the whole way in breaking with a conventional pattern of thought.

It is amazing to find a nonevolutionist (as Dana insisted upon being at the time) writing such passages as those that follow, for which I give the page references to the continuously paged Parts I and II of *Crustacea:* (p. 4) "Crustacea also pass, by almost imperceptible shades, into Vermes, through the Caligus and Lernæa tribes; the most degraded Lernæan forms having the sluggishness and almost memberless character of the lowest worms. Their resemblance to the typical Crustacea is so slight, that, without a knowledge of the gradations through the well-modelled Caligi to the higher forms, their relations to the class would hardly be suspected."

(p. 7) There are species (of the stalk-eyed Crustacea) which are removed from the rest by characters of high importance; yet such species are only examples of inferior development;—that is, they are analogous in general character to the condition which the typical species present before arriving at complete maturity. Their degradation is seen in their having the thoracic branchiæ exposed, instead of covered by the carapax; and, in a lower stage, in having no thoracic branchiæ, but only similar appendages to the abdomen; and, in a stage still lower, in the branchiæ being wanting altogether, and even the abdominal appendages rudimentary. . . .

(p. 8) In the Anomobranchiates the feet are in part two-branched or bifid, and this is an additional mark of their relation to immature forms.

(p. 43) The excessive number of joints in the Phyllopoda finds an analogy in the Vermes and in the larvæ of insects, and it is perfectly in harmony with the law laid down by Prof. Agassiz, who observes that the same peculiarity characterizes the Crustacea of the earliest geological epochs. This peculiarity is evidence of inferiority of grade, such as marks animal life of other kinds in the Palæozoic period.

(p. 45) . . . we naturally look upon the Brachyura [crab-like Crustacea] as having the higher position—as higher in space, if we con-

ceive of the Brachyura as having a position; and the Macroura [lobster-like types] in a similar manner as having a lower position; while between the two, partaking neither of the typical characters of the former or latter, a number of forms are arranged that are as stepping stones from one to the other.

(p. 46) It is of great interest to trace out these relations; and in order to appreciate their true value, we must first comprehend in what way, and by what characteristics, superiority of grade is attained. (Here the reader holds his breath, thinking that an avowal of evolutionary doctrine is coming—but only to be let down by the following sentence.) Concentration in the nervous system has been well shown to be the basis of it, and simplicity (under certain limitations) in number of external members or parts, its exhibition.

(p. 53) I have attempted to represent this relative grade by the relative level or height in the following table; and, although a rude representation of nature, it gives some idea of the gradual relations of the groups. I give only the prominent and obvious lines of relation and not the many interlinkings of affinities or convergences between the several lines.

(p. 54) There are neither straight lines nor circles in nature, but main branching lines, with subordinate branches, and almost endless reticulations or anastomoses, by curves of all kinds, and of all grades of divergence and convergence.

(p. 55) Reaching the Macroural level, we find no longer a few species only to a type, as in the Anomoura; there is a vast development of forms with even a smaller variety of types. A distinct system of structure is arrived at, which is not of the nature of a transition or mixed style of insect-architecture, but a perfect and simple style of itself, and upon this system as a basis, the number of modifications is exceedingly large. There is a surprising fertility in the expressions of the one idea exhibited in the Macroural structure and we cannot fail to admire the infinity of resource displayed—which is the more wonderful as it is not developed where the diversity of types and grades favored diversity of forms, as in the Anomoura, but in a single grade, and as the development of a single defined thought.

Finally Dana accepts Agassiz's doctrine of cephalization to explain his "lines of degeneration".

(p. 1396) The distinctions [among Crustacea] are obviously distinctions of rank. There is no ambiguity as to which is the higher or superior group, as among Insecta. The variations are manifestly variations in grade, and we may readily trace out the several steps of gradation, as we descend from the highest Brachyura to the lowest Lernaea. And while we so readily distinguish these gradations, we as plainly see that they are not steps of progress followed by nature in the production of species; but, simply successive levels (grades of types), upon which species have been multiplied.

We, therefore, may consider the class Crustacea as especially well adapted for instruction in some of the higher principles of classification in Zoology; and, if we mistake not, laws may be educed which have not hitherto taken form in science.

The fundamental idea, which we shall find at the basis of the various distinctions of structure among the species is, the *higher centralization of the superior grades and the less concentrated central forces of the inferior*. . . .

Dana goes on to identify "centralization" with "cephalization", the concurrent development of the function and structure of the head. Constantly emphasizing descending series rather than ascending, he says: "As we descend, the head loses one part after another, and with every loss of this kind, there is a step down in rank."

(p. 1401) . . . the process of degradation . . . is not actually passed through in the system of creation; for by its progress we should never reach the Macroural structure; nor, in the reverse order, should we from the Macroural reach the Brachyural structure.

(p.. 1402) The Macroura and Brachyura belong to subordinate, yet correlated types of structure, each perfect in itself, and admitting of wide modifications, and having its own system of degradations.

Yet, by way of picking an inconsistency between the last two quotations and a preceding discussion of the affinity of the Anomoura (a group of anomalous types, neither crab-like nor yet lobster-like), I must point out that he had already said:

(p. 398) As the Anomoura constitute properly a transition group between the Brachyura on the one side and Macroura on the other, there may be much doubt as to the proper limit in each direction.

(p. 49) This review of the relative rank of the different grand divisions of the Brachyura, prepared us to trace farther the gradations through the Anomoura to the Macroura.

Finally (p. 52) "Through Galathea, we believe we may point out a passage into the Macroural dominion by Æglea to Astacus. . . . We should thus connect the Macroura with the Brachyura through two lines, one by Callianassa and Pagurus, and the other by Astacacus and Galathaea".

A reader constantly feels that Dana was really thinking along evolutionary lines, but that when he came to drawing the obvious conclusion, he would not permit himself to speak.

At the very end, when he deals with the geographical distribution of the marine Crustacea, he comes to most interesting con-

clusions regarding faunal regions, but does not allow himself to indulge in speculation of unorthodox type. He weighs the problems of disjunct and local distribution, stating that the alternate explanations are *original local creations* and *migration.* In cases of doubt he argues well for migration but declares for original local creation. He says (p. 1581): "These characteristics are of no climatal origin. They are the impress of the Creator's hand. . . ." As an example of the same or very closely similar Temperate Zone species occurring in distant regions, he says: "An example of this, beyond all dispute, is that of the Mediterranean Sea and Japan. . . . The two regions have their peculiarities and their striking resemblances, and we are forced to attribute them to original creation and not intercommunication."

We cannot feel otherwise than that Dana was just on the edge of great conclusions. He was too inexperienced a geologist at the time to admit the possibility even that the occurrence of the same crustacean on both sides of the Isthmus of Panama could be explained by the former continuity of the ocean across that narrow ribbon of land. He forced himself to say that special creation of one and the same species had taken place twice on opposite sides, after telling what simple alternative belief would explain the phenomenon satisfactorily.

It was not until long after the publication of Darwin's *Origin of Species* that Dana became an avowed evolutionist. We can see, however, as one of his biographers [57] has said, how "Dana had moved toward evolution without himself being aware of it. The principle of evolution is so universally accepted today and forms so fundamentally a part of our thinking that it is difficult for us to realize a time when it formed no part of the mental outfit of science; we should do this, however, to appreciate how in some ways of thinking Dana was ahead of his time and how inevitable was his final acceptance of it".

Dana suffered a breakdown of health in 1859, the very year of the publication of the *Origin of Species,* and never fully regained his full vigor. He was obliged to give himself an almost complete rest for a year. For long periods he could work at

[57] Pirsson, Louis V., "Biographical Memoir of James Dwight Dana, 1813–1895." *Nat. Acad. Sci., Biog. Mem.,* 9: 41–92, Washington, 1919.

most an hour or two a day. As a result, he did not read Darwin's classic for several years. He ultimately accepted its conclusions, and in the last edition of his *Manual of Geology* he said: ". . . the law of nature, as regards kingdoms of life, is not permanence, but change, evolution."

Dana obtained eminence in mineralogy before his appointment to the Exploring Expedition. During the cruise he laid the foundations for fame in zoology and geology. In his later years he maintained an interest in both mineralogy and zoology because of their bearing upon geology, which gradually became his chief field of activity. Of his fame in geology I need say nothing. Another participant in this symposium has dealt with the influence of the Expedition in shaping Dana's later geological work. "Of his contributions to science, on which in the future his fame will be found chiefly to rest, this [his geological work], we believe, will have most distinction and importance" (Pirsson, *l.c.*, p. 77).

I may, therefore, close my account of Dana as a zoologist by quoting what Darwin wrote to him regarding the report on Crustacea, in a letter quoted by Pirsson from Gilman:[58]

> I have experienced such great interest in many parts and have found it so suggestive towards my Cirripedia work that I cannot resist expressing my thanks and admiration. The geographical discussion struck me as eminently good. The size of the work and the necessary labor bestowed upon it are really surprising. Why, if you had done nothing else whatever, it would have been a *magnum opus* for life! Forgive my presuming to estimate your labors, but when I think that this work has followed your *Corals* and your *Geology* [of the *Expedition*—Vol. X, 1849] I am really lost in astonishment at what you have done in mental labor. And then, beside the labor, so much originality in all your works.

There could have been no more satisfying commendation than this from Darwin. Because of the opportunity it gave Dana and his companions of the Expedition to attain world-wide distinction in science and exploration, the Expedition justified itself. America had set out to take her place in world science, and if the Expedition had done nothing more, the development of a Dana would have been enough.

[58] Gilman, Daniel C., *Life of James Dwight Dana.* New York and London, Harper and Brothers, 1899.

Gray's Report on the Flowering Plants, Vol. XV

It was natural that when Rich's report on the flowering plants of the Expedition was found to be unsatisfactory for publication, Gray should have been asked, eventually, to write the replacement, since he had become far and away the leading American botanist. Part of his own work, one large text volume with Atlas, was published.[59] Another volume remains in manuscript at the Gray Herbarium,[60] but most of the new species which it would have contained undoubtedly found publication elsewhere. The history of Gray's connection with the Expedition may be found in his Letters.[61]

In 1836 Gray was one of the original appointees to the civilian scientific staff of the Expedition, which had not yet sailed when he received the offer of an appointment at the University of Michigan. The long delay in sailing and constant change of plans had led Gray to lose confidence somewhat in the enterprise. In 1838 there was an opportunity for him to drop out. He tells us (p. 21) that the assistant botanist, William Rich, an appointment of the Secretary of the Navy, was to be left out on account of last minute changes in the squadron. Gray was able to resign in Rich's favor, a matter which might possibly have hurt his conscience a little bit, for he had written to W. J. Hooker in 1836 regarding his appointment: "I am anxious to engage in this

59 [Unofficial Edition.]

Botany. / Phanerogamia. / By / Asa Gray, M.D. / with a folio Atlas of One Hundred Plates. / Vol. I. / New York: / George P. Putnam & Co. / 1854. [pp. 777, dated at end of index "May 20, 1854."]

[Official edition.]

Atlas. / Botany. / Phanerogamia. / Vol. I. / By / Asa Gray, M.D. / By Authority of Congress. / Philadelphia: / C. Sherman & Son, Printers. / 1856. [4 pp. + 100 pl.]

60 With regard to the second of Gray's volumes, which still remains in manuscript, Mrs. Gray wrote that in 1872 "Dr. Gray had the manuscript prepared some years before and notified the Library Committee that he was ready for publishing. Meantime came the war, and there was no money or thought for such things. When the country was again quiet and prosperous, the library committee who had formerly known and been interested in the work and its printing had passed away; there was no one to care for it, and the manuscript was never called for" (*Gray's Letters*, II, p. 623). Prior to the issue of vol. 15, Gray had published only three preliminary papers (in *Proc. Amer. Acad. Arts and Sci.*, vols. 2 and 3) bearing upon the plants of the Wilkes Expedition, which vitiated only to a slight degree the importance of the volume. Between 1855 and 1862, however, when it was clear that further official publication might never be provided for, or at best would be long delayed, Gray carved six more papers out of his uncalled-for manuscript, which were published in *Proc. Amer. Acad. Arts and Sci.*, vols. 5 and 6.

61 Gray, Jane Loring, ed., *Letters of Asa Gray.* 2 vols. London, Macmillan and Co., 1893.

work, and I suppose may do so if I choose, but I fear that the expedition, which, if well appointed and conducted, may do much for the advancement of the good cause of science, may be so marred by improper appointments as to render it unadvisable for me to be connected with it.''

In a letter to his father in the same year he had said (p. 61): ''The scientific corps will consist of several persons, in different departments of science, and the persons who will probably be selected are mostly my personal friends: two of them at least having been recommended at my suggestion.''

One of these two friends whom he had personally recommended was Dana, who was in part, at least, persuaded to go by the argument that he would have Gray's companionship. Gray had taught natural science at ''the flourishing school of Mr. Bartlett at Utica'' from 1829 to 1834 and Dana had been a student there until 1830. He was not Gray's student, but their friendship began at Utica in 1829 or 1830. Gray's other especial friend on the staff of naturalists of the Expedition seems to have been Pickering. It was in July 1838 that Gray resigned his place to Rich, and this was after Poinsett had taken over the planning of the Expedition, and was really shaping up the personnel in a manner that would have been very satisfactory to Gray. Poinsett wanted Gray to go to Washington ''to have a talk with him, to learn more definitely about what their plans, etc., are, and thinks he will be able to remove my present dissatisfaction, and if not says I may have leave to withdraw. . . . The only difficulty is that I am afraid they will ply me with such strong reasons as to prevail on me to hold my situation, particularly as their new plan has the advantage of leaving home all the blockheads and taking the best fellows''.

Gray certainly did not help the Expedition by resigning his place to Rich, as he well knew, for in later years he did not hesitate to name Rich as one of the blockheads.[62] One of Gray's

[62] Gray's favorable contemporary opinion of Poinsett's plans is important, for even years after Wilkes's squadron returned there were still grostesquely unfair and absurd attacks being made against Poinsett for placing primary emphasis on the scientific objectives of the Expedition. Since one that appeared in *Harper's Magazine* especially reveals the atmosphere of hostility that the badly informed populace in general had to the Poinsett plans, I shall quote part of it: ''Although President Jackson favored the Expedition, his Secretary of the Navy was privately opposed to it, and threw every possible obstacle in the way of its success. . . .

''. . . Mr. Reynolds was careful so to project it as to make its national and

letters to J. D. Hooker, under date of Dec. 31, 1845, says: "I suppose you are now going on with the 'Flora Antarctica'. I need not say that I should be very glad to see the Antarctic plants of the Wilkes Expedition in your hands. The botanist who accompaned the expedition is no doubt perfectly incompetent to the task, so greatly so that probably he has but a remote idea how incompetent he is. I have not seen him nor the plants. Certainly I would not touch them (any but the Oregon and Californian) if they were offered to me, which they are not likely to be. I consider myself totally incompetent to do such a work without making it a special study for some years, and going abroad to study the collections accumulated in Europe. Of course if they are worked up at all in this country, they will be done disgracefully. I publicly expressed my opinion on the subject in *Silliman's Journal*. But I have long been convinced that nothing can be done. The whole business has been in the hands till now of Senator . . . , the most obstinate, wrong-headed,

commercial ends of primary importance. Dickerson did his utmost to defeat the law of Congress. . . . The Navy Department clique held up the enterprise as an encroachment on the rights of navy officers, to have a corps of scientific citizens accompanying the expedition, and called this class mere clam-catchers.

". . . This being *the* maritime enterprise in which we as a nation were to make our *début*, the projector Reynolds had planned it to a high degree of perfection and was the only person capable of pointing out the places for general rendezvous; but the Department clique had resolved that he should not accompany the expedition. . . . The misapplication of funds, the change in the vessels, the efforts to create discord, the delay of reports, the withholding of information from Congress, and the indecision and inconsistency shown in shelving the friends of the enterprise and rewarding its enemies, compelled the Executive to transfer the control from the Secretary of the Navy to the Secretary of War, Joel R. Poinsett, who proved to be as hostile to the movement as ever Dickerson had been.

". . . A general order was issued on the 22d of June, 1838, declaring the expedition purely *scientific*, and leaving the President to select a commander without regard to previous rank. Congress was deceived by Poinsett and Company [when the expedition was] . . . placed under the command of Lieutenant Charles Wilkes.

"The comparison between the *good* and the *bad* plan completely disheartened the friends of the enterprise, particularly the treatment of the eminent scientific corps, upon whom Wilkes laid his hostile hand after they had made, *under the plighted faith of the government*, every preparation to sail with the expedition. In the judgment of this commander, who acted under the direction of Poinsett and Company, several of these gentlemen were declared useless incumbrances, and were not permitted to engage in it. Among these, to the astonishment and regret of every friend of the enterprise, and in defiance of the known wishes and requests of members of Congress, and the ablest scientific men in the land, Commander Wilkes declared that, for the sake of harmony, the *author* [*i.e.*, Reynolds!] should not be allowed to accompany his expedition in any capacity, public or private; and to prevent the possibility of his doing so, the flotilla hurriedly sailed. . . ." [Carrell, Miss A. E.] "The first American Exploring Expedition." *Harper's New Monthly Magazine*, **44**: 60–64, Dec. 1871.

narrow-minded, impracticable ignoramus that could well be found. . . . If to this you add an utter ignorance of those principles of comity and the spirit of interchange that prevail among naturalists, and a total want of comprehension of what is to be done in the scientific works in question, you will see that nothing is to be expected from such sources. They have thrown every obstacle they could in the way of their naturalists, . . . Dana and Pickering, for instance, . . . so much so that Pickering, though a patient man, once threw up his position in disgust, I have heard, but by some concessions made to him, was finally persuaded to retain it.''

Within three years after this letter was written, Wilkes had asked Torrey to undertake the entire Botany, Torrey had refused, and Gray had agreed to do exactly what he had expressed himself as most disinclined to do, namely to work up all the collections except those from the Pacific Coast. To Torrey had been assigned this part, which was all that Gray had been willing to do. Haskell (*l.c.*, p. 101) has just published an interesting letter that Torrey wrote Brackenridge in June, 1848. ''You have doubtless, by this time, been informed of Capt. Wilkes' negotiations with Dr. Gray as to the Expl. Expn. plants. . . . I knew nothing about his movements, however, until Dr. Gray told me of them. From what the Dr. had told me a year or more ago, I concluded he would have nothing to do with the job, and began to fear that the whole matter would fall through. At last, however, Capt. W has partially (and must wholly) agreed to adopt the only plan which is practicable. What he declined doing at my office he has acknowledged must be done . . . viz. to put out the plants to monographers. Dr. G. will do this work well (better than I could do it) if his terms are agreed to—and let me tell you he will drive a harder bargain than I could have accomplished. He will not only be well paid for the job, . . . but will require much more. The committee must be at the expense of his voyage to England and France. As the phrase is 'it takes Dr. G. to make a bargain'.''

It worked out that Gray had his way in part, but not altogether. He was not required to live in Washington to write the report, but took the plants abroad for comparison as he had insisted he would do. Some of the other authors suffered under such onerous restrictions that Gray's success in dealing with the

Library Committee seems phenomenal. Dana, for instance, had been informed by Senator Tappan in 1846 that for the rest of the time till the works were out he would have to live in Washington. Dana flatly refused, a propos of which he wrote Gray: "It is perfectly absurd that I should be able to prepare my reports in a city where there are no books."

In June 1848 Dr. and Mrs. Gray ("J") visited Washington to make all the arrangements, and Gray, immediately upon his return to Cambridge, wrote Torrey about what happened: ". . . Now as to the Exploring Expedition. We will talk it over in full when you come on here toward the end of this month. Suffice it to say . . . that I had made up my mind what I would do it for before I left home; that on looking over the collection, as to various parts of it, as far time allowed, I found it less ample than I supposed, but with many difficulties owing to specimens in fruit only, or flower only. I think it no very awful job, if done in the way I propose, which is, not by monographs by people abroad, which the committee will not agree to, but by working up a part abroad in Hooker's, or Bentham's, or Garden of Plants herbarium.

"The chairman of the committee and Wilkes behaved very well, and told me they were very desirous I should take it up.

"On Friday evening Wilkes came in, before we went to the President's; asked me to say just what I would do. I told him at once just what I would do (just what I had told J. before we left Cambridge) and Wilkes at once accepted my terms, as I supposed he would. My terms were based on the supposition that there is five years' work in preparing for the press the collections left on hand, and in superintending the printing. . . ."

By January 1850 Gray was making plans to take leave of absence for a year to go abroad with the plants. He wrote to Geo. Bentham: "As far as it has yet shaped itself my plan is . . . to sit down hard to work for the autumn and winter on the Exploring Expedition plants, to go to Paris in the spring and settle such questions as must be settled there after I come to know better than I now do (except in the Compositae) what they are. Excepting the Oregon and Californian plants, which are assigned to Torrey, and the Sandwich Islands Collection, a fine one, the collection is a poor one, often very meagre in specimens, too much of an alongshore and roadside collection to be of great

interest. I am not familiar with tropical forms and have no great love for them. I dislike to take the time to study out laboriously and guessingly, with incomplete specimens, and no great herbaria and libraries to refer to, these things which are mostly well known to botanists, though not to me, and I want to be taken off from North American botany for as short a time as possible. I must therefore come abroad with them, which the pay that is offered will enable me to do. I have found a good deal to interest me in the Compositae, especially those of Rio Negro, of north Patagonia and of the Andes of Peru. . . .

"Now, will you take it as a bore, an imposition on your kindness, if I frankly ask whether I can possibly offer you any sort of inducement to aid me, at least so far as to run over the collections with me, and name those that are familiar to you as we pass, and refer others, as nearly as one can without study, to their proper places? Your mere comments in running through would save half my time.

"It is most natural that you should not incline to any such trouble, and I know your hands are always full; so, if you say no, I shall feel it is quite right, and do the best I can. . . ."

The Grays sailed in June 1850 and carried through their program of travel and study. At Mr. Bentham's, in Herefordshire, Gray spent two months of very hard work. Bentham "most kindly went over with him the plants of the United States Exploring Expedition" after which he spent the winter in hard work in Sir William Hooker's Herbarium at Kew. At Paris in April 1851, he wrote, "I have not seen Gaudichaud yet; but he has offered to come and show me his Sandwich Island collections, etc., of which he has issued some plates in 'La Voyage de la Bonite', but no text has appeared and none seems likely to appear".

By the middle of 1853 manuscript of the completed part of Gray's work was piling up, and a third was ready for printing. Gray wrote A. De Candolle of Geneva: "As to the 'Botany of the South Sea Exploring Expedition', the manuscript and the drawings are ready up nearly to the Leguminosæ; and the printing, which is not under my control, is about to commence. The work will probably make three quarto volumes and 300 folio plates. I shall be sure to have a copy to send you. As to the specimens, there are few duplicates; and of these I am not my-

self allowed to retain any. Possibly, hereafter, some may be awarded to me. That expedition did not land on the high Antarctic coasts it saw, and therefore made no collections there. Its Antarctic collection is all from Orange Harbor, Tierra del Fuego, and has little that is new.

"The most interesting part of the collection was made at the Sandwich and Feejee islands."

On June 1, 1854, Gray was able to send De Candolle "a copy of the 1st volume of the *Botany of the United States Exploring Expedition in the South Seas,* which has been more than a year in printing. . . . The atlas, of 100 plates in folio, which should accompany this volume, is by no means ready, owing to the slowness as well as the feeble health of the artist, Mr. Sprague . . .".

Years were to pass before publication of the Botany was resumed. Then in 1872 Torrey's manuscript was called for, and he felt that such extensive revision would be called for that he was inclined to withhold it. Gray did not agree with him and wrote to him as follows: "I write a brief line, in response to yours of yesterday, mainly to say that I fear I disagree with you about the reply to be made to Wilkes's urgent request to print the manuscript of the Oregon collection of Wilkes' Expedition.

"It was prepared to print long ago; is not your fault that it has been delayed so long. The library committee have a right to print it, and might do so without your corrections if you decline to make any. We want the plates, which are now thrown away, and must be published. I would print in the form of a naked list, . . . except where remarks and descriptions are still wanted, . . . and to make all right and sure, and to relieve you, I, with Watson's kind help, will fix it all up for you and read the proofs once, and so save you the worry. And I urgently request you to send this line to Professor Henry, as embodying my opinion, and my offer of help.

"I am sure that if the rest of my manuscript is called for, I shall turn it over with satisfaction, though the same applies to it as to yours. And I should either alter accordingly or add notes.

"The rest of your letter I will respond to in due time.

"But I feel concerned to have those Oregon plates out.

"I think I have some right to, as I paid for one hundred of

them; but that is no matter. They are now neither published nor unpublished, which is a bad state of things.''

When the joint Committee of Congress on the Library was assigned the duty of publishing the Wilkes reports its members were as unprepared for such a task as can well be imagined. As the Committee gained in experience, what it had learned was passed on to Congress. The lessons were amazingly expensive, but there can be no doubt whatever that their educational value was likewise extremely great, and that a tradition was created which was of great value in future enterprises that involved scientific publication. So convinced did Congress become of the disservice to science through too restricted publication that the pendulum swung too far the other way, and editions of other expensive scientific reports were so enormous that for decades after they were out of print, some, like the reports of the Pacific Railroad Survey, clogged the second-hand book shops at 50c a volume. This criticism that was freely and abundantly showered upon the Library Committee led other divisions of the Government to try to emulate the good features of the Wilkes reports and to avoid the bad.

Certainly it was a good thing for the future of botanical science in America that our botanists were led to take a geographically unrestricted view of their field and that some of the great American herbaria have grown to international importance, rivaling those of Europe in this regard. The beginnings were made by the Wilkes Expedition, and regardless of how strongly we agree with Gray in his advocacy of scientific internationalism, we have to admit that much is to be said for the nationalism of Wilkes, in so far as it led to putting American science on a world basis. It was a good thing for the botanists who followed Gray in America that he devoted much of his time for several years to distant floras. In all branches of natural science one may do excellent work within geographic limitations, but there is a broader type of monographic work which is worldwide in scope and that American botanists for decades might not have participated in effectively if it had not been for the perhaps crude nationalism of Wilkes's period, which would not admit that there were any limitations to what we Americans might undertake quite as well as anyone else.

In Gray's volume and in subsidiary preliminary or subse-

quent publications genera are dedicated to several members of the Expedition—to most of those who participated actively in the botanical and zoological work, and, in addition, to Reynolds, projector of the enterprise. The interesting dedications follow:

Brackenridgea. "The name selected for this genus is intended to commemorate the important scientific services of Mr. William D. Brackenridge, the Assistant Botanist of the Expedition, through whose indefatigable zeal and industry this botanical collection was principally made, of which he has himself elaborated the Ferns and allied orders."[63]

Richella. "The name Richea being preoccupied, I trust I may be permitted somewhat to modify and prolong it, in order to dedicate this well-marked genus to William Rich, Esq., the Botanist of the Expedition in which these collections were made." (There were also eighteen species that received the name "Richii".[64])

Agatea. "As the ancients garlanded the graves of their deceased friends with violets, so I dedicate this new genus of Violaceæ to the memory of Alfred T. Agate, the Botanical Artist of the Expedition, who died at Washington shortly after its return. The engraving is copied from a drawing made by Mr. Agate, from the fresh plant, at the Feejee Islands."[65]

Draytonia. "With much satisfaction I dedicate this genus to Joseph Drayton, Esq., the principal of the scientific artists of the Expedition, of no small attainments in natural history, especially in Conchology, to whose pencil and superintendence the illustrations of the whole invertebrate zoology of the Expedition owe their high perfection."[66]

Couthovia. "[This genus] is dedicated to Joseph P. Couthouy, Esq., the zealous Conchologist of the Expedition, of which

63 Brackenridgea was republished in *Proc. Amer. Acad. Arts and Sci.*, 3: 51, 1857. There were also species named "Brackenridgei" in seven genera, viz., *Astragalus, Clidemia, Cupania, Draba, Eugenia, Hibiscus,* and *Pittosporum*, these in vol. XV; subsequently there were species in several other genera.

64 In vol. XV species of the genera *Acronychia, Astragalus, Capparis, Casearea, Celastrus, Cleome, Cupania, Diclidocarpus, Didymochiton, Eugenia, Eurya, Gouania, Haplopetalon, Malvastrum, Phaca, Pittosporum, Terminalia,* and *Weinmannia;* subsequently several other species. Richella had been prepublished in *Proc. Amer. Acad. Arts and Sci.*, 3: 128, 1857.

65 Agatea had been published in advance of the report in *Proc. Amer. Acad. Arts and Sci.*, 2: 323, 1852.

66 The genus *Draytonia* was redescribed in *Proc. Amer. Acad.*, 3: 128, 1857, probably because Gray felt that the distribution of the large volume was too limited to be quite effective.

the plant is one of the fruits." (*Proc. Amer. Acad. Arts and Sci.*, 4: 324, 1860.)

Gouldia. "I have much pleasure in dedicating this genus to Augustus A. Gould, M.D., a distinguished zoologist, author of the Natural History of the Invertebrata of Massachusetts, and of the Conchology of the South Sea Exploring Expedition under Captain Wilkes." (*Proc. Amer. Acad. Arts and Sci.*, 4: 310, 1860.)

Reynoldsia. "I dedicate the genus [67] to J. N. Reynolds, Esq., who merits this commemoration for the unflagging zeal with which he urged upon our government the project of the South Sea Exploring Expedition, and also for having made, under trying circumstances, an interesting collection of dried plants in southern Chili, many years ago."

Brackenridge's Report on the Ferns and Fern Allies, Vol. XVI

On the cruise William D. Brackenridge, Horticulturist and Assistant Botanist, devoted himself to the ferns, and the report [68] on this group was his only contribution to scientific botany. Unfortunately it is such an exceedingly rare volume that it has been inaccessible to many students of ferns in this country and abroad. Twenty copies of the small official edition were destroyed by fire in 1856, and although there was an unofficial edition, it is even more excessively rare than the official one, for it was destroyed by fire, Gray [69] tells us, after only ten of presumably a hundred or more copies had been sold.

[67] The genus Reynoldsia was republished in *Proc. Amer. Acad. Arts and Sci.*, 3: 128, 1857.

[68] Vol. XVI. / Botany. / Cryptogamia. / Filices, / including Lycopodiaceæ and Hydropterides. / By / William D. Brackenridge. / With a Folio Atlas of Forty-six Plates. / Philadelphia: / Printed by C. Sherman. / 1854. [pp. viii + 357.]

Atlas. / Botany. / Cryptogamia. / Filices, / including Lycopodiaceæ and Hydropterides. / By / William D. Brackenridge. / By Authority of Congress. / Philadelphia: / C. Sherman, Printer. / 1855. [pp. 7 + 46 plates.]

[69] In a letter of June 30, 1856, to W. J. Hooker, Gray wrote: "By the way, it was most lucky that I hurried up and had sent on to you the copy of Brackenridge's 'Filices,' for a fire in Philadelphia has consumed all of the poor fellow's edition of the volume except ten copies which had been sold, mostly in Europe. A sad and a heavy loss to B., who had no insurance, and something to me who had advanced to him the paper for printing it on, which now the poor fellow is in no position to pay for. I have not even a copy of the atlas myself, but I shall get one from the government plates, which are preserved. Brackenridge utterly despairs of reprinting it. But possibly the government will set up the type for him again, as they have also lost a part of their small impression. Otherwise the book will have the value of excessive rarity, if it has no other. . . ." (Asa Gray, *Letters*, I, p. 422.)

In the preface to his volume Brackenridge stated that he encountered many difficulties because of "the absence of a good botanical library in Washington" and also "the want of a collection of authenticated species of exotic ferns in this country". He acknowledged his indebtedness in the preparation of his work to Drs. John Torrey and Asa Gray, for the use of books, and for translating his English specific characters into Latin.

The drawings to illustrate his volume were made by Mr. William S. Lawrence, who also engraved the plates. "As it was his first attempt at this kind of drawing, there frequently occurred omissions, or but partial representations of the minor details. . . . This will account for the discrepancies in some few cases between the plates and the letterpress. . . ." Both descriptions and plates compare favorably with the work of reputable contemporaries of Brackenridge's in the same field, and a large proportion of the species which he proposed have stood the test of time.

Barnhart,[70] author of the best biographical sketch of Brackenridge, refers to the volume on ferns as a "masterpiece", and says that its author "was a good field botanist, with the advantage of four years of intimate association during the voyage with the scholarly Pickering; and he was by no means illiterate. That he could write English clearly and fluently is proven by his letters still in existence. But he was not well versed in the technical forms of descriptive plant taxonomy, and his knowledge of Latin was very limited.

"Brackenridge's modesty was so excessive that he could never be induced to furnish biographical information to those

[70] Barnhart, John Hendley, "Brackenridge and his book on ferns". *Jour. N. Y. Bot. Gard.*, 20: 117–124, 1919.

In the preparation of this article Barnhart had the advantage of referring to the letters written by Brackenridge to Torrey, and now preserved in the library of the New York Botanical Garden. He states that Brackenridge's indebtedness to Gray was not so great as might be assumed from a letter written by Gray to George Engelmann, December 7, 1853. ". . . one would suppose that Gray was responsible for all that there is of value in Brackenridge's masterpiece, but this is certainly far from true. Even the Latin translations were not all Gray's work; unless, indeed, Torrey's notes were not made available to him, and he was compelled to do all of his work without their assistance." Gray's letter had said: "Exploring Expedition Botany stopped printing for a long time, but it now renewed; three hundred or more pages are printed, and copy sent to printer up to Leguminosæ. Meanwhile, to look over Brackenridge's manuscript of the Filices, to turn a loose ungrammatical lingo into English, and his English characters into Latin, is a tedious job; then to read his proofs is another. But if I did not do all this, very bad work indeed would be made of it." (Asa Gray, *Letters*, II, p. 404.)

who sought to secure it directly from him as the most authoritative source, and the world is indebted to the late Thomas Meehan for the preservation of not a few of the facts of his interesting career. Meehan published an account of Brackenridge, with a portrait, in the number of his *Gardeners' Monthly* for December, 1884, and recorded his death February 3, 1893, in the number of *Meehan's Monthly* for March, 1893."

William D. Brackenridge, a Scotchman, had been a student of gardening at Berlin under Friedrich Otto before he came to America about 1837. He was in the employ of Robert Buist, Philadelphia nurseryman, but the following year seized the opportunity to become assistant botanist and horticulturist of the Exploring Expedition. He was responsible for bringing a not inconsiderable number of living plants home from the cruise, and many seeds. In order to take care of them a little greenhouse was erected as soon as possible on the lot behind the Patent Office, but at first, Goode tells us, Brackenridge seems to have had to take the personal responsibility for hiring greenhouse space for them in Washington, until the official house should be ready. The plants grew and multiplied so fast that enlargement of their quarters to at least three times the original size was required between 1843 and 1845. This we know from Charles Mason Hovey's account of a visit in the fall of 1843 to the "Experimental Garden of the National Institute", compared with the same writer's account of two years later, when he found that "a great accession has been made to the collection, through the untiring exertions of Mr. Brackenridge; and in order to do justice to the plants, an additional wing has been added to the house, so that it now extends upward of one hundred and fity feet, built in the most thorough and substantial manner".[71]

A movement for the foundation of a National Botanic Garden was sponsored by the National Institution in 1840, as one of its prime objectives. Congress gave six acres of land into the custody of the Institution in the Mall, near the Capitol, and there new conservatories were constructed in 1850. Brackenridge took care of his plants as long as Wilkes had supervision, that

[71] The Editor [Hovey, Charles Mason], "Experimental garden of the National Institute (containing the plants collected on the Wilkes Expedition)". *Mag. Hort. Bot.*, 10: 81–2, 1844. "Notes of a visit to several gardens in the vicinity of Washington, Baltimore, Philadelphia, and New York, in October 1845." *Mag. Hort. Bot.*, 12: 241–248, 1846.

is, until 1854. In 1855 he removed to a farm near Baltimore and devoted himself to landscape architecture, the operation of a nursery, and to editorial work as horticultural editor of the *American Farmer.*

We have in Brackenridge's own words his ideas of the significance of his work on the Expedition. It appears that his purely systematic interest in botany was confined to the ferns, but that his horticultural interests embraced all the plant groups. Goode has published a manuscript report which Brackenridge presented to the National Institute in November, 1842, on the botanical collections in his custody. I have rearranged short extracts from it to deal first with the living specimens, and secondly with the herbarium material: "The number of seeds brought and sent home by the expedition amounted to 684 species, the most of which have been sent all over the country. Several cases of live plants were also sent home [in advance of the return of the Expedition] of the existence of which there are no traces. The live plants brought home by the squadron amounted to 254 species, and these now form part of the greenhouse collection.

"For their preservation a greenhouse 50 feet long, and partitioned into two apartments, has been erected on the lot behind the Patent Office. The number of species in cultivation amounts to 500. [The collection of the Expedition has] received several additions in return for seeds distributed from the same source; also a few donations of other plants from various quarters. . . . With duplicates there are about 1,000 plants in pots, over and above those now coming up from seeds. As it is expected that donations will frequently be made, and as the plants we now have will be increasing in size, the present house by another year will hardly suffice to contain them. The propriety also of having a lot of ground fenced in where these plants could be set out during the summer months, and which could also be used for the raising of ornamental trees, shrubs, and other hardy plants, which may come into the possession of the Institute, is strongly urged. The meagerness of our parterres and shrubberies evidently shows that additions are wanting for ornamental gardening.

"It would also be a receptacle for proving all samples of fruits, flowers and esculents that may from time to time be pre-

sented to the Institute, there being, so far as I am aware, no public establishment of the kind in existence in the Union. Officers of our Navy and consuls residing in foreign countries might do a great deal in introducing fruits, vegetables and flowers, and whenever it is known that such an establishment exists, there is every reason to anticipate donations, where the country in general is to be benefited by such an enlightened and commendable scheme. A nucleus once formed, with a gradual accumulation of stock and a steady perseverance in its support and furtherance, we might, at some not very distant day, vie with the most celebrated establishments of the same kind in Europe. The progress of the benefit to be expected must be, like the undertaking, slow but sure, and the effects will soon become evident to every enlightened citizen."

We find in the passages just quoted the ideas which led to the development of two of our national establishments, the United States Department of Agriculture and the United States Botanic Garden. Both of these Cinderellas had their beginnings under the wing of a sometimes unwilling and resentful foster parent, the Patent Office. The Congressional Committee on the Library of Congress took the part of a fairy prince for the Garden. Under its custody the Garden has served as a non-scientific show place and conservatory near the Capitol down to the present time. It has certainly distributed enough plants (to say nothing of cut flowers) to have amazed Brackenridge, and has undoubtedly done some good with disproportionately great expenditure, since it has long been an instrumentality better adapted to Congressional petty graft than to the advancement of either Botany or Horticulture. May it signalize its approaching hundredth anniversary by turning over many new leaves! The present vast work of the Department of Agriculture's Plant Introduction Office is just as clearly to be interpreted as an outgrowth of what Brackenridge started at the little Patent Office greenhouse. Great oaks from little acorns grow!

It may be added that we find in Brackenridge's early and most meritorious efforts the germ of the later Congressional Free Seed Distribution, of malodorous memory. The horticulturist Hovey [72] visited the Experimental Garden of the National

[72] The Editor [Hovey, Charles Mason], "Notes on gardens and nurseries in the vicinity of New York, Philadelphia, Baltimore and Washington". *Mag. Hort. Bot.*, 14: 241–244, 1848.

Institute March 10, 1848, and wrote: "This garden, under the superintendence of our correspondent, Mr. Brackenridge, is well repaying the expense attending its management: through the exertions of Mr. Burke, the Commissioner of Patents, thousands of papers of seeds, raised in the garden, are distributed, through the representatives of the several states, annually assembled in Washington, and handsome, even rare and choice, kinds of seeds find their way where, through the ordinary course of trade, they would not be probably introduced for years. In a country extending over so vast a territory as ours, and deprived, as we are, of cheap postage, this medium [the franking privilege!] of sending seeds is of the greatest importance; for it brings to immediate notice many varieties which would otherwise remain but little known for years. . . ."

Now we may consider Mr. Brackenridge's words about what was to become the nucleus of the United States National Herbarium (administratively the Division of Plants of the U. S. National Museum):

The National Institute through the Exploring Expedition possesses one of the most extensive and varied botanical collections, from the numerous places which the Expedition touched at, that is yet known to have been accumulated during any voyage of similar character. This collection has not yet been arranged or set up according to any particular system. . . .

The following is a list of plants, or number of species in the herbarium, collected at the various places visited by the Expedition:

Madeira	300	Low Coral Islands (in all)	27
Cape de Verde Islands	60	Sandwich Islands	883
Brazil	989	Oregon country	1,218
Patagonia (Rio Negro)	150	California	519
Tierra de Fuego	220	Manila	381
Chile and Chilean Andes	442	Singapore	80
Peru and Peruvian Andes	820	Mindanao	102
Tahiti	288	Sulu Islands	58
Samoa, or Navigator Islands	459	Mangsi Islands	80
New Holland	789	Cape of Good Hope	330
New Zealand	398	St. Helena	20
Lord Auckland Island	50		
Tongatabu	236	Total number of species	9,674
Fiji Islands	786		

After the return of the Expedition, and before Brackenridge made his report, the famous botanist Thomas Nuttall had been engaged in ordering up the collection "according to orders and genera". He said that the number of specimens was about 50,-

000, which justified Brackenridge in saying that "wherever a set of specimens of the whole is classified and arranged systematically there will still remain a great number of duplicates to dispose of to institutions of a similar character, either in exchange or otherwise, as the Institute may think fit".

Lower Cryptogamia and the Phanerogamia of the Pacific Coast, Vol. XVII

A miscellaneous volume of botanical contributions by several authors was edited by Asa Gray.[73] The first part appeared years before the remainder and its unofficial edition is cited as not corresponding at all to the official. The copy of the official edition at the University of Michigan has been rebound and does not have the first part, and therefore it is not known whether it is as complete as when issued.[74] It lacks a half-title (which other text volumes have) and pp. 1–112, which according to a prefatory note by Gray, comprised the Musci, of which an edition in imperial folio form, with 26 plates, was separately issued by the author, W. S. Sullivant. This "prefatory edition" is cited by bibliographers (e.g., E. D. Merrill[75]) as consisting of 32 pages, 26 plates. A copy of it at the Gray Herbarium (referred to by Collins) conforms with this description. Meisel says definitely that the "text printed in folio corresponds to pp. 1–112 printed in 1874", but he cites a printed catalogue card of the Library of Congress according to which Vol. XVII "is an imperial

[73] United States / Exploring Expedition. / During the Years / 1838–1842, / under the command of / Charles Wilkes, U.S.N. / Vol. XVII. / Botany. / I. Lower Cryptogamia. / II. Phanerogamia of the Pacific Coast / of North America. / Philadelphia: / Printed by C. Sherman / 1862, 1874. [no half-title, 1 prelim. leaf, pp. 113–514, 2 + 9 + 1 + 17 folded plates.] pp. 113–152, 2 plates, Lichenes, by Edward Tuckerman; pp. 153–192, 9 plates, Algae, by Jacob Whitman Bailey and William Henry Harvey; pp. 193–203, 1 plate, Fungi, by Rev. M. A. Curtis, F.A.A.A.S., and Rev. M. J. Berkeley, F.L.S.; pp. 205–514, 17 plates, Phanerogamia of Pacific North America, by John Torrey (a second sub-title page reads: Phanerogamia of the United States Exploring Expedition).

[74] There is some mystery to be solved about Vol. XVII, for although apparently catalogued for the official cards of the Library of Congress from an incomplete copy (lacking pp. 1–112) I was shown in 1912 at the Library of Congress a complete copy in the original green morocco binding of the official set. Since it had been searched for in order to answer a specific inquiry from Mr. Collins, I cannot believe that I reported to him that there was a complete copy at the Library of Congress "with title-page dated 1874" if I had not seen it.

[75] Merrill, E. D., "Bibliography of Philippine Botany," in his *Enumeration of Philippine Plants*, Vol. IV. Manila, 1926.

volume of 420 pages (pp. 113–514) and 29 plates. (See *Amer. Jour. Sci.,* ser. 3, v. 8, pp. 321–22, 1874.)"

The lower cryptogams brought back by the Expedition were not especially numerous. The reports regarding them were so long delayed in publication that many of the new species were first published elsewhere. This circumstance, combined with the fact that, according to Haskell (*l.c.,* p. 107), the whole "official" volume was never "officially distributed", has resulted in their being seldom referred to in subsequent literature. The original plan for publication of the reports required that each volume should be by a single *American* author. No collaboration from abroad was to be allowed, for, as I have said, the all-American reports were going to signalize a sort of a scientific declaration of independence. Gray secured the remission of this restriction in order to get the help of his good Dublin friend Harvey for the Algae and the English mycologist, Berkeley, for the Fungi. In each instance, however, an American had to be joint author. According to Collins [76] the publisher's announcement of the second (trade) edition of Wilkes's *Narrative* said that "Everything about this work will be *strictly American*". Collins surmised that the reversal in order of the names of the of the authors of the part on Algæ, from Harvey and Bailey (given as authorities for individual species) to Bailey and Harvey on the subtitle of the official volume, indicated not carelessness, but patriotism!

In his discussion of the Expedition Algæ Collins (p. 59) says: "It is not difficult to understand why the Harvey and Bailey paper has been forgotten. It is possible that a careful search would find authors who mention it, but the only instances in algological papers that have come to my notice are Setchell and Gardner, and Tilden. . . . It may be of interest to note that specimens of many of the species, some of them evidently the individuals from which the plates were drawn, are in the herbarium of Brown University, Providence, Rhode Island, where Professor W. W. Bailey, son of J. W. Bailey, was for many years the head of the department of botany."

Jacob Whitman Bailey (1808–1857), the best American phycologist of his day, was a microscopist of note. He was Pro-

[76] Collins, Frank S., "The botanical and other papers of the Wilkes Exploring Expedition". *Rhodora,* 14: 57–68, 1912.

fessor of Natural Sciences at West Point Military Academy, and studied not only Algæ but Infusoria as well. He had many European correspondents. His Irish colleague, W. H. Harvey (1811–1866), was a personal friend as well as a correspondent, and likewise a close friend of Gray's. Harvey was a guest of the Grays at Cambridge for seven weeks in 1849, and then visited his botanical friends and acquaintances in New York and Philadelphia. Gray wrote W. J. Hooker (December 3, 1849): "Harvey is a most winning man; my wife and I have become extremely attached to him, and are sorry to part with him. We do not mean to let any naturalist be idle who comes to this country, so he is already engaged to give illustrations of our peculiar Algæ for the Smithsonian Contributions and to prepare (after his return home, of course) a manual of United States Algæ after the fashion of his second edition of 'British Algae' ". This led to the writing of his well-known "Nereis Boreali-Americana", and during the short remainder of his life he had close and very friendly associations with American botanists.

Among the Lichenes collected by the Expedition, which were described by Tuckerman, there were about 8 miscellaneous new species and varieties.

Edward Tuckerman (1817–1886) was all-round systematic botanist, but he attained international reputation as an authority on lichens. His work in this group was well under way by 1847, when he issued the first of his six fascicles entitled "Lichenes Americae septentrionalis exsiccati". There was no one in the world better qualified than he to study the Lichenes of the Expedition. If their number had been greater the published results would have been more notable! Tuckerman is chiefly known for his *Synopsis of the North American Lichens,* of which the second volume was not issued until 1888, two years after the death of the author.

Berkeley and Curtis described only some nine new Fungi collected by the Expedition. Rev. M. A. Curtis (1808–1872) was one of the pioneer American mycologists. He was a New Englander by birth but removed early to the South, and did much of his botanical work near his home at Hillsboro, North Carolina. Most of his publications on fungi resulted from his collaboration with Rev. Miles Joseph Berkeley (1803–1889) who for many

years acted as a clearing house of mycological information for American as well as many British and British Colonial correspondents. He was a graduate of Cambridge University and became a Fellow of the Linnaean Society in 1836. His publications on American and other fungi were very numerous.

Torrey's report upon the flowering plants collected in Washington Territory, Oregon, and California would have been a much more significant contribution if it had been issued when it was written. As finally published its timeliness had passed and it contained little that was new. It was held twelve years unpublished, from 1861 to 1873, and during this time much that was novel found publication elsewhere. After the death of Torrey, a giant in American botany whose career needs no summary here, this manuscript was edited by Gray.

The most remarkable American plant found by the Expedition was the Californian pitcher plant, *Darlingtonia californica* Torr., of which the first specimens, comprising leaves and scapes but no flowers or fruit, were collected by Brackenridge in marshes at the headwaters of the Sacramento River near Mt. Shasta. Near the same place it was long afterward collected in flower by Dr. G. W. Hulse, and still later brought into cultivation—all before the Expedition report was issued. The reason for the thirty-year delay in publication was that it was originally intended that the flowering plants should be reported upon by Rich, the botanist of the Expedition. His report was submitted, but condemned by Wilkes as scientifically unacceptable. In this verdict, it is reported, competent botanists concurred, but so much time had been lost and money expended that Congress had become exasperated at the delay and provided only unwillingly for the substitute reports, and not at all for the publication of Dr. Gray's final volume, which was to have been Vol. XVIII.

Vol. XIX, Pickering's volume on the *Geographical Distribution of Animals and Plants,*[77] is missing from the Michigan set, and has not been available for review.

[77] United States / Exploring Expedition. / During the Years / 1838, 1839, 1840, 1841, 1842. / Under the command of / Charles Wilkes, U.S.N. / Vol. XV. / The / Geographical Distribution / of Animals and Plants. / By / Charles Pickering, M.D., / Member of the Scientific Corps attached to the Expedition. / Boston: / Little Brown & Co. / London: / John Murray, Albemarle Street. / 1854. (A thin volume, seen in the Library of Acad. Nat. Sci., Philadelphia.)

The Report on Reptiles and Batrachians, Vol. XX

The report on *Herpetology*[78] was the only one prepared under the superintendence of an officer of the Smithsonian Institution. This circumstance had somewhat to do indirectly with the transfer of the collections of the Expedition to the Smithsonian, which took place formally in 1857. Curiously enough, the name of the actual author of the report does not appear at all on the title-page of the official edition, which, in this instance, is truly a different edition rather than merely a different issue. The author was Charles Frederic Girard (1822–1895), pupil and assistant of Louis Agassiz, who came with Agassiz to America in 1847 and remained with him in Cambridge until Baird went to the Smithsonian Institution in 1850 as Assistant Secretary. One of Girard's chief assignments as Baird's assistant was to write the Herpetology of the Wilkes Expedition. The arrangement had been made with Wilkes that the work should be done under the superintendence of Baird, and Baird's name is the only one on the title-page. The introduction has the following statement:

> The Joint Committee of the Library of Congress entered into an engagement with the undersigned, in 1851, to prepare the Report upon the Herpetological collections made by the United States Exploring Expedition. Finding that other duties would interfere with the proper performance of the work, he was permitted to associate Dr. Girard with him in its execution; by whom the determinations and descriptions have been made, the drawings overlooked, and the work carried through the press.

Most of the material of the report was prepublished under the sole authorship of Girard as a series of papers in the *Proceedings of the Philadelphia Academy* and the English of the official report, that of Girard, throughout, shows that Baird did not even bother to polish it up from a stylistic standpoint although it was to be attributed to him. Goode[79] says: "This book was not written by Professor Baird, who assures me that he did

[78] Vol. XX. / Herpetology. / Prepared under the superintendence of / S. F. Baird. / With a folio Atlas. / Philadelphia: / Printed by C. Sherman & Son. / 1858. [xv + 492 pp.]

Atlas. / Herpetology. / Prepared under the superintendence of / S. F. Baird. / By Authority of Congress. / Philadelphia: / C. Sherman & Son, Printers. / 1858. [10 pp. + 25 partly colored pl.]

[79] Goode, G. B., "Bibliographies of American Naturalists. V. The published writings of Dr. Charles Girard". *Bull. U. S. Nat. Mus. 41.* Washington, 1891. The following reference to the author's edition is quoted from this source.

not touch pen to it. The book was done entirely by Dr. Charles Girard, but through some technicality his name was not allowed to appear on the title page by the naval authorities having the matter in charge, who insisted in publishing the book under the name of Professor Baird, to whom the original contract was given out."

In view of this curious situation, the author's edition[80] had an entirely different title-page, and was modified slightly with regard to the introduction and the number of plates in the Atlas.

The first Secretary of the Smithsonian Institution, Joseph Henry, was for several years opposed to the Institution taking on museum activities, and was especially averse to taking over any responsibility for material which had already been scientifically exploited and published upon. He had, at the beginning of his distinguished career as Secretary, little conception of the future historical importance as standards of reference of what we now call type specimens. The group who had organized the National Institute, however, were keenly alive to the value of the authentic specimens that were being accumulated in ever increasing numbers by the several surveying expeditions, of which there were already many in the decade following the Wilkes Expedition. After jurisdiction over the collections of the Expedition had been taken away from the National Institute, officers of the Institute protested to Congress, using in a long and vigorous memorial the following striking sentence: "In justice to the Institute it should also be borne in mind that, but for its efforts, these very specimens from the exploring squadron would have been scattered, we know not where; and but for those efforts the scientific describer might have searched in vain for a specimen upon which to found a description or to prove a discovery." It was the wish of Congress to turn over to the Smithsonian Institution, when that was finally organized, the custody of the national collections. Secretary Henry was determined that the

[80] Vol. XX. / Herpetology. / By Charles Girard, / Doctor in Medicine and Surgery, / Corresponding Member of the Boston / Society of Natural History; the Academy of Natural Sciences of Philadelphia; /the Lyceum of Natural History of New York; the Elliot Society of / Natural History of Charleston, South Carolina; / the California Academy / of Natural Sciences, San Francisco; the "Societé Helvetique / des Sciences Naturelles," the Naturforschende Gesell- / schaft in Zurich; and the Societé des Sciences / Naturelles de Neuchatel (Switzerland) etc. / with a folio atlas. / Philadelphia: / J. B. Lippincott & Co. / 1858. [4to pp. i–xvii, 1–496, pl. i–xxxii. "Several additional plates were added in the atlas to this edition."]

capacity of the Smithsonian Institution to promote original research should not be hampered by taking on museum functions. With regard to the Expedition collections he said in 1849: "Moreover, all the objects of interest of this collection have been described and figured in the volumes of the expedition, and the small portion of our funds which can be devoted to a museum may be better employed in collecting new objects, such as have not yet been studied, than in preserving those from which the harvest of discovery has already been fully gathered."

During the long wait between Baird's application for the curatorship of natural history at the Smithsonian Institution and the receipt of the appointment, Dana had written Baird (under date of August 27, 1849) as follows:[81] "As to your application to Professor Henry. The fact is that Henry has no idea of requiring, yet a while, a curator. He intends to have nothing to do with the Exploring Expedition Collections, or any other government property. I regret that he takes this stand,—for collections are better than books to the naturalist; they contain the whole that was ever put in words on the subject, and they illustrate a thousand times more. He is more interested in the library and publications—both very important purposes—but the plan is one-sided, and not of the wide comprehensive character I had expected from Henry."

Henry had much right on his side, for there was really no reason for the government to force upon a private endowment the burden of an ever-growing public function, but there was quite as much to be said on the side of the advocates of the museum idea who were the nucleus of the old National Institute group.

It was Baird, the new Assistant Secretary, who tactfully reconciled Henry to the Smithsonian taking over the administration of all the collections, and he began by taking over the reptiles of the Wilkes Expedition. Henry's opposition had had the permanently advantageous result that the public treasury eventually bore the cost. The dream of the founders of the National Institute of a great National Museum came true, and Baird was the one who finally brought it about.

He knew from the moment of undertaking responsibility for

[81] Quoted from: Dall, William Healey, *Spencer Fullerton Baird, a Biography.* Philadelphia and London, J. B. Lippincott Company, 1915. (See p. 189.)

the "superintendence" of the report upon Herpetology that Girard would do the work.[82] In taking on this responsibility, he had the foresight to begin to concentrate in the Smithsonian the care and study of the specimens of the Wilkes Expedition and also the many western and boundary surveys that were to pour scientific materials into Washington for decades, forming a public treasure of inestimable value. He made a beginning by taking over the remaining Expedition material on which the research was still to be done. This would seem reasonable to Henry, who could gradually be converted to an appreciation of the value and importance of the other collections. Secretary Henry at first had gone only so far as to agree that since a Museum was prescribed by law in the Smithsonian charter, he would start it, but he would accumulate only new objects in it to which the Government had no prior claim. These limitations, Dall tells us, were loyally accepted by Baird and prevailed until Henry was really convinced that a broader policy should be adopted. Then the Wilkes and other neglected collections belonging to the Government were taken over for permanent deposit, and became the present National Museum. Baird's services to the great Expedition are therefore not to be minimized in appraising its final significance for the advancement of Natural History in America. It was the responsibility for taking care of the Expedition collections that made the development of a great National Museum inevitable.

As was true of too many of the Expedition reports, Girard's had its importance greatly diminished by the prior publication elsewhere of most of the descriptions of new discoveries. Only the part on Chelonia, with subtitle dated 1857, was never published in any other form. Of course the illustrations gave some value to the final official report that the prepublished descriptions did not have.

Girard's work seems anomalous from a modern standpoint because of the large number of established names which he changed through sheer whim. When Girard had occasion to

[82] A letter of Baird's to G. P. Marsh (Feb. 9, 1851) says: "I have undertaken the reptiles of the Exploring Expedition, to be completed in 2 years at 500 dollars per annum. The work and pay I shall turn over to my assistant Charles Girard, formerly with Agassiz, who is more competent for the labor than I am. It will, however, require a good deal of my time, at the most favorable view." (Dall, W. H., *Spencer Fullerton Baird, a Biography.* See p. 257.)

establish a new genus by removal of species from an old one, or to transfer a species from one genus to another already established, he as often as not proposed entirely new specific names and gave entirely new diagnoses and descriptions, which, from the standpoint of a modern "splitter", might have validated some of his new names if he had not thrown them out of court at the start by citing as synonyms perfectly valid older names based upon entirely different types. Non-systematists who groan at new names that have to be proposed in order to bring nomenclature into conformity with definite rules and who often seem to think of nomenclatural instability as a modern pest, would be amazed if they realized the capriciousness with which workers of past generations changed names for no especial reason at all.

Girard named new species in honor of Commander Wilkes and Mr. Drayton, the chief artist. Also, it is quite interesting to find among his new species one dedicated to a Polynesian chief, namely *Hoplodactylus Pomarii,* "brought on board by King Pomare, in March, 1840, while the Expedition was tarrying at the Bay of Islands, New Zealand". Chamisso had set the precedent for thus honoring Polynesian royalty in naming a Hawaiian plant *Cyathodes Tameihameiae,* in honor of King Kamehameha of the Hawaiian Islands, and was followed by Asa Gray,[83] who named another species of the same genus *Cyathodes Pomaræ* with this note: "The common Hawaiian species having been dedicated to a celebrated king of those islands, this may bear the name of the gentler Tahitian queen, Pomare."

APPENDIX. PARALLEL REFERENCES TO PEALE'S NEW MAMMALS AND BIRDS IN THE TWO "EDITIONS" OF VOL. VIII OF THE WILKES REPORTS, WHICH WERE, IN REALITY, DISTINCT WORKS

In view of the excessive rarity of Titian R. Peale's *Mammalia and Ornithology* (1848) which constituted the original Vol. VIII of the Reports of the Wilkes Expedition, I have considered it desirable to index the new species which it contains and to indicate what disposition of them was made in the corresponding

[83] Gray, Asa. "Characters of new or obscure species of plants of monopetalous orders in the collection of the United States South Pacific Exploring Expedition under Captain Charles Wilkes, U.S.N." *Proc. Amer. Acad. Arts and Sci.,* 5: 321–352, 1862.

volume by Cassin which was substituted for Peale's in 1858. I have used the copies in the General Library of the University of Michigan. That of Peale is the "official" edition. That of Cassin is the so-called "author's" edition. It is well known that each author was permitted to arrange with a publisher to place on sale 150 copies, which were printed in excess of the edition of 100 that was ordered by Congress for distribution to designated foreign governments, states, institutions, and individuals. It appears that Peale did not avail himself of this privilege in time to secure an adequate distribution of his work before the issuance of the "official" edition was stopped by order of Wilkes. Some of the officially designated recipients of the full series of reports did, however, receive copies of Peale's volume, but never, it seems, officially received Cassin's. This I infer from the fact that the Michigan set has Peale's volume in the official edition, and that the copy of the Cassin volume recently added to the University Library is in the "author's" edition, and was formerly in private possession.

The replacement of Peale's volume by Cassin's has often been alluded to, as, for example, by Witmer Stone,[84] who did not make it clear, in alluding to "Cassin's edition", that this was a distinctly different work, although based upon the same material and incorporating much of Peale's text by direct quotation.

The utility of the following notes will be indicated by the citation of the Mindanao hornbill. In recent works upon Philippine natural history, this bird appears under the name *Hydrocorax mindanensis* (Tweeddale), which was published much later than *Buceros obscurus* Peale. It should now be called *Hydrocorax obscurus* (Peale). Cassin considered that Peale's species was

[84] Stone, Witmer. "An interesting bit of manuscript." *Auk*, 17: 179–180, 1900.

"I have elsewhere explained how part of the edition of the Birds of the U. S. Exploring Expedition by Peale was burned, and that owing to the very small number of copies published, the volume became very scarce, especially in Europe, where it was in great demand. The plates, which really did not appear until Cassin's edition of the Report came out, were supposed at the time to have been issued with Peale's volume.

"In this connection the following inscription in Bonaparte's writing is of much interest. It was written on the cover of a copy of Bonaparte's 'Notes sur Les Larides' ext. de *la Rev. et Mag. de Zool.*, 1854, which I picked up in an old book store in Philadelphia. 'Titian Peale, Esq., Zoologist, etc., from his friend the author. How can I manage to get a copy of the Am. Expl. Exp. Zool. in exchange or sale? The Plates have not yet reached Europe.' Below, in Peale's hand is 'Rec'd June 14, 55. T. R. P.' "

the same as *Buceros hydrocorax* Linn., now known in Philippine zoology as *Hydrocorax hydrocorax* (Linn.). In this, and probably in some other instances, Cassin went too far afield to find species to which Peale's might be reduced, and the more modern conception of geographic species might justify the reinstatement of others of Peale's species, as in the instance just cited. Some interested zoologist might do long-deferred justice to Peale by examining into the validity of all of his other proposals.

In view of the possibly confused bibliography of the reports of the Wilkes Expedition, I shall take the precaution to indicate the exact issues of the two volumes which I have indexed for new names, as follows:

United States / Exploring Expedition. / during the Years / 1838, 1839, 1840, 1841, 1842. / Under the Command of / Charles Wilkes, U.S.N. / Vol. VIII. / Mammalia and Ornithology. / by / Titian R. Peale, / one of the Naturalists of the Expedition / Member of the American Philosophical Society, of the Academy of / Natural Sciences of Philadelphia, etc., etc. / Philadelphia: Printed by C. Sherman. / 1848. pp. i–xxv, (17)–338.

The table of contents of this rare volume calls for 84 plates, which were planned, but never issued. It is in the full green morocco binding with the seal of the United States and blind stamped wreath on front and back, also gold stamped on spine with variant of the seal (eagle alighting, grasping in talons, arrows and olive branch). The binder's label reads "Gaskill, Binder, No. 42 S. 5th Street, Philadelphia".

United States / Exploring Expedition. / during the Years / 1838, 1839, 1840, 1841, 1842. / Under the Command of / Charles Wilkes, U.S.N. / Mammalogy, and / Ornithology. / by / John Cassin, / Member of the Academy of Natural Sciences of Philadelphia; of the American Philosophical Society; / of the National Institute; of the Natural History Society of Charleston; of the Lyceum / of Natural History of New York; of the Natural History Society of Montreal; / Corresponding Member of the Zoological Society of London; Honorary / Member of the United Society of German Ornithologists, etc. / With a Folio Atlas. / Philadelphia: / J. B. Lippincott & Co. / 1858. pp. viii + 466.

This copy of the "2d edition" of Vol. VIII, now in the library of the University of Michigan, was owned by Henry Bryant, whose autograph is on the title page. The half title is merely

"United States / Exploring Expedition", omits the volume number, as the title page does, and is printed on the same kind of paper as the body of the volume, whereas the title page is an inserted replacement, printed on a whiter, heavier paper of superior quality. The original black cloth binding does not indicate the volume, and is lettered: United / States / Exploring / Expedition / Mammalogy / and / Ornithology / Cassin / J. B. Lippincott & Co. Fifty-three of the plates which were planned or actually engraved to accompany Peale's volume, but were not issued with it, were published as the Atlas of Cassin's volume. Although not numbered in correspondence with the table of contents and references in Peale's report, they may legitimately be used as adjuncts to his descriptions. The great majority were drawn by Peale himself. The Atlas to Vol. VIII in the Library of the University of Michigan is a portfolio of loose plates with binder's title "Illustrations to Cassin", lacking a title page. The eleven plates of mammals have the caption "U. S. Exploring Expedition. Mammalogy Pl. 1" [-11]; the forty-two of birds, "U. S. Exploring Expedition. Ornithology Pl. 1" [-42].

The citations of new species from the two "editions" give a reference to Peale's volume first, followed by that to Cassin's, which is designated "ed. 2".

MAMMALIA

Vespertilionidæ

pp. 19, 303	*Pteropus vociferus* Peale. Noisy Roussette. From Mangsi, Island of Balabac, Philippine Islands. (Woodcut of skull.)
ed. 2, p. 10	Referred to *Pteropus Macklotii* Temminck.
pp. 20, 303	*Pteropus Samoënsis* Peale. "Pe'a" of the Samoans. From Tutuila, and other islands of the Samoan group. (Woodcut of skull.)
ed. 2, p. 7	Name unchanged.
pp. 21, 303	*Dysopes aurispinosus* Peale. Spinous-eared Bat. Caught at sea, 100 m. from land, south of Cape St. Roque, Brazil.
ed. 2, p. 5	*Molossus aurispinosus* (Peale) Cassin.
pp. 23, 303	*Vespertilio semicaudatus* Peale. Half-tail Bat. "This species was found at the Samoan Islands, and at Upolu was not uncommon."
ed. 2, p. 4	Name unchanged.

	Phocidæ
pp. 30, 305	*Halichœrus antarcticus* Peale. Antarctic Herring Seal. From Deception Island, Antarctic Sea. (Woodcut of skull.)
ed. 2, p. 25	Referred to *Lobodon carcinophaga* (Homb. et Jacq.).
	Cetacea
pp. 32, 305	*Phocæna pectoralis* Peale. Spotted-breast Grampus. Hilo Bay, Island of Hawaii.
ed. 2, p. 28	*Delphinus pectoralis* (Peale).
p. 33	*Phocæna australis* Peale. South Atlantic Ocean, off the coast of Patagonia. (Omitted from Catalogue.)
ed. 2, p. 27	Referred to *Delphinus obscurus* Gray.
pp. 33, 305	*Delphinus albimanus* Peale. White-flipper Porpoise. Coast of Chili, lat. 27° 16′ S., long. 75° 30′ W.
ed. 2, p. 29	Maintained under the same name, with the suggestion that it may be *Delphinus novæ zeelandiæ* Suoy & Gaim.
p. 34	*Delphinus albirostratus* Peale. Pacific Ocean, lat. 2° 47′ 5″ S., long. 174° 13′ W. of Greenwich. (Omitted from Catalogue.)
ed. 2, p. 31	Referred to *Lagenorhynchus cæruleoalbus* (Meyen).
p. 35	*Delphinus lateralis* Peale. In the Pacific Ocean, lat. 13° 58′ N., long. 161° 22′ W. (Omitted from Catalogue.)
ed. 2, p. 32	*Lagenorhynchus lateralis* (Peale).
p. 35	*Delphinapterus borealis* Peale. North Pacific Ocean, lat. 46° 6′ 50″, long. 134° 5′ W. from Greenwich. (Omitted from Catalogue.)
ed. 2, p. 30	*Delphinus borealis* (Peale).
	Ruminantes
pp. 39, 306	*Cervus Lewisii* Peale. (*Cervus macrotis* var. *Columbiana* Richardson, Fauna Boreali-Americana, vol. i, p. 257.) Black-tail deer. "We propose the name *Lewisii*, from the conviction that the above description [Lewis and Clark's description of the black-tailed fallow deer] written forty years since on the Columbia

River, was intended for the animal now before us. In courtesy we should have called it Columbiana, as suggested by Dr. Richardson, in his Fauna Boreali-Americana, but that name would imply locality, and he has not chosen a correct one." Specimens were killed on Feather River, Upper California, and Bay of San Francisco. (Woodcut, p. 43, of forefoot.)

ed. 2, p. 59 — Name retained.

Glires

pp. 44, 306 — *Arvicola montana* Peale. Mountain Field-mouse. Near the headwaters of the Sacramento River in California.

ed. 2, p. 47 — Changed to *Arvicola montanus* Peale.

pp. 45, 306 — *Arvicola occidentalis* Peale. Western Field-mouse. Obtained at Puget Sound.

ed. 2, p. 44 — Name retained.

pp. 46, 306 — *Arvicola californica* Peale. Californian Field-mouse. Bay of San Francisco, California.

ed. 2, p. 45 — Changed to *Arvicola californicus* Peale.

pp. 47, 306 — *Mus exulans* Peale. Wandering Rat. Dog Island, Disappointment Island, Tahiti, Wake's Island, Hull's Island, Pacific Ocean.

ed. 2, p. 38 — Retained under the same name.

pp. 49, 306 — *Mus Vitiensis* Peale. Feejee Rat. Feejee Islands. (Figure of device for "rat-proofing" provisions hung on cords, called "provision safe.")

ed. 2, p. 40 — Retained under the same name.

pp. 51, 306 — *Mus Peruvianus* Peale. Peruvian Mouse. Callao, Peru.

ed. 2, p. 43 — Referred to *Drymomys parvulus* Tschudi.

pp. 52, 306 — Cricetodipus (new genus).

ed. 2, p. 48 — Referred to *Perognathus*, De Wied.

pp. 53, 306 — *Cricetodipus parvus* Peale. Pouched Jumping-mouse. "A single specimen of this singular animal was obtained in Oregon, but no notes were furnished by the person who obtained it. The formation of its hind legs leaves but little room to doubt that its habits are similar to the

jumping mice, *Meriones Labradorius* (Richardson) which are inhabitants of the same region. Its singularly large head, which equals its body in bulk, its ample cheek pouches, long hind legs and long tail, present a general form which is peculiar and altogether very remarkable."

ed. 2, p. 48 — *Perognathus parvus* (Peale).

pp. 55, 307 — *Sciurus fossor*. Burrowing Squirrel. Oregon, found wherever *Pinus Lambertii* grew; also California.

ed. 2, p. 49 — Name unchanged.

BIRDS

RAPTORES

Falconidæ

pp. 62, 308 — *Buteo solitaria* Peale. Solitary Buzzard. "This Buzzard was seen at the island of Hawaii, and from what we could learn from the natives, does not inhabit any other island of the group. . . . The specimen from which the drawing was made, was obtained near Karakakoa Bay, by the Rev. Mr. Forbes, Presbyterian missionary on that station; he transmitted it to Mr. J. K. Townsend, who kindly loaned it to be drawn, —most of our own specimens of birds collected on the island of Hawaii, having been lost at the mouth of the Columbia River, by the wreck of the U. S. ship Peacock."

ed. 2, p. 97 — *Pandion solitarius* (Peale).

pp. 64, 308 — *Circus approximans* Peale. Feejee Harrier. "Our specimen . . . was shot at Mathuata, island of Venua Levu (Feejee Islands)."

ed. 2, p. 101 — Referred to *Circus assimilis* Jardine & Selby.

pp. 67, 308 — *Falco ferox* Peale. Fierce Hawk. Bay of Islands. New Zealand.

ed. 2, p. 89 — Referred to *Teracidea nova zealandiæ* (Gmelin).

p. 308 — *Astur ferox* Peale. Speckled Hawk. New South Wales. (No description.)

ed. 2, p. 429 — Referred to *Astur cruentus* Gould.

pp. 68, 309 — *Astur rufitorques* Peale. Ring-necked Hawk. ". . . found on all the Feejee Islands. . . .

	The . . . adult male from which our description is taken . . . was dashing through the valleys of the island of Ovolau, near the town of Levuka."
ed. 2, p. 90	*Accipeter rufitorques* (Peale).
pp. 70, 309	*Astur bifasciatus* Peale. Two-barred Hawk. "Peninsula of Malacca; our specimens were obtained at Singapore."
ed. 2, p. 430	Referred to *Accipiter badius* Gmelin.

Strigidæ

pp. 74, 309	*Strix Lulu* Peale. Samoan Owl, *lulu* of the Samoans. "This species is quite common at the island of Upolu and other islands of the Samoan Group . . . we obtained numerous specimens. . . . A single specimen was obtained at the island of Ovolau, one of the Feejee Group."
ed. 2, p. 105	Referred to *Strix delicatula* Gould.
pp. 75, 309	*Noctua venatica* Peale. Hunting Owl. Bay of Islands, New Zealand.
ed. 2, p. 112	Referred to *Athene nova zealandiæ* (Gmelin).
pp. 78, 309	*Athene Patagonica* Peale. Patagonian Owl. Patagonia.
ed. 2, p. 109	Referred to *Athene cunicularia* (Molina).

DENTIROSTRES

Laniadæ

pp. 81, 310	*Colluricincla maculosa* Peale. (No common name proposed.) ". . . a common species on all the islands of the Feejee Group. One specimen was obtained at Upolu, one of the Samoan Islands."
ed. 2, p. 143	Referred to *Lalage terat* (Boddaert).
pp. 83, 310	*Ocypterus superciliosus* Peale. (No common name proposed.) New South Wales.
ed. 2, p. 434	Referred to *Artamus superciliosus* Gould.
pp. 84, 310	*Ocypterus mentalis* Peale. Feejee Swallow Shrike. "They inhabit all the islands of the Feejee Group."
ed. 2, p. 141	Referred to *Artamus mentalis* Jardine.

Merulidæ

pp. 86, 311	*Turdus pallidus* Peale. Pale Thrush. Near Valparaiso, Chili.
ed. 2, p. 142	Referred to *Colluricincla harmonica* (Lath.).
pp. 87, 311	*Mimus Peruvianus* Peale. Peruvian Mocking-bird. Near Callao, Peru.
ed. 2, p. 436	Referred to *Mimus melanotis* (Gould).
pp. 89, 312	*Pitta coronata* Peale. (No common name proposed.) "Peninsula of Malacca; we obtained a single specimen at Singapore."
ed. 2, p. 437	Referred to *Pitta cucculata* Hartlaub.

Sylviadæ

pp. 90, 313	*Saxicola fumifrons* Peale. (No common name proposed.) "Found on arid grounds near Callao, Peru."
ed. 2, p. 435	Referred to *Ptyonura mentalis* (Lafresnaye).
pp. 92, 313	*Petroica coccinea* Peale. (No common name proposed.) Sydney, New South Wales.
ed. 2, p. 276	Referred to *Petroica phoenicea* Gould.
pp. 93, 313	*Petroica pusilla* Peale. (No common name proposed.) ". . . not uncommon at the Samoan Islands, particularly at Upolu. . . ."
ed. 2, p. 164	Name retained.
pp. 94, 313	*Regulus plumulosus* Peale. (No common name proposed.) Valparaiso, Chili.
ed. 2, p. 156	Referred to *Euscarthmus parvulus* (Kittlitz).
pp. 95, 313	*Zosterops flaviceps* Peale. (No common name proposed.) Venua Levu, Feejee Islands.
ed. 2, p. 167	Name retained.

Ampelidæ

pp. 96, 314	*Eopsaltria flavifrons* Peale. (No common name proposed.) Island of Upolu, Samoan Islands.
ed. 2, p. 160	Name retained.
pp. 96, 314	*Eopsaltria icteroides* Peale. (No common name proposed.) Samoan Islands.
ed. 2, p. 161	Name retained, and *Eopsaltria diademata* Pucheran referred to it as a synonym.
pp. 97, 314	*Eopsaltria albifrons* Peale. (No common name proposed.) Samoan Islands.
ed. 2, p. 162	Name retained.

Muscicapidæ

pp. 99, 315	*Rhipidura nebulosa* Peale. Samoan Fantail. Upolu, Samoan Islands.
ed. 2, p. 151	Name retained.
pp. 101, 315	*Monarcha cinerea* Peale. (No common name proposed.) Near Bahr, Island of Vita-levu, Feejee Islands.
ed. 2, p. 165	Referred to *Leucocerca Lessoni* (Gray).
pp. 102, 315	*Platyrhynchus albiventris* Peale. "They are not uncommon at the Samoan Islands."
ed. 2, p. 149	Referred to *Myiagra rubecula* (Latham).
pp. 103, 315	*Lepturus brevicauda* Peale. Callao, Peru.
ed. 2, p. 435	Referred (with name corrected to *L. brevicaudus*) to *Muscigralla brevicauda* D'Orb. & Lafr.

CONIROSTRES

Corvidæ

pp. 105, 315	*Corvus leptonyx* Peale. Near Funchal, Island of Madeira.
ed. 2, p. 116	Referred to *Corvus ruficollis* Lesson.
pp. 106, 315	*Corvus Hawaiiensis* Peale. "Alala" of the Hawaiians. Island of Hawaii.
ed. 2, p. 119	Name retained.
p. 315	*Barita cinerea* Peale. No description. (No common name proposed.) Argyle, New South Wales.
ed. 2, p. 431	Referred (as *Baryta cinerea*) to *Strepera anaphonensis* Temminck.

Sturnidæ

pp. 109, 316	*Lamprotornis atrifusca* Peale. (No common name proposed.) Samoan Islands.
ed. 2, p. 124	Referred to *Calornis corvina* (Kittlitz).
pp. 110, 316	*Lamprotornis fusca* Peale. (No common name proposed.) Tonga and Feejee Islands. "At the Feejee Islands we obtained a specimen which is much larger and darker in its colour, although it is apparently the same species."
ed. 2, p. 125	Referred to *Aplonis marginata* (Gould).

pp. 111, 316	*Lamprotornis brevirostris* Peale. (No common name proposed.) Samoan Islands.
ed. 2, p. 126	*Aplonis brevirostris* (Peale).
	Fringillidæ
pp. 115, 318	*Geospiza Peruviensis* Peale (also called *G. Peruviana* Peale). Between Callao and Lima, Peru.
ed. 2, p. 135	Referred to *Volatinia jacarina* (Linn.).
pp. 116, 318	*Geospiza prasina* Peale. (No common name proposed.) Venua Levu, Feejee Islands.
ed. 2, p. 138	*Erythrura Pealei* Hartlaub. (The name chosen by Peale had been preoccupied by *Loxia prasina* Sparrmann = *Erythrura prasina* (Sparrmann).)
pp. 117, 318	*Geospiza cyaneovirens* Peale. (No common name proposed.) Upolu, Samoan Islands.
ed. 2, p. 137	*Erythrura cyanovirens* (Peale).
p. 118	*Camarhynchus leucopterus* Peale. (No common name proposed.) Near Callao, Peru.
ed. 2, p. 133	Referred to *Spermophila telasco* (Lesson).
p. 121	*Fringilla (Niphaea) laciniata* Peale. (No common name proposed.) Vicinity of Valparaiso, Chili.
ed. 2, p. 136	Referred to *Phrygilus alaudinus* (Kittlitz).
pp. 123, 317	*Pipilo cinerea* Peale. Chilian Ground-finch. Along the road from Valparaiso to St. Jago, Chili.
ed. 2, p. 135	Referred to *Euspiza diuca* (Molina).
	Buceridæ
pp. 125, 318	*Buceros obscurus* Peale. Dusky Hornbill. Near Sambuangu (Zamboanga), Mindanao, Philippine Islands.
ed. 2, p. 433	Referred to *Buceros hydrocorax* Linn.
	SCANSORES
	Psittacidæ
pp. 127, 320	*Platycercus splendens* Peale. Splendid Lory. "It was found inhabiting the shores of Peale's River, at the Island of Viti-levu (Great Feejee)."

ed. 2, p. 237	*Aprosmictus splendens* (Peale).
pp. 129, 320	*Platycercus atrogularis* Peale. Mangrove Lory. Feejee Islands.
ed. 2, p. 234	Referred to *Aprosmictus tabuensis* (Gmelin).
	Picidæ
pp. 132, 321	*Meiglyptes fuscus* Peale. Brown Woodpecker. "Inhabits the Malay Peninsula. Our specimen was obtained at Singapore." Catalogue says "Malacca."
ed. 2, p. 443	Referred to *Meiglyptes brunneus* (Eyton).
pp. 133, 322	*Bucco rubritorquis* Peale. Obtained at Singapore. Catalogue says "Malacca."
ed. 2, p. 444	Referred (as *Bucco rubritorques*) to *Megalaima malaccensis* (Hartlaub).
pp. 134, 321	*Cuculus simus* Peale. (Common name not proposed.) Sandal-wood Bay, Feejee Islands.
ed. 2, p. 245	Name retained.
pp. 136, 322	*Cuculus fucatus* Peale. Painted Cuckow. "Island of Mangsi, in the Sooloo Sea."
ed. 2, p. 244	Referred to *Cucculus tenuirostris* Lesson.
pp. 137, 322	*Centropus nigrifrons* Peale. Black-masked Coucal. "A single specimen was killed in a jungle near La Caldera on the Island of Mindanao." Locality also given as Sambuangu (Zamboanga).
ed. 2, p. 249	Referred to *Centropus melanops* Lesson.
pp. 139, 322	*Eudynamys cuneicauda* Peale. Wedge-tailed Eudynamys. Island of Ovolau, Feejee Group.
ed. 2, p. 248	Referred to *Eudynamys taitensis* (Sparrmann).
pp. 140, 322	*Phœnicophæus nigriventris* Peale. Black-bellied Malkoha. Obtained at Singapore. (Woodcuts of heads, showing difference of this species from *P. sumatranus* Raffles.)
ed. 2, p. 445	Referred to *Zanclostomus sumatranus* (Raffles).
pp. 142, 322	*Anadænus ruficauda* Peale. Rufous-tailed Anadænus. Obtained at Singapore; from Malacca.
ed. 2, p. 444	Referred (as *Anadænus ruficaudus* Peale) to *Rhinortha chlorophæus* (Raffles).

Tenuirostres

Meliphagidæ

pp. 145, 323	*Entomiza olivacea* Peale. (No common name proposed.) "We saw them on all the islands of the Samoan Group; at Upolu and Tutuila they were most plentiful."
ed. 2, p. 172	Referred to *Leptornis samœnsis* (Homb. & Jacq.).
ed. 2, p. 439	Referred to *Tropidorhynchus samœnsis* (Homb. & Jacq.).
pp. 147, 323	*Entomiza angustipluma* Peale. (No common name proposed.) Hawaii.
ed. 2, p. 168	*Moho angustipluma* (Peale).
pp. 150, 323	*Myzomela nigriventris* Peale. Black-belly Myzomela. Samoan Islands.
ed. 2, p. 175	Name retained.
pp. 150, 323	*Myzomela jugularis* Peale. Red-throated Myzomela. Feejee Islands. "Several specimens, obtained in the early part of the month of June, at the Island of Venua Levu, had scarlet feathers mixed with the black occipital plumage."
ed. 2, p. 176	Name retained and *Myzomela solitaria* Jac. & Puch. referred to it as a synonym.

Fissirostres

Halcyonidæ

pp. 155, 326	*Dacelo nullitorquis* Peale. (No common name proposed.) Tahiti.
ed. 2, pp. 199, 217	Referred to *Todiramphus divinus* Lesson.
pp. 156, 326	*Dacelo Vitiensis* Peale. (No common name proposed.) Feejee Group. ". . . We have selected a male, which we think is in full plumage, to illustrate the species; it was killed at the Island of Venua Levu. . . ." Also Tonga Islands.
ed. 2, pp. 195, 209	*Todiramphus vitiensis* (Peale).
pp. 159, 326	*Dacelo minima* Peale. (No common name proposed.) Upolu, Samoan Islands.
ed. 2, pp. 198, 216	Referred to *Todiramphus recurvirostris* Lafresnaye.

pp. 160, 326	*Dacelo coronata* Peale. (No common name proposed.) Island of Tutuila, Samoan Group.
ed. 2, pp. 192, 206	Referred to *Todiramphus tuta* (Gmelin).

Trogonidæ

pp. 166, 326	*Harpactes rodiosternus* Peale. (No common name proposed.) Near "Sambuangu" (Zamboanga) Mindanao.
ed. 2, p. 229	Referred to *Harpactes ardens* (Temm.).

Caprimulgidæ

pp. 168, 327	*Caprimulgus æquicauda* Peale. Near Callao, Peru.
ed. 2, p. 188	Referred to *Stenopsis parvulus* (Gould).
pp. 169, 327	*Caprimulgus conterminus* Peale. (No common name proposed.) Near Valparaiso, Chili.
ed. 2, p. 188	Referred to *Stenopsis longirostris* (Bonaparte).
pp. 170, 327	*Caprimulgus bimaculatus* Peale. (No common name proposed.) "Inhabits the Malay Peninsula; the specimen was obtained at Singapore." Malacca.
ed. 2, p. 441	Referred to *Caprimulgus albonotatus* Tickell.
pp. 172, 327	*Chordeiles Peruvianus* Peale. (No common name proposed.) Near Callao, Peru.
ed. 2, p. 189	Referred to *Chordeiles acutipennis* (Boddaert).

Hirundinidæ

pp. 175, 327	*Hirundo rufocollaris* Peale. (No common name proposed.) Near Callao, Peru.
ed. 2, p. 181	Referred to *Petrochelidon fulva* (Vieill.).
pp. 176, 327	*Macropteryx spodiopygius* Peale. (No common name proposed.) Island of Upolu, Samoa; Samoan and Feejee Islands.
ed. 2, p. 184	*Collocalia spodiopygia* (Peale).
pp. 178, 327	*Macropteryx leucophæus* Peale. (No common name proposed.) Island of Tahiti.
ed. 2, p. 183	Referred to *Collocalia cinerea* (Gm.).

Rasores

Columbidæ

pp. 187, 329	*Columba castaneiceps* Peale. Chestnut-head Pigeon. Upolu, Samoan Islands.

ed. 2, p. 252	Name retained.
pp. 190, 329	*Ptilinopus coralensis* Peale. Coral Island Dove. Paumotu Islands.
ed. 2, p. 272	Name retained.
pp. 191, 329	*Ptilinopus furcatus* Peale. Tahitian Dove. Island of Tahiti.
ed. 2, p. 269	Referred to *Ptilinopus purpuratus* (Gmelin).
pp. 193, 329	*Ptilinopus fasciatus* Peale. Samoan Dove: "Manu tangi" of the natives. Samoan Islands.
ed. 2, p. 271	Name retained.
pp. 195, 329	*Ptilinopus Perousii* Peale. La Perouse's Dove: "Manu ma" of the Samoans. Island of Upolu, Samoan Islands.
ed. 2, p. 274	Name retained, and *Ptilinopus Mariae* Jacq. & Puch. referred to it as a synonym.
pp. 200, 330	*Carpophaga latrans* Peale. Barking Pigeon. Feejee Islands. ("Manu mow" of the natives.)
ed. 2, p. 261	Name retained.
pp. 201, 330	*Carpophaga Auroræ* Peale. Aurora Pigeon. Aurora or Maitea Island, Society Group.
ed. 2, p. 256	Name retained. (Woodcut of bill.)
pp. 203, 330	*Carpophaga Wilkesii* Peale. Wilkes's Pigeon. Tahiti: also Aurora or Maitea Island, Society Group.
ed. 2, p. 258	Name retained.
pp. 204, 330	*Carpophaga casta* Peale. Chaste Pigeon. Near "Soung, the capital of the Sooloo Islands. The specimen from which our drawing was made, was killed on an island nearly opposite to Soung. . . ."
ed. 2, p. 265	Referred to *Carpophaga bicolor* (Scopoli).
pp. 205, 330	*Peristera pectoralis* Peale. Tawny-breast Dove. Carlshoff Island, Paumotu Archipelago.
ed. 2, p. 277	Referred to *Peristera erythroptera* (Gmelin).
pp. 208, 329	*Didunculus* Peale (new genus).
ed. 2, p. 279	Referred to *Pleiodus* Reichenbach (1847).
pp. 209, 329	*Didunculus strigirostris* Peale. "Manu mea" of the Samoans. Upolu, Samoan Islands. Woodcut of head. A footnote is as follows: "Since writing the above, Knathodon [*i.e.*,

	Gnathodon] has been proposed as a name for this genus, by Sir William Jardine, Bart. It would give me pleasure to adopt it, as better than the one now proposed, but for the fact that it has been preoccupied in Entomology. T.R.P."
ed. 2, p. 279	Referred to *Pleiodus strigirostris* (Jardine).

GRALLATORES

Ardeidæ

pp. 216, 331	*Ardea patruelis* Peale. Tahitian Green-heron. Tahiti.
ed. 2, p. 297	Referred to *Ardea stagnalis* Gould.

Tantalidæ

pp. 219, 331	*Ibis brevirostris* Peale. Short-billed Ibis. River Rimac, Peru.
ed. 2, p. 302	Referred to *Ibis guarauna* (Linn.).

Rallidæ

pp. 220, 331	*Porphyrio Samoënsis* Peale. Samoan Sultana. Island of Upolu, Samoan Group.
ed. 2, p. 308	Referred to *Porphyrio indicus* Horsefield.
pp. 221, 331	*Porphyrio Vitiensis* Peale. Feejee Islands.
ed. 2, p. 309	Name retained.
pp. 223, 332	*Rallus luridus* Peale. Lurid Rail. Orange Harbour, Tierra del Fuego.
ed. 2, p. 304	Referred to *Rallus sanguinolentus* Swainson.
pp. 224, 331	*Fulica Alai* Peale. Hawaiian Coot; "alai" of the Hawaiians. Sandwich Islands.
ed. 2, p. 306.	Name retained.

Scolopacidæ

pp. 227, 332	*Scolopax pectinicauda* Peale. Singapore.
ed. 2, p. 313	Referred to *Gallinago stenura* (Temminck).
pp. 229, 332	*Scolopax meridionalis* Peale. Southern Snipe. Orange Bay, Tierra del Fuego.
ed. 2, p. 310	Name retained.
pp. 231, 332	*Limosa Foxii* Peale. Fox's Godwit. "Obtained at Rose Island, Samoan Group, collected by Dr. J. L. Fox, U.S.N., to whom we have dedicated the species."

ed. 2, p. 314	Referred to *Limosa novæ zealandiæ* G. R. Gray.
pp. 233, 332	*Numenius femoralis* Peale. Hairy-legged Curlew. Vincennes Island, Paumotu Group.
ed. 2, p. 316	Name retained.
pp. 235, 332	*Tringa parvirostris* Peale. Small-billed Sandpiper. Dog Island, Paumotu Group; also Raraka Island of the same group.
ed. 2, p. 321	Name retained.
pp. 237, 332	*Totanus Polynesiæ* Peale. Polynesian Sandpiper. Feejee Islands (male specimen described); Dog Island, Paumotu Group (female specimen described); others seen at Samoan Islands, Society Islands, and Mathew's Island, Kingsmill Group.
ed. 2, p. 318	Referred to *Totanus oceanicus* Lesson.

Charadriadæ

pp. 240, 333	*Charadrius vanelloides* Peale. (No common name suggested.) District of Argyle, New South Wales.
ed. 2, p. 328	Referred to *Sarciophorus tricolor* (Vieillot).
pp. 244, 333	*Glareola cuneicauda* Peale. Wedge-tailed Pratincole. Island of San Lorenzo, Peru.
ed. 2, p. 288	Referred to *Thinocorus Ingæ* Tschudi.

NATATORES

Anatidæ

pp. 249, 334	*Anser Hauaiënsis* Peale. Hawaiian Goose. Island of Hawaii.
ed. 2, p. 338	Referred to *Bernicla sandwichensis* Vigors.

Alcidæ

p. 260	*Aptenodytes flavilarvata* Peale. Yellow-masked Penguin. Auckland Islands.
ed. 2, p. 351	Referred to *Eudyptes antipoda* (Homb. & Jacq.).
pp. 261, 335	*Aptenodytes longicauda* Peale. Long-tailed Penguin. "Male, obtained by Dr. S. Holmes, of the Brig Porpoise, lat. 64° 40′ S., and 103° 4′ E. from Greenwich." Antarctic icebergs.
ed. 2, p. 352	Referred to *Eudyptes Adeliæ* (Homb. & Jacq.).
pp. 263, 335	*Aptenodytes magnirostris* Peale. Large-billed Penguin. Cape Horn, Tierra del Fuego.

ed. 2, p. 350	Referred to *Spheniscus demersus* (Linn.).
pp. 264, 335	*Aptenodytes tæniata* Peale. White-cockage Penguin. Macquarie's Island, "with the crested penguin, but in separate communities."
ed. 2, p. 350	Referred to *Eudyptes papua* (Forster).
	Pelicanidæ
pp. 268, 336	*Carbo fucosus* Peale. Painted Cormorant. Bay of Islands, New Zealand.
ed. 2, p. 372	Referred to *Carbo hypoleucus* Brandt.
pp. 269, 336	*Carbo purpuragula* Peale. Purple-throated Cormorant. Manua Bay, New Zealand.
ed. 2, p. 374	Referred to *Carbo sulcirostris* Brandt.
pp. 270, 336	*Carbo flavagula* Peale. Yellow-throated Cormorant. Bay of Islands, New Zealand.
ed. 2, p. 375	Referred to *Carbo brevirostris* (Gould).
pp. 274, 336	*Sula rubripeda* Peale. Red-footed Gannet. Honden Island; Enderby's Island; also Wake's Island.
ed. 2, p. 365	Referred to *Sula piscator* (Linn.).
	Laridæ
pp. 277, 337	*Sterna lunata* Peale. Lunate Tern. Vincennes Island, Paumotu Group.
ed. 2, p. 382	Name retained.
pp. 279, 337	*Sterna albifrons* Peale. White-fronted Tern. Bay of Islands, New Zealand.
ed. 2, p. 381	Referred to *Sterna frontalis* G. R. Gray.
pp. 280, 337	*Sterna Antarctica* Peale. Antarctic Tern. Orange Bay, Cape Horn.
ed. 2, p. 385	Becomes *Sterna meridionalis* Cassin, not *S. antarctica* Lesson, Traité, p. 621, 1831.
pp. 281, 337	*Sterna rectirostris* Peale. Straight-billed Tern. Feejee Islands, particularly Sandalwood Bay.
ed. 2, p. 384	Referred to *Sterna poliocerca* Gould.
pp. 285, 337	*Megalopterus plumbeus* Peale. Lead-colored Noddy. Honden Island, Paumotu Group.
ed. 2, p. 393	Referred to *Anous parvulus* Gould.
pp. 288, 337	*Larus albipennis*. White-quilled Gull. Valparaiso, Chili.
ed. 2, p. 379	Name retained.

pp. 293, 337	*Thalassidroma lineata* Peale. White-lined Petrel. Island of Upolu, Samoan Group.
ed. 2, p. 403	Name retained.
pp. 294, 337	*Procellaria brevipes* Peale. Short-footed Petrel. Lat. 68° S., long. 95° W., Antarctic Ocean.
ed. 2, p. 414	Referred to *Procellaria Cookii* Gray.
p. 295	*Procellaria candida* Peale. Snowy Petrel. In the vicinity of ice, lat. 64° S., 104° W. of Greenwich. Published (with description) as a synonym of *Procellaria nivea* Gmel. because the identity with the older species was not established until after the Plate had been engraved.
ed. 2, p. 451	Here it is not clear from the form of the citation that Peale's binomial was originally published as a synonym.
pp. 296, 338	*Procellaria rostrata* Peale. Large-billed Petrel. Island of Tahiti.
ed. 2, p. 412	Name retained.
pp. 298, 338	*Procellaria parvirostris* Peale. Small-billed Petrel. Near Honden Island, Dangerous Archipelago.
ed. 2, p. 411	Name retained.
pp. 299, 338	*Procellaria gularis* Peale. White-throated Petrel. Amidst icebergs, Antarctic Ocean, lat. 68° S., long. 95° W. of Greenwich.
ed. 2, p. 410	Referred to *Procellaria mollis* Gould.

THE EXPLORING EXPEDITION IN THE PACIFIC

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(Read February 23, 1940, in Centenary Celebration of the Wilkes Exploring Expedition, 1838–1842)

THE attention focussed upon the heroic Antarctic cruise of our first United States Exploring Expedition has caused the other important accomplishments of this expedition to suffer undeserved neglect. The total amount of time occupied in the two excursions into high southern latitudes was less than one twelfth of the time of the entire cruise, and the chart of the Antarctic Continent was only one of well over one hundred charts resulting from the labors of the expedition. Although the hope of making discoveries in polar regions had attracted some interest and support to the expedition, its primary purpose was to survey and explore the islands, reefs, and shoals of the Southern Pacific, particularly in those regions frequented by our vessels engaged in the whale fisheries and other ocean commerce.

Probably no officer in the navy was better fitted to undertake the direction of such surveys than Lieutenant Charles Wilkes, the commander of the expedition. At an early age he had been given a sextant and taught to understand thoroughly its operation and adjustment. As a boy before the mast in the merchant service and later as a midshipman on the *Franklin* he had on many occasions been conspicuous for his unusual knowledge of nautical astronomy and the skill and dexterity with which he handled the instruments. While on leave from active naval service he had studied with Ferdinand Hassler, the eminent Swiss geodesist, who was the first superintendent of the United States Coast Survey, and from Hassler, Wilkes had learned the very accurate methods of nautical surveying which had been developed in Europe. This knowledge served him in good stead when he was on the survey of Narragansett Bay under Captain Wadsworth in 1832, and again in 1837 when he was placed in charge of the survey of Georges Bank.

Full of zeal to learn and to put his learning into practice, Wilkes felt that the lack of incentive for naval officers to keep up their scientific interests was deplorable, and in the Exploring Expedition he saw the fulfillment of a long felt need—the opportunity for naval officers to put their scientific accomplishments to some use and thereby win some credit to themselves and to the nation. With this in mind, he insisted that the duties pertaining to astronomy, surveying, hydrography, geography, geodesy, magnetism, meteorology, and physics, should be confided to naval officers rather than to civilian scientists. Not at all loth to assume the heaviest burdens himself, he willingly undertook to direct the work of all these departments. It is worthy of note that, although he, like Pooh-bah in the Mikado, occupied all of the important positions, he did not, like that gentleman, receive the corresponding salaries.

He was too much of an idealist to realize that not all of his officers would share either his intense eagerness or his capacity to forward the scientific work. Several of them were inclined to look on an exploring expedition as something of a pleasure cruise, and felt much put upon when they were required to take hourly temperatures of the air and water, try for currents, and continuously devote themselves to innumerable scientific observations in addition to their ordinary naval duties. When the natural shortcomings of some of his officers, and the disinclination of others, became apparent to Wilkes, his disillusion was extreme, and he was then inclined to underrate their abilities and intentions as much as he had overrated them in the beginning, and in his more gloomy moments he thought that they were intentionally thwarting the objects of the expedition. When this situation is considered, it is hardly surprising that occasional tempestuous scenes took place; it is rather to be wondered at that they were not more frequent and more serious than they were.

The expedition had already been absent from the United States almost one year before the surveying operations in the Pacific were begun. This was unfortunate in that the officers had lost their first fine enthusiasm, but it was unavoidable. Unfortunate also was the loss of the schooner *Sea Gull,* which had never been heard from after leaving Orange Harbor, Terra del Fuego, in April 1839. It had been expected that the two small

schooners, the *Flying Fish* and the *Sea Gull,* former New York Harbor pilot ships, would be extremely useful in working among the coral islands, as they could venture into shallow water and among the reefs more safely than the larger ships. The squadron, originally consisting of six ships, was after leaving Callao Peru reduced to four, the two sloops of war *Vincennes* and *Peacock,* commanded by Wilkes and Hudson respectively, the brig *Porpoise* commanded by Ringgold, and the schooner *Flying Fish,* which was commanded by several different officers at different times. There were in addition seventeen small auxiliary launches and cutters.

Most of the surveys were to be made among the low coral islands of the Pacific, and Wilkes devised an effective procedure for accomplishing them. When an island was to be surveyed, the larger vessels took stations off-shore, and smaller boats were dispatched to positions along the reefs or on land. Each of the larger vessels in turn, after hoisting a signal, fired a gun so that the distance to that vessel from each of the others and from the small boats could be measured by sound, and at the same time angles were taken to the different ships and to objects on shore. From the smaller boats angles were taken between the different vessels stationed off-shore. At the time of each set of observations, the altitude of the sun was taken in order to ascertain the exact time, angles were measured to calculate the azimuth of some one of the objects, and the altitude and azimuth of the mast of one of the ships were observed. When all of the desired observations had been made, one of the larger vessels moved into a new position, and a new set of observations was taken. In this way the survey progressed around the island. Frequently the ships engaged in the work would split into two parties, one going around the island in a clockwise direction, and the other counter-clockwise, and the two parties would meet and close the survey on the opposite side of the island from the starting point. The deck boards were then sent to the flagship where the survey was calculated and plotted. By this method an island about seven miles long was surveyed in three hours and thirty-five minutes.

The manner in which the observations were taken and recorded was very definitely prescribed by Wilkes, and a syllabus of instructions was issued to the officers. Any officer who failed

to follow the methods indicated was certain to be taken to task, and in a number of instances surveys were required to be done over again when the first results did not come up to the high standard set by the commander.

The first coral island to receive the attention of the squadron was Clermont de Tonerre, one of the easternmost islands of the Low or Tuamotu archipelago, which they reached on August 13, 1839. The unprepossessing appearance of this island was a dis-

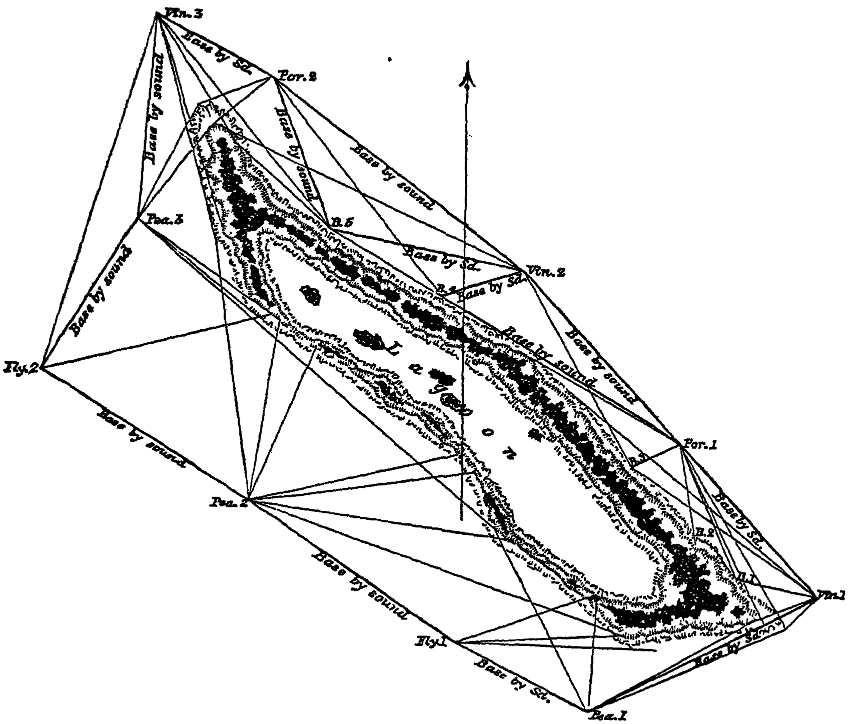

FIG. 1. Wilkes' diagram to illustrate the method of surveying a coral island. From Charles Wilkes, *Narrative of the United States Exploring Expedition*, Lea and Blanchard, Philadelphia, 1845, Vol. I, p. 431.

appointment to those who had pictured the coral islands as a sort of fairyland. On the first day some of the officers and naturalists went ashore and made collections of shells, but later when Wilkes wished to land with magnetic instruments, the natives showed opposition. Mr. Couthouy, the conchologist of the expedition, attempted to win them over by venturing through the surf naked with an armful of trinkets, but he was forced to beat

a hasty retreat. Not wishing to harm the natives, and yet anxious to effect the landing, Wilkes ordered some mustard shot to be fired at their legs, which caused them to withdraw. Unfortunately so much time had been taken up in attempted negotiations that it was too late to take the desired observations.

There was a difference of seventeen minutes in the longitude of Clermont de Tonerre as ascertained by the British and French navigators, Captain Beechey and Captain Duperry respectively. The French position was found to be correct. When the survey of the island was completed the squadron moved toward the northwest. The vessels separated in order that a wider area might be covered, and during the four weeks spent in the Low Archipelago in 1839, fifteen islands were surveyed. Two of these were not to be found on any of the charts in Wilkes' possession, and hence were considered to be discoveries. One was named Vincennes Island after the flagship, the other King's Island after the man at the masthead who discovered it. Seven more islands of this group were surveyed in 1840 and 1841 by Lieutenant Ringgold in the *Porpoise*. The resulting charts are still issued by the Hydrographic Office of the United States Navy Department.

On the 10th of September 1839 the *Vincennes* reached Tahiti, where the chronometers were checked at Point Venus, so named from Captain Cook's observation there of the transit of Venus in 1769. The other ships came in within the next few days, and soon the work of charting the important harbors of the Society group was well under way, and parties of naturalists and officers made several inland excursions.

In early October the squadron started to survey and explore the Samoan or Navigator Islands. Here again the vessels separated in order to cover more territory. The *Vincennes* took the island of Tutuila, the *Peacock* and *Flying Fish* were to go to Upolu, and the *Porpoise* to Savaii. The ships reassembled in the harbor of Apia, Upolu, on November 10th. The surveys had all been completed except for the south side of the island of Upolu. The *Peacock* and *Flying Fish* had been delayed at Tahiti on account of repairs to the latter, and had not been able to accomplish the part of the work assigned to them. This omission was made good in 1840 and 1841.

The squadron then sailed for Sydney, New South Wales,

where Wilkes astonished the local authorities by boldly bringing the *Vincennes* followed by the *Peacock* into Sydney Harbor at night without a pilot. The beating of the roster drum and the firing of the daylight gun of the *Vincennes* were the first intimation to the populace that two foreign sloops of war were in the harbor.

The energies of the expedition for the next four months were occupied in preparing for and carrying out the historic Antarctic Cruise, which will not be described here. On April 6, 1840, the *Vincennes, Porpoise,* and *Flying Fish* had been reunited after the Antarctic Cruise and were sailing from the Bay of Islands, New Zealand, toward Tongataboo in the Friendly Islands, where it was expected that the *Peacock,* which was undergoing repairs at Sydney necessitated by her encounters with the Antarctic ice, would join them again.

There were hostilities at this time in the Friendly Islands between the so-called Christian and Devil's parties. Wilkes did not consider it proper to give active assistance to either party, particularly as a large share of the wrong seemed to lie with the Christian party, but he did attempt to bring about a reconciliation between them. His efforts to produce mutual appeasement did not meet with any success, and the state of the country interfered with the intended surveying operations. On the 4th of May the four vessels left Tongataboo and sailed for the Fiji Islands, where they remained until the end of the first week in August. The *Porpoise* was detached to survey the eastern part of the group, and the other vessels and all the small boats were soon busily employed in making the surveys of the main part of the group.

In addition to the scientific work, certain diplomatic errands were carried out. Commercial regulations governing the relations between the natives and foreigners were drawn up and agreed to by the principal chiefs, and one native chief, Vendovi, who had been implicated in the massacre of the crew of the American Brig *Charles Doggett* in 1834, was made a prisoner in the hope that this act would have a salutory effect and prevent such outrages.

As the natives of these islands were known to have made treacherous attacks on crews of ships, it was necessary for the surveying parties to be exceedingly cautious. According to na-

tive tradition, any boat driven on a reef or on shore was held to be an offering to the gods and a legitimate object of pillage, and they were always eagerly on the watch for such a happy accident to take place. The first cutter of the *Vincennes* was captured at Sualib Bay when cast on a reef, and although the boat itself was finally returned after some negotiations and threats, so much of her equipment was retained by the savages that it was thought expedient to punish them for this theft, and a village was burned with the loss of some supplies but no lives.

In late July when Wilkes was congratulating himself that the surveys of this group were nearly completed, a more serious misfortune occurred when two young officers, Lieutenant Joseph Underwood and Midshipman Wilkes Henry, were set upon and killed while they were attempting to purchase provisions from the natives at Malolo. A seaman, Joseph Clark, was severely wounded during the same encounter.

The journals of several of the officers testify to the overwhelming lust for revenge which pervaded the whole company. Yet Wilkes, who was most closely affected by the tragedy, for Midshipman Henry was his own nephew, realized the necessity of keeping this thirst for vengeance from exceeding all control. Lieutenant Ringgold was sent on shore with a party of sixty men to attack the native fortified village. The savages apparently believed their village to be impregnable, for they shouted tauntingly at their attackers, but they were no match for the Americans who, though greatly outnumbered, were able to fire the village and drive out the inhabitants. It was estimated that fifty-seven natives were killed, while one or two of the Americans received slight wounds.

The next day the surviving natives sued for peace, and promised that they would never again allow any white man to be harmed. Although back in the United States Wilkes was accused of wanton cruelty for ordering this attack, it is evident that most of his men felt that the punishment was too mild.

Since the survey of the Fiji group was considered one of the best made by the expedition, it is interesting to know the opinions of some of the members of the expedition at the time. One of the younger officers, acting-master George Sinclair of the *Flying Fish,* commented on the survey in his journal as follows: "The whole group with its attendant reefs has been surveyed by

the Expedition in a manner which must reflect great credit upon it, when all things are taken into consideration. I hope and trust that Capt. Wilkes will not claim too much for the chart, which I have no doubt will be a very handsome one. If he publishes the chart as completed and as one of perfect accuracy, he will claim for it more than it is entitled to, and if the ground should ever be reëxamined by future navigators, with more time to devote to the work than we had, we will lose the credit to which we are most justly entitled, of having accomplished in the short period of three months, so vast a work with even an approach to accuracy; but I claim for this survey more than an approach to accuracy, although I cannot claim for it perfection. If we had had provisions for two months longer we could and would have made it perfect. . . . There are many parts of this group that could not be bettered if we had years to devote to the work, and there are again many parts that from want of time we have been compelled to hurry over and slight.''[1]

The squadron left the Fiji Islands on August 11, 1840, and sailed for the Hawaiian group. On the way the *Vincennes* examined several of the islands of the Phoenix group, two of which were not found on any of Wilkes' charts. One of these was named Hull's Island after Commodore Isaac Hull of the United States Navy, and the other McKeen's Island after the man who first saw it.

It had originally been intended that the squadron would go next to the west coast of North America, but Wilkes felt that the season was too far advanced for the surveys there, and he decided to postpone that work until the next spring. The *Porpoise* was sent back to the Tuamotu group to complete the surveys there, while the *Peacock* and the *Flying Fish,* after making a few excursions in the Hawaiian Islands, were sent back to the Samoans with orders to visit also the Ellice and Kingsmill groups. The *Vincennes* remained in the Hawaiian Islands while Wilkes and his men effected the remarkable exploit of transporting the pendulum and other instruments to the summit of Mauna Loa, over 13,000 feet above sea level, and making a series of observations there. The difficulties of this undertaking can best be appreciated by reading Wilkes' own account in

[1] Journal number 522, Hydrographic Office, U. S. Navy Department.

his *Narrative.*[2] One of the briefest yet most pertinent comments is to be found in the journal of William Briscoe, armourer of the *Vincennes,* who wrote: "It is no doubt an arduous undertaking, but if it is possible for it to be accomplished by any man, Capt. Wilkes is the one who will succeed, for a more persevering man never lived." [3]

In March the *Porpoise* returned from the Tuamotu Group, and in April the *Vincennes* and *Porpoise* left Oahu and sailed for

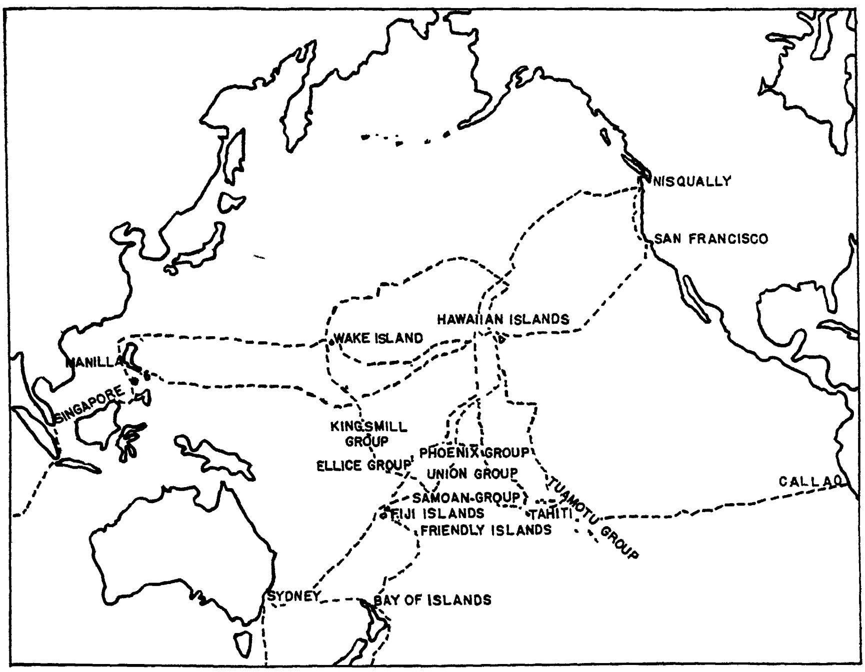

FIG. 2. Generalized route of the United States Exploring Expedition in the Pacific Ocean.

the coast of Oregon Territory. They expected to go first to the Columbia River, but when they approached the bar at its mouth, the waves were breaking so furiously that Wilkes decided to proceed to the Straits of Juan de Fuca rather than wait for more favorable weather to cross the bar. This was done, and after narrowly missing being wrecked near Destruction Isle,

[2] Charles Wilkes, *Narrative of the United States Exploring Expedition during the years 1838, 1839, 1840, 1841, 1842,* Lea and Blanchard, Philadelphia, 1845, Vol. IV, Chapter IV.

[3] Journal number 512, Hydrographic Office, U. S. Navy Department.

they entered the Straits and anchored at Port Discovery on the 2nd of May. After surveying the harbors at Port Discovery and Port Townsend, they anchored at Fort Nisqually at the south end of Puget Sound between the present sites of Olympia and Tacoma, Washington. Surveying parties were organized and dispatched to different parts of Puget Sound, and several land parties were also sent out to explore the country. One of these, commanded by Lieutenant Robert Johnson, crossed the Cascades to the Columbia River and followed it upstream as far as Fort Colville, examining part of the Grand Coulee on the way. They then crossed over to the Snake River, came down it and up the Yakima, and back to Nisqually.

Arduous as all these duties were, time was taken on the 5th of July, the 4th being Sunday, to hold a celebration which has since been recognized as the first public observance of that holiday on the Pacific Coast or west of the Missouri River. A parade of sailors and marines followed by games and a feast were the events of the day, and according to one account, "Capt. Wilks & officers joined in the play at a gaime of catt & foot ball."[4] In 1906 a monument with appropriate inscription was erected on the supposed site of this celebration by the Pierce County, Washington, Pioneer Association.

On the 27th of July Wilkes, who was then engaged in surveying some of the islands in the Straits of Juan de Fuca, received dispatches telling of the wreck of the *Peacock* on the bar of the Columbia River on July 18th. Since leaving the Hawaiian Islands in December 1840, the *Peacock* and *Flying Fish* had visited the Phoenix, Samoan, Ellice, Kingsmill and Union Islands. A previously uncharted island in the Ellice group was named Hudson Island, and one in the Union group was named Bowditch Island after the well known authority on navigation. The *Peacock* had made Cape Disappointment on July 17th, but the next day ran aground in attempting to cross the Columbia bar, where she was subjected to the furious pounding of the breakers. All efforts to get her off were unavailing, and with great difficulty all the crew and officers were safely brought to shore, although it looked for a time as if the ship must surely go to pieces before Captain Hudson and his companions were able to leave.

[4] Journal number 502, Hydrographic Office, U. S. Navy Department.

This disaster forced the curtailment of the surveying activities, so that they were not extended as far north in the Straits of Juan de Fuca as had been intended.

Not wishing to risk the *Vincennes* on the Columbia bar, Wilkes shifted his pennant to the smaller *Porpoise*, and sent Ringgold with the *Vincennes* on to San Francisco to survey San Francisco Bay and the Sacramento River. A brig, the *Thomas H. Perkins*, was purchased and after being refitted and renamed the *Oregon*, was added to the squadron and placed under the command of Lieutenant Overton Carr.

Wilkes in the *Porpoise* assisted by the *Flying Fish* and the *Oregon* proceeded in August to survey the Columbia and Willamette Rivers. This duty occupied him until October, when the *Porpoise, Oregon* and *Flying Fish* left Astoria to join the *Vincennes* at San Francisco. Ringgold had in the meantime surveyed the northern part of San Francisco Bay, the Sacramento River beyond the mouth of the Feather River, San Pablo Bay, and part of the San Joachim River. An overland party under the leadership of Lieutenant George F. Emmons made the journey from the Columbia to California by land.

By October 28th, all of the exploring parties had returned and the observatory and surveying duties had been completed, and on November 5, 1841, the squadron weighed anchor for the homeward passage. As they sailed westward across the Pacific they continued to look for uncertain islands and shoals. Wake's Island, now one of the Pacific outposts of the United States, is one which they visited and surveyed. Manilla was reached on January 13, 1842, and a week's stop was made there while parties made excursions into the interior and climbed two of the mountains.

The surveying duties were not yet over, for Wilkes had been ordered to examine the Sulu Sea in the hope of ascertaining a safe route through it which would shorten the passage of American vessels to and from China. The existing charts of this region were found to be very inaccurate and misleading.

At Singapore the *Flying Fish* was examined and found to require such extensive repairs that it was thought best to sell her. The three remaining vessels were soon headed for home, and the *Vincennes* reached New York on June 10, 1842, after stopping briefly at Cape Town and St. Helena. The *Porpoise* and the

Oregon called at Rio de Janeiro and arrived in New York a little later than the *Vincennes.*

It is not easy to evaluate the results of the surveys made by the expedition. About 280 islands were surveyed, as well as some 800 miles of coast and inland waters of Oregon Territory and 1500 miles along the Antarctic Continent. The published atlas on Hydrography lists 106 charts, but this probably does not represent the whole number made, as Wilkes wrote in 1842, "We now have one hundred and eighty charts nearly ready for the engraver; it is probable that we shall have as many more when all the islands, harbors, shoals, and reefs are plotted, that have been examined and surveyed." [5]

There is abundant testimony that the charts were highly esteemed. A review published in 1846 of the scientific results of the expedetion alludes to the charts as having been pronounced by competent judges to be of unrivalled excellence,[6] and according to another review, the secretary of the navy had declared that the charts of California and the coast of Oregon Territory alone were worth the entire cost of the expedition.[7]

James Dwight Dana, Geologist of the expedition, whose opinion should carry some weight, wrote to Asa Gray as follows: "If you examine Wilkes's charts, you will find them well done. They are the surveys of his officers (as well as himself) and among them were some excellent surveyors. The Feejee chart is very far superior to the French one by D'Urville, made after their late voyage, a rival of our expedition. . . . I mention these particulars, because, whatever may be said of him [Wilkes] and the Narrative, the hydrographical department has been well carried out. Wilkes, although overbearing with his officers, and conceited, exhibited through the whole cruise a wonderful degree of energy, and was bold even to rashness in many of his explorations. . . . I much doubt if with any commander that could have been selected we should have fared better, or lived together more harmoniously, and I am confident that the Navy does not contain a more daring explorer, or driving officer." [8]

[5] Charles Wilkes, *Synopsis of the Cruise of the U. S. Exploring Expedition,* Washington, 1842, p. 41.

[6] *North American Review*, Boston, 1845, 63, p. 211.

[7] *North American Review,* Boston, 1843, 56, p. 266.

[8] Daniel C. Gilman, *The Life of James Dwight Dana,* New York, Harpers, 1899, p. 149.

It is an interesting coincidence that one of Wilkes' great grandsons, whose first duty after graduating from Annapolis was to be assistant navigation officer on a Pacific cruise, found that the charts he was using had been made by the Exploring Expedition eighty years before.[9] This incident, as well as the fact that many of these charts are still in use even today, a century after they were made, is certainly a positive tribute to their accuracy.

The Exploring Expedition came a little too late to make numerous or important discoveries of new islands in the Pacific. Its contribution to geographical knowledge lies rather in the accurate fixing for the first time of the positions of many of the islands in the groups visited, and in improved charts and descriptions of them. In this respect the work of the expedition is considered by geographers of today as of outstanding importance.[10]

The lack of enthusiasm on the part of Congress to appropriate from year to year the funds necessary to publish the important contributions of the expedition was naturally discouraging to Wilkes, yet he always felt confident that recognition would come eventually. This feeling he expressed in a letter to Captain Hudson written in March 1857, when he wrote: "Although the government has treated us most miserably in every respect as well as the Navy generally I am well assured the time will come when our exertions will be appreciated and dwelt upon by both with satisfaction and pride."[11]

9 Personal letter from Mr. J. Renwick Wilkes to Professor W. H. Hobbs.

10 S. Whittemore Boggs, "American Contributions to Geographical Knowledge of the Central Pacific," *The Geographical Review*, Vol. XXVIII, No. 2, p. 188, 1938.

11 Wilkes Papers, Library of Congress.

JAMES DWIGHT DANA'S STUDIES OF VOLCANOES AND OF CORAL ISLANDS

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(Read February 23, 1940, in Centenary Celebration of the Wilkes Exploring Expedition, 1838–1842)

THE name of James Dwight Dana stands preëminent in American geology. It is a relatively easy matter to write of the work and accomplishments of Dana. There are so many things about which to write. Geologist, mineralogist, chemist, and zoologist, few men have accomplished such distinguished results in so many fields of endeavor. Dana's chief love was Geology and many of the great principles on which this subject is based and which have influenced geological thought throughout the years originated with him. His success can be attributed to an active and brilliant mind, a boundless energy, and an inspiring personality. However, without detracting in the least from these qualities, Dana was also fortunate. At the beginning of his career he had an opportunity which is presented to but few men. He was invited to become naturalist for the United States Exploring Expedition and to spend three years in scientific study in many, at that time, little known regions of the world. Dana would have been a great man even if he had not accepted this invitation. His *System of Mineralogy,*[1] which at once made him a recognized authority in this subject, was published a year before the Expedition set sail, when he was only 24 years of age. Nevertheless, the fact remains that most of the great ideas and principles which he later expounded sprang from the experiences of this unparalleled opportunity. His genius was inspired by and his ability was directed by the events of those three all-important years. In the nearly sixty years of productive work which followed, the great majority of his over 200 published writings exude the aroma of the salt seas and distant lands he visited. Thus Dana, like Darwin, his famous contemporary, received his start by an ocean voyage.

Of Dana's numerous accomplishments on the expedition, two

stand out above the others, first his work on coral reefs and corals, and second, his observations on volcanoes. It is the object of this paper to discuss briefly Dana's studies in these two fields.

Work on Coral Reefs

Breadth of Dana's Experience.—Dana spent parts of three years travelling among coral reefs and islands in the Pacific, during which the reefs of Tahiti, Samoa, Hawaii, and Fiji were examined with care and fifteen other coral islands visited, seven of these in the Tuomotu Archipelago, one in the Friendly Group, two in the Gilbert Group, and five others near the equator east of the Gilbert Group. Few if any students of corals either before or since Dana's time have been granted so rich an experience.

In August 1839, when he was 26 years old, Dana saw his first low coral island. This was one of the easternmost islands of the Tuomotu Group. Here he began his study of corals and coral reefs which were destined to have such a far reaching effect on geologic thought. After an examination of many islands in this group the expedition set sail for Tahiti where three weeks were spent in surveying the island and its reefs. Samoa with its luxuriant coral growth was next in line. Dana had the opportunity of spending a month in the study of these reefs before the expedition moved to New South Wales. During the three months between his arrival in the Tuomotus and his departure from Samoa on November 10th, he had examined intimately excellent examples of atolls, barrier reefs, and innumerable fringing reefs. As regards the origin of barriers and atolls he apparently had not as yet formulated a definite hypothesis. Darwin's theory of subsidence was at this time unknown to him. When he reached Sydney he heard of it for the first time. It seemed to him to fit in admirably with what he had observed.

The Subsidence Theory as Proposed by Darwin.—Darwin—Dana—Davis, the three D's. The subsidence theory of coral barrier reefs and atolls will always be associated with these three men. Darwin, the originator, Dana, the strengthener, and Davis, the skillful champion. It is a curious fact that all three were born on February 12th, Darwin in 1809, Dana four years

later, and Davis in 1850. In order to understand Dana's important contributions to the theory, it is essential to follow the line of thought which led Darwin to originate it.

Briefly the subsidence theory is as follows. A fringing reef bordering a continent or island, which is subjected to subsidence, not more rapid than the upward growth of the reef, will eventually turn into a barrier reef since corals grow better on the outer margin of the reef where ecological conditions are most favorable. In this case the outer edge of the reef will keep pace with the rising sea level as the land subsides, whereas the landward part will become submerged and turn into a lagoon. In cases where the barrier reef surrounds an island the reef will become an atoll as subsidence proceeds and the island is completely submerged beneath the water.

The fundamental postulate then of the theory is slow subsidence of the land mass on which the reef rests. Darwin arrived at his theory from the recognition of two important contentions: first, that the reef building corals can flourish only at limited depths below sea level, and second, that throughout vast areas of the Pacific and Indian oceans coral reefs and coral islets do not rise to greater heights above sea level than can be obtained by wave and wind action working on loose materials. The first contention leads to the conclusion that the active, growing part of the reefs, at least as far as they are affected by corals, is of limited depth (120–180 feet according to Darwin). This leads to the question of the foundations on which the corals rest. Darwin quickly discarded the idea that the foundations were immense piles of sediment because of the remoteness of the regions from large land areas which could give rise to these sediments. He [2] concluded, therefore, that the reefs must rest on foundations of a "rocky" nature. Two possibilities now presented themselves. The first was that each coral atoll rested on a mountain summit which reached the height of 120 to 180 feet below the surface of the sea. He considered this as most improbable because of the vast extent of territory occupied by atolls and low coral islets. He reasoned that mountain summits beneath the sea should be similar to those on the land and should rise to different heights, some of which certainly would project high above the sea. This brought him to the second and more plausible conclusion, that volcanic islands surrounded by fring-

ing reefs once occupied the atoll and low coral island areas and that slow subsidence occurring over these regions and accompanied by upward coral growth resulted in the conversion of the fringing reefs into barriers and finally atolls.

Having reached this conclusion, Darwin hunted for some independent confirmation, particularly as regards subsidence. He realized that without this the theory stood on rather weak ground. He[3] said, "But, although subsidence may explain a phenomenon otherwise inexplicable, it may be asked, are there any direct proofs of a subsiding movement in the areas? This, however, can hardly be expected, for it must ever be most difficult, excepting in countries long civilized, to detect a movement the tendency of which is to conceal the parts affected."

The only independent verification he attempted was in connection with the geographic distribution of the various kinds of reefs. By gathering together all the then available information he plotted on a world map the location of the coral reefs and volcanic areas. He colored the barrier reefs and atolls two shades of blue and the fringing reefs and volcanic regions red. This led him to observe that the areas tinted blue were widely separated from the red areas and that there was very little mixing of the two colors. He concluded that fringing reefs were associated with volcanoes and that barrier reefs and atolls were in volcano-free regions. He associated volcanoes with rising areas with volcanic matter bursting forth every now and then. In proof of this contention he cited the finding of fossil corals and other sea organisms at some distance above sea level on volcanic islands such as Hawaii. He therefore concluded that fringing reefs were to be associated with rising areas or at least areas of still-stand. On the other hand, regions which were free of volcanoes, the barrier reef-atoll regions, he believed to be sinking. Although this attempt at verification brought out many interesting points it lacked strength because of the scanty information Darwin had at his disposal regarding the geologic history of the many islands he considered in his map.

Dana's Contribution to the Subsidence Theory.—It was Dana who contributed the independent confirmation of submergence so much needed to strengthen the theory. Although it is true, as Darwin had said, that it is difficult actually to detect subsidence in operation, still it remained for Dana to point out what

amounts to a direct proof of a sinking land area or a rising sea level. He reasoned that a subsiding island should have an irregular, embayed shoreline as the sea water fills in the subaerially eroded valleys.

Dana[4] reached this conclusion during his ascent of Mt. Aorai, one of the two high peaks of Tahiti. He realized that the gorge-like valleys leading from the high central mass of Tahiti to the sea were made by sub-aerial erosion. Also it dawned upon him what the consequences of a partial submergence of such an eroded land would be. He clearly saw that a subsidence of 500 feet would turn the "erosion-made" valleys into deep bays and that a sinking of over 1000 feet would make them like fiords. His own observations and a study of available maps of others quickly convinced Dana that islands behind barrier reefs were as a general rule distinctly embayed and thus strongly substantiated the subsidence theory.

As Davis has pointed out, it is remarkable that Darwin could have failed to have recognized this all important fact, that subsidence of a land area creates an irregular shore line. Apparently Darwin missed the point because he believed that the embayments were caused by the erosive action of sea waves. The fact that Dana was the first geologist to stress subaerial erosion of the valleys and their subsequent flooding by sea water to make an irregular coast as subsidence progressed, strikingly indicates the infantile condition of the subject of Physiography in those days.

It is interesting that Darwin apparently failed to see the significance and importance of Dana's contribution to his own theory of subsidence and made little mention of it in the second edition of his book on coral reefs. As[5] Davis has pointed out, in this respect he was not alone, as "no students of coral reefs recognized and employed Dana's principle of embayed shorelines in evidence of the subsidence of reef-encircled islands for many years after his announcement of it."

Other Contributions by Dana on Coral Reefs.—Although Darwin expressed satisfaction in Dana's general acceptance of his explanation of the origin of coral reefs, he took emphatic exception to a number of other closely related points brought out by Dana in his reports. Two of the more important of these were: (1) effect of oceanic temperature on the distribution of

coral reefs, and (2) Dana's discovery that many of the localities labeled by Darwin as regions of subsidence show signs of recent elevation.

As regards the first of these, Dana emphasized the causes which limited coral growth and the distribution of coral reefs. He pointed out the significant fact that corals cannot exist in a locality if the temperature becomes lower than the critical temperature during even a small part of the winter months, regardless of how high it becomes during the remainder of the year. Darwin minimized this factor and stated that it was necessary to look for other causes for the absence of coral reefs in certain regions, such as on the coasts of Peru and the Galapagos Islands. Dana[6] answered that this was cause enough and "where temperature excludes there is no use in discussing other unfavorable conditions."

In respect to the second point, Dana's more thorough and extensive studies enabled him to examine in more detail the geologic evidence of changes in sea level in many of the islands shown on Darwin's map. The result of this was that he could not agree with Darwin's generalization that barrier reef regions were subsiding and fringing reef areas were rising or at a still-stand. He recognized evidences of recent elevation in many of the regions which Darwin marked as subsiding. Dana recorded elevations from 1 or 2 feet to as much as 600 feet. Darwin failed to refer in his second edition to those islands which gave evidence of considerable elevation and referred only to those which Dana reported to have experienced an elevation of but a few feet, the low coral islands. Darwin believed that these islands merely took on the appearance of having been slightly elevated after a long period of stationary conditions, during which time wave and wind work heaped loose material above sea level. It is interesting that Dana recognized in these islands a shift of the strand line which Daly and others have since shown to be eustatic. Furthermore, some present day students of coral reefs, such as Gardiner, Crossland, and Spender, would even go so far as to say that without a negative shift of the strand line no structure such as coral-sand islands or shingle ramparts could possibly form. In other words, they confirm Dana's contention of a strand line shift as opposed to Darwin's idea that waves and wind work during long periods of still-stand conditions would produce these structures.

Work on Corals

Complexity of Coral Reef Problem.—The coral reef problem is a complex one, and the recognition of its complexity is essential to its solution. W. M. Davis [7] once wrote, "The origin of reefs is a geological one," and criticized biologists, particularly British biologists, for befogging the issues of the problem because they were not sufficiently equipped to handle the geological implications. He failed to mention the fact that the subsidence theory, which he so strongly defended, was originated by a biologist, and a British biologist at that. Davis' criticism is partly justified, but on the other hand, it is also true that many geologists have failed to consider important biological facts and have been equally as guilty of impeding the final solution of the problem as have the biologists. The fact of the matter is that the solution does not belong within the realm of any one subject but requires the attention of scientists of many fields. Dana was well fitted to tackle coral reefs. Although primarily a geologist, he was also a biologist.

He not only recognized the geological implications of reef formation but also realized that the final solution of the problem was linked with the characteristics and habits of the corals themselves and other reef forming organisms. While in the field he devoted much time to the living corals and made large collections for future study of the hard parts. On his return from the expedition in 1842 he began work on the preparation of a report on Zoophytes.

Report on Zoophytes.—Corals are universally recognized as being among the most difficult groups to classify systematically. The variation within the species is enormous. Dana soon found himself in difficulty. In writing to Asa Gray [8] he said, "Errors in description of species and in the laying down of genera were numerous in the books; many species were confounded under a single name, and the same name had been differently used by different authors. . . . I have consequently undertaken to reconstruct the science, revise, correct, and systematize the whole." This he did and the result, his *Report on Zoophytes,* a quarto volume of 741 pages with a folio atlas of 61 plates, published in 1846, represents a monumental and epoch-making work in this field. Like the classic volumes of James Hall on the Paleontology of New York, it remains today one of the chief source

works on systematic classification. The large divisions which he defined, the Actinoidea, now called the Anthozoa, and the Hydroidea, and the subdivisions of the former, the Actinaria and the Alcyonaria, are still accepted by zoologists. Furthermore, the majority of the over 200 species which he described, now housed in the United States National Museum, are still considered valid. It is interesting to note that the latest publication on corals, one based on a collection from an island group in the Dutch East Indies, lists 92 species of which 26, or nearly 30 per cent, were originally described by Dana, and still recognized as true species after 100 years.

Work on Volcanoes

Breadth of Dana's Experiences.—Next in importance to his work on corals and coral reefs were his observations on volcanoes. Dana's interest in volcanoes dates back to the beginning of his scientific career. The first paper he ever published was [9] "On the condition of Vesuvius in July, 1834," written in the form of a report to Professor Benjamin Silliman. Dana's experiences at Vesuvius not only aroused his interest in volcanoes but also made it possible for him to study much more intelligently the volcanism he later observed in the Pacific. This comparison between the two regions led to a number of important generalizations.

While on the expedition he visited volcanoes in Madeira and the Cape Verde Islands, the extinct volcanic regions of Tahiti, Samoa, and the Fiji Islands, various flows of New South Wales in Australia, active and extinct craters of the Hawaiian Islands, and extinct volcanic mounds in Oregon and California.

It was not until he reached Hawaii that his studies on volcanism began in earnest. The active Hawaiian volcanoes, Mauna Loa and Kilauea, have probably contributed as much information to the subject of volcanism as any others in the world. This is due partly to their accessibility during times of eruption. Dana was the first of a line of distinguished volcanologists to make detailed studies of their activity.

His observations were made in Hawaii, while on the Wilkes Exploring Expedition, during October and November of 1840, and ten days in November of the following year. The results

are included in the report on the Geology of the expedition and include pages 155–284 and 353–456, a geologic map of Oahu, and many text figures. Years later, in 1887, when Dana was 74 years old, he had the privilege of revisiting the scenes of his early endeavors in Hawaii. Shortly after this visit, in 1891, his book [10] *Characteristics of Volcanoes* appeared, which set forth his general views on the subject after a life-long study, but based primarily on his observations of the two active Hawaiian volcanoes.

Importance of Detailed Observations.—Probably Dana's chief contribution to the subject of volcanism was his detailed and systematic account of the progressive changes that accompany activity of the two Hawaiian volcanoes, Kilauea and Mauna Loa. He painstakingly gathered together all the obtainable information regarding the condition of the volcanoes previous to his visit, and throughout the remainder of his life kept a record of subsequent changes. Special attention was paid to all topographic changes such as shifts in the level of the crater floor, avalanches, and faulting. From this information he attempted to arrive at some conclusions regarding periodicity of activity. Although not too successful here, the fact remains that he recognized that this was the most promising approach to the understanding of the fundamental factors of volcanic activity. He thus established the basis of the brilliant detailed survey work later carried on by Jaggar and his associates, which has led to such promising results in the field of predicting the time of future eruptions.

Linked with this contribution was his recognition that the Hawaiian volcanoes went through a cycle of three changes [11]: "(1) a rising in level of the liquid lavas and of the bottom of the crater; (2) a discharge of the accumulated lavas down to some level in the conduit determined by the outbreak; (3) a down-plunge of more or less of the floor of the region undermined by the discharge." This filling, discharge, and collapse he believed to be common to the life of both Kilauea and Mauna Loa and in its essential aspects even to Vesuvius.

Origin of Volcanic Cones.—Dana contributed considerably to a clear understanding of the origin of volcanic cones. Von Buch, the distinguished German geologist, had years before announced his theory of Craters of Elevation. He held that volcanic cones

were formed by extrusion of molten material from below as a dome-shaped mass pushed upward by hot steam. If this mass cooled and solified it retained its dome-like shape and had no crater. If, however, the steam burst through at the summit of the dome the material would fall inward toward the center and form a typical crater. Dana, with Lyell and Scrope, combated this theory and showed that cones were produced by the accumulation of ejected material from successive eruptions. Also Dana stressed the causes of differences between the shapes of volcanic mountains. He recognized in Mauna Loa and Kilauea an entirely different kind of volcano from Vesuvius. Instead of steep sides their cones were low angled. Instead of violent eruptions activity generally resulted in quiet outwelling of lava. He argued that the liquid lavas of Hawaii and the more viscous lavas of Vesuvius were primarily responsible for these differences.

Causes of Volcanic Eruption.—Dana gave considerable attention to the problem of the immediate cause of volcanic eruption. He believed that inflation by heat of vaporizable substances, such as water and sulphur, resulted in the ascent of the lavas and their subsequent overflow. Thus he followed Scrope in assigning a major role to the expansion of water vapor in propelling magma to the surface. He believed that this water was chiefly of meteoric origin, and that it gained access to the hot rocks about the lava column either directly from the sea or from ground water descending deeply into the volcanic mountain. He agreed with other investigators that there was a definite relation between the amount of rainfall and the time of eruption. He [12] pointed out that in the case of Mauna Loa 13 of the 19 recorded eruptions occurred during the wetter season and concluded "it is probable that if there is any periodicity in eruptions it is more or less dependent on meteorological cycles." Although it is believed that many volcanic explosions are caused by the presence of much water in a magma, few geologists today would agree with Dana that the chief source of the water is meteoric.

Dana and Phreatic Eruptions.—Dana was the first geologist to recognize what he termed *semi-volcanic* explosive eruptions, which are now known as *phreatic* eruptions. In these explosions only old rock fragments are extruded and no liquid lavas are directly concerned. He attributed them to large volumes of

water gaining sudden access to rocks which, although hot, are not heated to the fusion point. This type of volcanic eruption is well established and in the main Dana's explanation is still accepted.

Denudation of Volcanic Cones.—Dana was the only member of the Expedition to ascend Mt. Aorai, near the center of Tahiti. This peak stands 6773 feet above sea level and is the second highest point on the island. This excursion, a difficult one even at the present time, resulted in one of the earliest detailed descriptions of the erosion of a volcanic cone. He clearly showed that Tahiti was once a smooth surfaced volcanic dome like Mauna Loa and that its rough interior, with steep sided valleys and knife-edge ridges, is largely the result of stream erosion. This observation strengthened considerably the general principle of the importance of stream erosion on any land mass, a principle which at that time was only partially understood.

Major Problems Derived from the Study of Reefs and Volcanoes.—This paper is limited to a description of some of the most important phases of Dana's work on volcanoes and coral reefs. Dana, however, was not content to let his own work stop here. His observations in these two fields were merely the stepping stones which led to the study of larger problems concerning the origin and evolution of the earth.

His coral reef studies caused him to believe that a large part of the Pacific area had undergone subsidence. This brought to his attention the problem of the origin of ocean basins and continents. He believed the earth to be a cooling globe, the continental regions to have been the first to solidify, and the ocean areas the last. Since the latter were, according to him, at one time the most intensely heated areas of the earth, they had consequently undergone the greatest amount of contraction while cooling. This contraction caused a continued deepening of their basins and at the same time caused the continents to stand out in relief above them. Also the contraction accounted for the formation of mountains along the borders of the continents. He thus reasoned further that the continents and ocean areas have always been in approximately their same locations. This theory of the permanency of the continents and ocean basins, which has influenced geologic thought so tremendously for nearly one hundred years, was first clearly presented in his volume on the Geology of the Wilkes Exploring Expedition.

The investment of the United States Government in this expedition has been repaid many times over by the accomplishments achieved. New lands were discovered, little known regions surveyed, a tremendous mass of valuable information compiled, and great principles expounded. Dana's contributions rank high in those from a list of an illustrious personnel. The influence on scientific thought which he exerted through his studies cannot be calculated. In the words of Henry Shaler Williams:[13]

> In the breadth and richness of his knowledge an equal to Professor Dana is not likely to arise. For the thoroughness and industry which he applied to all his investigations, the fairness with which he treated all with whom he could not agree, the kindness and consideration he showed to all, and the unswerving devotion to the truth, James Dwight Dana will be long remembered by all students of science. His geological contributions to American geology constitute such a fundamental part of our knowledge that so long as the science endures he cannot be forgotten.

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THE FISHES OBTAINED BY THE WILKES EXPEDITION, 1838–1842

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(Read February 23, 1940, in Centenary Celebration of the Wilkes Exploring Expedition, 1838–1842)

THE United States Exploring Expedition was the first great American venture of its kind. The initiation of such an effort called for daring and labor in many fields, and from the evidence which has reached down to our time many of the obstacles must have then appeared unsurmountable. Dissention and controversy were also at hand with their usual devastating role. One would hardly feel in our day that a voyage of this magnitude, with all its elaborate preparation and equipment and backed by government support, could be solicited for such ideal purposes as the ingenious and intrepid Wilkes devised. Though unable to do justice to the glories of this truly great undertaking, we may ever cherish admiration for the gallant commander and his men for the luster they have added to our nation and flag. Their great work of exploration was, of course not among the least of the many achievements, as the numerous volumes relating to the expedition attest.

In the brief time allotted it is only my purpose to deal with a summary of the results made from a study of the collection of fishes. Just how extensive the collection was originally I do not know. The Exploring Expedition fishes in the United States National Museum number 1128 and are represented by over 500 species. This is most certainly a large collection today, as measured by any standards. At the time of its consummation, or during that early part of the nineteenth century, comparatively few voyages of scientific exploration in the vast stretches of the unknown Pacific and the southern hemisphere had been completed when Wilkes embarked. These are Freycinet's voyage of the "Uranie" reported by Quoy and Gaimard in 1824–1825; Duperrey's voyage of the "Coquille" reported by Lesson in

1830–1831; the voyage of the "Astrolabe" by Dumont d'Urville in 1834, also reported by Quoy and Gaimard. The voyage of "La Bonite" was reported by Eydoux and Souleyet in 1841, and the voyage of the "Beagle" by the Rev. Leonard Jenyns in 1842. If one glances at the results of these various expeditions it is very evident that their accounts all fall very far short in number and variety of species in comparison with the collections and work of the United States Exploring Expedition. It was not until the material from the voyage of the "Challenger" was reported by Günther that the Exploring Expedition results were equalled. This eventuated due to discovery of bathypelagic forms and the numerous captures with special equipment. During this same period the United States Steamers "Abatross," "Blake" and "Fish Hawk" accumulated equally great collections. Günther's report in 1880 of the "Challenger" shore-fishes with 1400 specimens identified as 520 species is equalled by the Exploring Expedition collection of forty years previous.

The localities which are still preserved along with the fishes may be briefly considered. In the Atlantic they are the Cape Verde Islands, Rio Janeiro, northern Patagonia, Orange Harbor in Tierra del Fuego and the Cape of Good Hope. Of this group the Rio Janeiro material is the most extensive, and more complete than Agassiz's report of the Spix collection in 1829. Most of the Orange Harbor materials have remained unique until quite recently. Along the western coast of South America collections were made in Chili, especially at Valparaiso, and Peru, some at Callao, and like the Patagonian and Tierra del Fuego material equally extensive as the voyage of the "Beagle" specimens. By far the most complete collection was made in the Hawaiian Islands, and long remained so or until shortly after the annexation by the United States, the development of the B. P. Bishop Museum in Honolulu, and the United States Bureau of Fisheries investigations. Another important lot of material was secured in the Fiji group and this remains even to the present day unsurpassed. Other localities in the tropical Pacific are Samoa, Tuamotu, Tahiti, Tongatabu, Raraka, the Phœnix coral group, Mangi, Manda and Wake Island. The Expedition visited the western coast of the United States at Puget Sound, the Columbia River and San Francisco. Interesting materials were secured in Australia, the only locality appearing to be Sydney,

New Zealand, Singapore, Balabac Passage and Manila. Several of the specimens are also credited to Colombo, Ceylon.

The fascinating five volumes of the Narrative of the Expedition by Wilkes are still interesting reading. Many of the chapters are virtual storehouses of the most valuable information, revealing the writer with fluent style. I have been led to note a few of the more interesting items he sets forth. Throughout these books abound with numerous vignettes and woodcuts. Many of these have now become classic, some representing all that is left to us of changed places and vanishing peoples.

The United States Exploring Expedition departed on August 17, 1838, passing beyond Cape Henry the next day. During the novelty of the first days out Wilkes says "it was amusing to see all entering into the novel occupation of dissecting the fish taken, and to hear scientific names bandied about between Jack and his shipmates". On September 6 a large cotton-wood trunk was met with and "Great quantities of fish were about it, consisting of dolphins, sharks, etc. We did not, however, succeed in taking any". At Madeira "sea fish are abundant; but not a single trace of a fresh water fish was seen or found in the streams". At Porto Praya, Cape Verde Islands "the seine was drawn for fish in one of the coves to the eastward of the anchorage, in what we understood was a place well adapted for the purpose, but it did not prove so. I should prefer the western beach, as offering better luck and being more advantageous. . . . The houses are of stone, one story high, partly thatched, and others tiled. Their interior presents only a few articles of absolute necessity. Of comfort and cleanliness, in our sense of the words, they have no idea. The houses and streets are filthy in the extreme, and in both of them, pigs, fowls, and monkeys, appear to claim, and really possess, equal rights with the occupants and owner".

At San Lorenzo, Peru, he says "the waters abound with excellent fish" and near Casa Concha "in the stream that flowed near it, were fish of from six to eight inches in length, but none of these were taken". In the Paumotu Group "the various snakes, the many-coloured fish, the great eels, enormous and voracious sharks, shells, large molluscs, spiders, with the curious lepidoptera, seemed to have quiet possession, their webs stretching in every direction, and occasioning us much annoyance: all gave a novelty to the scene, that highly interested and delighted

us''. At the entrance to the lagoon of Raraka ''the way of catching fish here is quite amusing, and to it we owe the many specimens in that department of natural history which we obtained. The natives enjoy the sport amazingly, and both old and young are all in some way participators in it. Near the mouth of the lagoon are laid some coral stones, forming a rude and shallow pen, with a channel leading to it; several natives proceed about one-third of a mile up the beach of the lagoon, where they enter the water, ranging themselves in a row, the tallest in the deepest water. They then move along down towards the pen, quite noiselessly at first, driving the fish before them. As they approach they begin to splash and make a noise; the clamour gradually increases, until it becomes one continued shout. They then contract themselves towards the pen, and the fish are seen jumping and dashing in all directions, as if very much alarmed, until they are forced to enter the pen, which is then closed with a few stones; afterwards the natives begin to spear them with great dexterity, and many were obtained. It was gratifying to witness the pleasure that both old and young appeared to take in this employment, and quite surprising that the fish do not escape over the low wall that surrounds them, only two or three inches above the water; but they appear bewildered''.

He says of Tahiti ''the small streams flowing through luxuriant woods, add much to its beauty; these run bubbling along to the sea, passing many cool and pleasant places; their entrances are usually closed up by the natives, for the purpose of taking fish, a sort of dam being constructed, over which the water flows and the natives, standing on the outside up to their waists in water, are often seen taking the fish in baskets''.

At Sapapale on Savaii Island, an interesting fishing was observed. ''The net was a kind of cheval-de-frise, made of the leaves of the cocoanut-tree, split and wound round a line, and was little less than half a mile in length. It was more formidable in appearance than in reality. This net was taken out at high water to the coral reef, in three pieces, then fastened together, and thus made to enclose a large extent of water. This space was gradually contracted by doubling up the net, which answered the same purpose as the drawing of a seine. The fish did not attempt to pass it, and were thus driven towards a certain point,

where a sort of sack of matting had been placed for them to enter. As the fish were gradually enclosed by the mat, and the tide fell, the scene became an animated one. Men, women and boys, to the number of two or three hundred, were eagerly engaged in picking up or catching the stragglers as they were seen leaping up; the whole area seemed alive with fish, jumping in every direction, some over the heads of the natives and thus escaping, while others leaped into hand-nets. About a canoe load was caught comprising thirty different kinds of fish, some of which were six or eight pounds in weight, but the majority were smaller''. Later, in describing the war-like habits of the savages of western Upolu, he notes ''their spears were pointed with the sting of the ray-fish, which, on breaking off in the body, caused certain death''.

Of Lake Illawarra, on the coast close to the sea-beach below Sydney, it ''contains a great quantity of fish, principally mullet,'' which were caught in large quantities, salted and dried. The fishermen assured them there were thirteen kinds of fish in the lake. At Lake Macquarie ''as a part of the lake was said to be fordable, it was determined to take advantage of it, in order to shorten the route. One mounted the horse to pass over. Whilst they were proceeding quietly along, the horse suddenly reared and plunged, relieving himself of his rider and load, which were thrown into the water two feet deep, without any further injury than a good ducking, and the disparagement of the wardrobe. It was found that the horse had trodden upon a sting-ray, which fully accounted for his sudden gambols''. At the Auckland Islands ''the rocks are covered with limpets, and small fish of many varieties are caught in quantities among the kelp''.

At Muthuata in Fiji ''the party witnessed some natives who were employed in taking fish, near the mouth of a small stream, by poisoning the water with the stems and leaves of a climbing Glycine, which grows abundantly near the coast''. At Ovalu ''sailing along the north side of the island, we passed many fish-wiers formed of reeds, into which the fish are sometimes driven. At other times the fish are lured by food into these traps at high water; the wier is then closed, and the fish taken at low water. The women use the hand-net, which is thrown over the school. They have large seines for turtles, as well as smaller ones, both of which resemble our own, the weights being small bits of coral,

while for floats they use the seed of the Barringtonia. These nets are all well made. They likewise make pens of stones into which they drive the fish, and capture them either by spearing or when the water runs out at low tide. It is also a custom with them to dam up small streams, and stupify the fish with Glycine. Hand-nets are sometimes used in a peculiar manner, thus: when they see a large fish take refuge in the coral shelf, they surround the place with a net and drive the fish out into it''. Wilkes describes an adventurer who went to Australia in a convict ship, by name of Paddy Connel, whom he met in Fiji living with the natives and said ''that his residence on Ambatiki was a forced one, and it was as though he was living out of the world, rearing pigs, fowls and children. Of the last description of live stock he had forty-eight, and hoped that he might live to see fifty born to him. He had had one hundred wives''.

Extensive fish-ponds were seen on Kauai, Hawaiian Islands. ''They are of different degrees of saltness. The fish are taken from the sea when young and put into the saltest pond; as they grow larger, they are removed into one less salt, and are finally fattened in fresh water.'' He also says ''in the neighborhood of Honolulu, there are a number of fish ponds belonging to the king, in which are bred several kinds of fish. There are many other ponds belonging to individuals. The taro-patches are used occasionally for this purpose, and not infrequently are seen to contain large fish. . . . They have several modes of taking fish, with the net and hook, and sometimes with poisonous herbs. They likewise take shrimps and small fish by forming a sort of pen in the soft mud, in one corner of which a net is placed; the shrimps and fish leap over the enclosure of the pen, which is gradually contracted towards the net, which acts like a large seine''. At Maui ''Dr. Pickering remarks, that the natives appear to be much better acquainted with the fish of their waters, than are the inhabitants of any civilized port we visited. A number of new species were obtained; for which I refer to the report on the ichthyology of cruise''.

At San Pablo Island in the Paumoto Group, two large fish, evidently scaroids, were received from the natives, ''the smaller of which measured five feet two inches in length, and its greatest circumference was four feet four inches. These proved to be excellent food. They were remarkable for their splendid colour,

the great size of the canine teeth in each jaw, and a large protuberance over the eyes; the head was without scales, the body being covered with large circular plates, over which the epidermis was very thick and of a rich blue colour, with regular concentric stripes of yellowish white; the fins and tails were striped with straight lines of alternate blue and yellow; the lips were fleshy, and the jaws strong and bony''.

At the salmon-fishery of the Nisqually Indians, on the Chickelis River, he describes ''hanging around their lodges were hundreds of lamprey eels, from a foot to eighteen inches long, and about an inch in diameter. We were told that these fish are caught in great quantities and dried for food; they are also used for candles or torches; for, being very full of oil, they burn brightly''. He says of the Willamette ''at the time of our visit to the falls the salmon-fishery was at its height, and was to us a novel as well as an amusing scene. The salmon leap the fall; and it would be inconceivable, if not actually witnessed, how they can force themselves up, and after a leap of from ten to twelve feet, retain strength enough to stem the force of the water above. About one in ten of those who jumped, would succeed in getting by. They are seen to dart out of the foam beneath and reach about two-thirds of the height, at a single bound: those that thus passed the apex of the running water, succeed; but all that fell short, were thrown back again into the foam. I never saw so many fish collected together before; and the Indians are constantly employed in taking them. They rig out two stout poles, long enough to project over the foaming cauldron, and secure their larger ends to the rocks. On the outer end they make a platform for the fisherman to stand on, who is perched on it with a pole thirty feet long in hand, to which the net is fastened by a loop four feet in diameter: the net is made to slide in the hoop, so as to close its mouth when the fish is taken. The mode of using the net is peculiar: they throw it into the foam as far up the stream as they can reach, and it being then quickly carried down, the fish who are running up in a contrary direction are caught. Sometimes twenty large fish are taken by a single person in an hour; and it is only surprising that twice as many should not be caught. The river at the falls is three hundred and fifty yards wide, and its greatest fall twenty-five feet. When the water is not very high, the rapids begin some distance

above the falls. Some of the Indians are in the habit of coming down in canoes to the brink of the falls, where they secure themselves by thrusting down poles in the crevices of the rock. There they take many fish, that have succeeded in passing the lower fall, with a hook fastened to the end of a pole. These are esteemed to be of the best flavour, as they are the strongest and fattest. It is said from these places the fish can be seen very distinctly passing up, and are taken very rapidly; but few Indians are willing or expose themselves to the risk of fishing there. The number of Indians at the Willamette Falls during the fishing season is about seventy, including all ages and sexes: there are others who visit the falls in canoes for fish, which at times will raise the number to not far from one hundred. Those fish which are unable to get up, remain some time at the falls, very much exhausted, and finally resort to the smaller streams below." Further on he says it "may be classed as one of the great sources of wealth, for it affords a large amount of food at a very low price, and of the very best quality: it does not extend above the falls. I found it impossible to obtain any data to found a calculation of the quantity taken, but it cannot be short of eight hundred barrels; and this after the Indian manner of catching them, as before described. The finest of the salmon are those caught nearest the sea. The settlers and Indians told us that the salmon as they pass up the river become poorer, and when they reach the tributaries of the upper Columbia, they are exceedingly exhausted and have their bodies and heads much disfigured and cut, and their tails and fins worn out by contact with the rocks. Many of the salmon in consequence die: these the Indians are in the habit of drying for food, by hanging them on the limbs of trees. This is to preserve them from the wolves, and to be used in time of need, when they are devoured, though rotten and full of maggots. The fish of the upper waters are said to be hardly edible, and, compared with those caught at the mouth of the Columbia, are totally different in flavor. The latter are the richest and most delicious fish I ever recollect to have tasted: if anything, they were too fat to eat, and one can perceive a difference even in those taken at the Willamette Falls, which, however, are the best kind for salting. There are four different kinds of salmon, which frequent this river in different months: the latest appears in October, and is the only kind that

frequents the Cowlitz river. The finest sort is a dark silvery fish, of large size, three or four feet long, and weighing forty to fifty pounds. There is one point which seems to be still in doubt, namely, where the spawn of this fish is deposited. It is asserted and generally believed, that none of the old fish ever return to the sea again. It has not been ascertained whether the young fry go to the ocean; and, if they do so, whether as spawn or young fish''. He then adds ''Mr. Drayton, during the time he remained at the falls, procured a beautiful specimen of a small-sized sucker, which the Indians caught in their nets, and of which he made a drawing. The lamprey eels were also a source of curiosity: they seemed to increase in numbers, crawling up by suction an inch at a time. At these eels the boy who accompanied Mr. Drayton took pleasure in throwing stones, which excited the wrath of the Indians, as they said they should catch no more fish if he continued his sport''.

In an interesting account of the Drummond Island warriors, of the Kingsmill Group, he says ''however singular the body-dress is, that of the head is still more so: it consists of the skin of the porcupine-fish, cut open at the head, and stretched sufficiently large to admit the head of a man. It is perfectly round, with the tail sticking upwards, and the two fins acting as a covering and guard for the ears: its colour is perfectly white, and by its toughness and spines affords protection against the native weapons''. Their weapons are also barbed with shark's teeth. ''Great numbers of fish are also taken in weirs, or enclosures of stone, which are made in the extensive coral flats, that are left bare by every tide: with these the fish are driven at high water, by a number of natives, who surround the shoal; the weir is then closed, and left until the tide falls, when the fish are easily taken in scoop-nets. Large seines are often used in places where the bottom renders it practicable to draw them. Flying-fish are taken in the daytime, by a trailing hook, attached to a short line, from the stern of a canoe. At night they are caught in scoop-nets as they fly toward a lighted torch, held in a part of the canoe.''

Seldom has a collection of fishes of such extent and importance as that gathered together by the United States Exploring Expedition been denied publication for so long a time. It has not entirely rested in seclusion during the entire past century.

The elder Agassiz had it in contemplation for some years, and during that time hundreds of drawings were made from the various specimens under his direction. However so far as known few if any of these were ever published. Strange must be the circumstances of such a collection not engaging the attention of the ichthyologists of the time. Of the various Exploring Expedition reports Girard wrote that on the herpetology, and other distinguished naturalists as Cassin, Breckenridge, Dana and Gould produced other massive volumes.

In 1920 the United States National Museum sent the entire collection of fishes belonging to the Exploring Expedition to Philadelphia with the request that I prepare a report on it, and so during that and the following year I studied the materials in the Academy of Natural Sciences. The great collection of drawings was also later sent to me and identified. Though the collections in other branches of natural history have suffered more or less from the ravages of time, it is gratifying that many of these old specimens have reached into our day in a comparatively fair condition of preservation in alcohol, perishable as they are. Some appear to have suffered more through the vicissitudes of wear and tear, though still persisting with broken bodies, abraided or worn fins, fallen scales, etc. Likely this may have been due to their condition when captured. From the figures in detail, made later, it would appear that only a portion of the materials are now extant. Possibly numbers of specimens were originally obtained of a given species and only a selection or a few of the apparent desiderata preserved. Less fortunate have been the labels and data accompanying them, as many were apparently lost or in other cases mixed, so that their origin is now conjecture. On the other hand many have been rectified through the data gleaned from the stack of original drawings and sketches, along with many field numbers.

Wilkes several times refers to an account or report on the fishes obtained by the expedition though a century has now passed and no such work has ever appeared. At the present time the extensive manuscript report on this collection I prepared in 1921 is still in the United States National Museum, reposing there apparently for lack of funds. With permission from the United States National Museum, therefore belated and curtailed as the present summary must be it is offered simply

as introductory. For a more detailed comparison the species marked with the prefixed asterisk were all unknown or undescribed at the time of the return of the expedition. They number 195 or a little over one-third of the total number listed (588).

Eptatretidæ

Eptatretus dombeyi (Shaw). Fig. 1. Drawing by Richard of example from Napon Bay, Valparaiso, Chili, January 28, 1839.

Petromyzonidæ

Entosphenus tridentatus (Richardson). Two from the Columbia River.

Eulamiidæ

Mustelus mustelus (Linnæus). Colored figure, without data.

**Mustelus antarcticus* Günther. Fig. 2. Colored figure, no data.

Scoliodon palasorrah (Cuvier). Fig. 3. Colored figure, without data.

Eulamia commersonii (Blainville). Three colored figures, without data.

Eulamia limbata (Müller and Henle). Colored figure, no data.

Glyphis glaucus (Linnæus). Two figures, one colored, without data.

Myliobatidæ

Aetobatus narinari (Euphrasen). Three figures, colored, without data.

Elopidæ

Elops saurus Linnæus. One 735 mm from Hilo, Hawaiian Islands, June 1841.

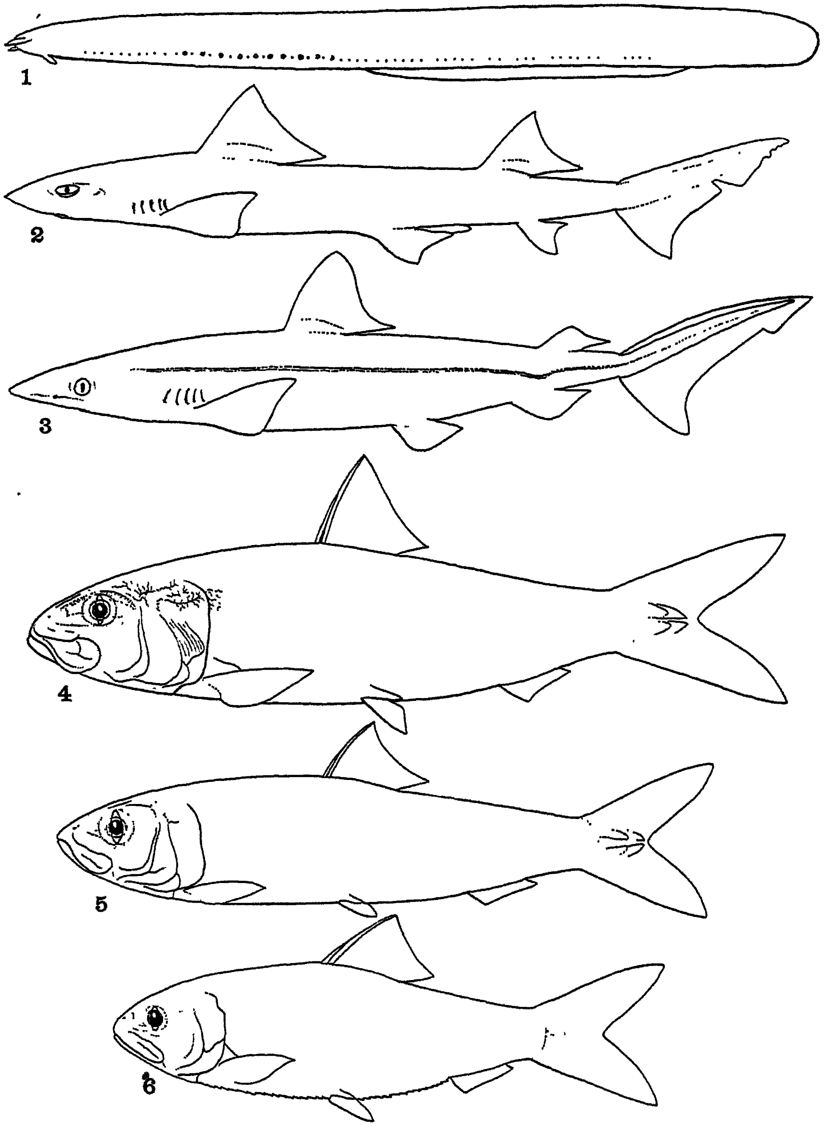
Albulidæ

Albula vulpes (Linnæus). Painting by Drayton 160 mm long of example from Hilo Bay, January 20, 1841.

Chanidæ

Chanos chanos (Forskål). Two, 120 and 160 mm from Fiji and Oahu. According to Drayton's painting "caught with the seine in Hilo Bay, June 18th, length 2 feet 6 inches".

H. W. F. del.



1. *Eptatretus dombeyi* (Shaw).
2. *Mustelus antarcticus* Günther.
3. *Scoliodon palasorrah* (Cuvier).
4. *Arengus sagax* (Jenyns).
5. *Sardinella allecia* (Rafinesque).
6. *Harengula peruana* Fowler and Bean.

DUSSUMERIIDÆ

**Etrumeus micropus* (Schlegel). Two from Oahu, 150 to 180 mm. Drayton's painting 663 mm long says "caught by the natives with a drift net with exceedingly fine meshes. Jan. 20, 1841. Hilo Bay, Hawaii. Native name Omaka. Drift net 120 feet long, some only 50 feet. The fish are enticed into the net by baiting with small fish by one man in a canoe, while two men in a larger one haul in the net".

Stolephorus delicatulus (Bennett). Four 32 or 33 mm from Tongatabu. Richard's drawing 95 mm long.

CLUPEIDÆ

**Clupea arcuata* Jenyns. One 87 mm from Rio Janeiro.

**Arengus sagax* (Jenyns). Fig. 4. Two 125 to 223 mm from Callao, Peru. One 90 mm without data. Drawing 230 mm long by Richard.

Sardinella sirm (Walbaum). Drawing, of Samoan example, by Richard 65 mm long.

Sardinella allecia (Rafinesque). Fig. 5. One 135 mm from "Callao" likely from Rio Janeiro, like one but 67 mm long. Richard's drawing 150 mm long from Rio Janeiro.

**Harengula peruana* Fowler and Bean. Fig. 6. Type 105 mm from Callao, Peru.

**Harengula majorina* Storey. Drawing 124 mm long of Rio Janeiro example.

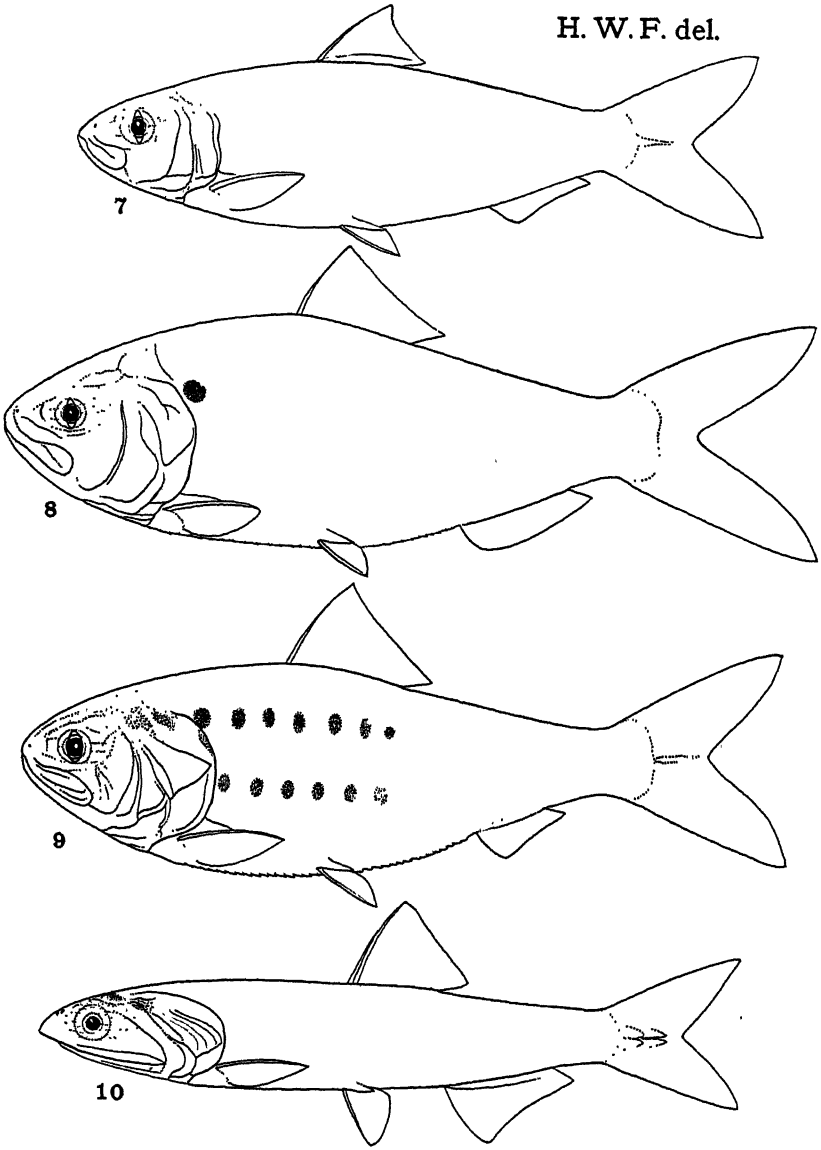
Harengula ovalis (Bennett). Eight 35 to 40 mm from Balabac Straits.

**Wilkesina nymphæa* (Richardson). Types of *Harengula fijiense* Fowler and Bean from Fiji, 64 to 73 mm.

Kowala coval (Cuvier). Fig. 7. Drawing 130 mm by Richard of example from Manila.

**Brevoortia pectinata* (Jenyns). Fig. 8. Three, largest 200 mm, from Rio Janeiro. Drayton's painting 160 mm long dated December 1839, and Richard's drawing 230 mm long.

**Ethmidium maculatum* (Valenciennes). Fig. 9. Two 215 mm from Peru. Drawing 160 mm by Dougal and one 170 mm long by Richard, both from Valparaiso specimen. Also figure by Richard 235 mm long.



7. *Kowala coval* (Cuvier).
8. *Brevoortia pectinata* (Jenyns).
9. *Ethmidium maculatum* (Valenciennes).
10. *Engraulis ringens* Jenyns.

Dorosomidæ

Anodontostoma chacunda (Buchanan-Hamilton). Drawing 150 mm long by Richard of example from Manila.

Engraulidæ

**Engraulis mordax* Girard. Drawing 84 mm long by Richard of San Francisco example.

Engraulis ringens Jenyns. Fig. 10. Drawing 135 mm long by Dougal from Peru or Valparaiso.

Engraulis australis (Shaw). Fig. 11. Types of *Anchoviella mauii* Fowler and Bean, wrongly "Maui", likely from Australia or New Zealand.

**Anchoviella salvatoris* Fowler and Bean. Fig. 12. Types from Rio Janeiro.

Anchoviella commersonnii (Lacepède). Drawing 120 mm long by Richard of example from Manila.

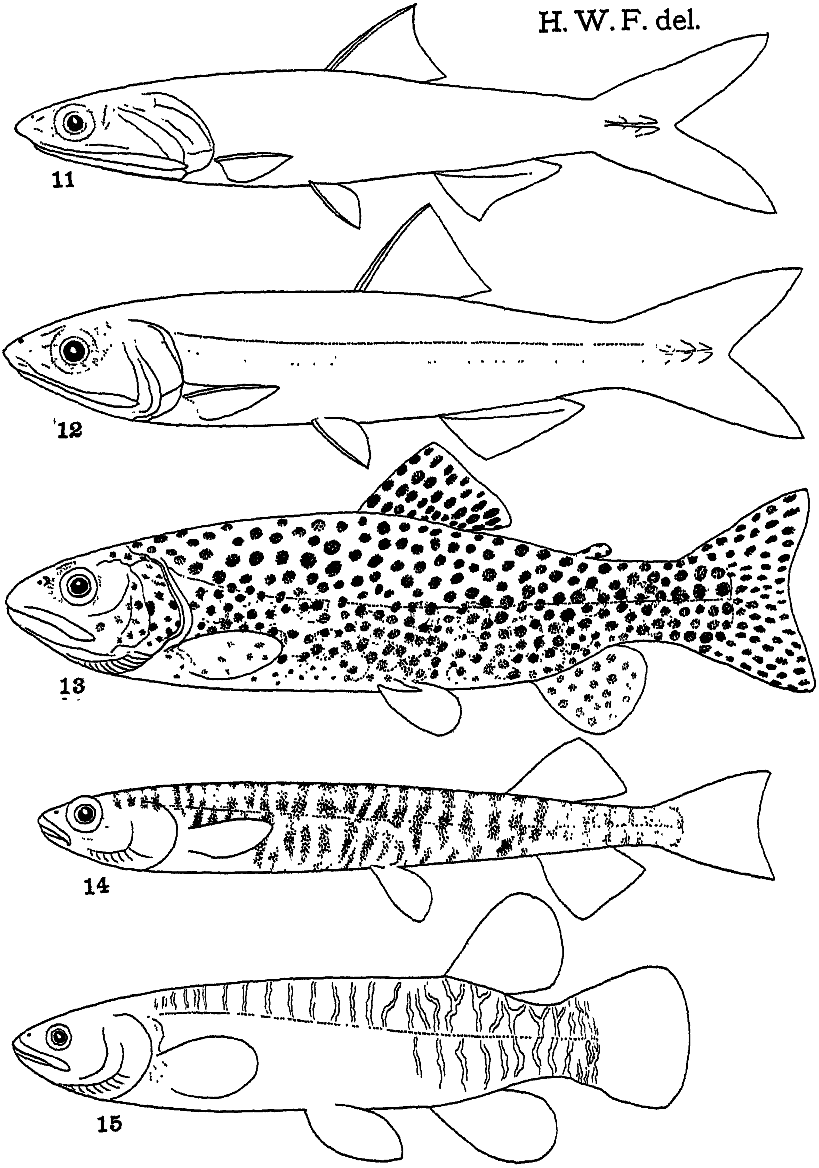
Thrissocles setirostris (Broussonet). One 123 mm from Manila.

Salmonidæ

Salmo clarkii Richardson. Fig. 13 (five miles above Fort Vancouver, June 22, 1841). One 180 mm Columbia River, Oregon, and one 68 mm from Puget Sound. Three figures by Drayton: uppermost from the Willamette Falls, June 1841; median a parr "caught at Dr. Whitmans with a hook in the Walla Walla River, July 10, 1840"; lower figure of example "caught with a hook, salt salmon bait, at the sawmill 5 miles above Fort Vancouver, June".

Salmo gairdnerii Richardson. Painting of one from Walla Walla, July 16, 1841.

Oncorhynchus tschawytscha (Walbaum). Painting by Drayton of one 1178 mm from Willamette Falls, June 12, 1841. "Some of these fish are much darker than the above, and some lighter and more silvery on the belly. They are very numerous at the falls and hundreds are seen at one time leaping in the rippled water and trying to ascend. Some get up over the lesser falls. They often leap to 10 feet out of water, some 15 feet, directly under the falls. Only this kind here in May and June. July and August two others come up."



11. *Engraulis australis* (Shaw).
12. *Anchoviella salvatoris* Fowler and Bean.
13. *Salmo clarkii* Richardson.
14. *Galaxias maculatus* (Jenyns).
15. *Galaxias fasciatus* Gray.

RETROPINNIDÆ

Retropinna retropinna (Richardson). Four 75 to 85 mm without data, doubtless from New Zealand.

GALAXIIDÆ

Galaxias attenuatus (Jenyns). Two from New Zealand, one from Sydney, 92 to 113 mm.

Galaxias maculatus (Jenyns). Fig. 14 (Tierra del Fuego). Five 53 to 63 mm from Tierra del Fuego. The original painting by Drayton gives "11 specimens from fresh-water lakes near Orange Harbor", April 1839.

Galaxias fasciatus Gray. Fig. 15. One 178 mm from New Zealand in "fresh water near Bay of Islands, March 1840". Drayton's painting 200 mm long.

STERNOPYGIDÆ

Vinciguerra attenuata (Cocco). Drawing 70 mm long without data.

Argyropelecus olfersii (Cuvier). Drawing 70 mm long from lat. 33′ 40° N., long. 62′ 10° W., by ship "Relief", August 30, 1838.

TACHYSURIDÆ

Tachysurus seemanni (Günther). One 270 mm without locality, likely Peru?

Tachysurus caelatus (Valenciennes). One 106 mm from Singapore.

Tachysurus fürthii (Steindachner). Two 155 to 188 mm without locality, likely from Peru?

Tachysurus leiotetocephalus (Bleeker). One 250 mm from Manila.

Tachysurus maculatus (Thunberg). One 200 mm labeled "Pacific", possibly from Manila?

PLOTOSIDÆ

Plotosus anguillaris (Bloch). Five 70 to 168 mm, from "Pacific" and Tongatabu, and small one from Fiji?

CLARIIDÆ

Clarias batrachus (Linnæus). One 240 mm from "Pacific?"

Pygidiidæ

**Pygidium dispar* Tschudi. Three 133 to 240 mm from Peru.

Cyprinidæ

**Acrocheilus alutaceus* Agassiz. Fig. 16 (Willamette, June 14, 1841). Two 285 mm from "Pacific Coast, North America", likely types. On original drawings "this fish caught with a hook at Dr. Whitman's, in the Walla Walla River. Same as the one taken in the mountains at Willamette Falls". Sketch by Drayton July 10, 1841. Also painting by Drayton, June 14, 1841, says "caught by natives while fishing in the falls for salmon. The fish is never taken but in rapids or falls, by a net. Natives think a great deal of them. They enter into their superstitions in salmon fishing".

Clarkina caurina (Richardson). Fig. 17 (Fort Vancouver). Drawing of *Mylocheilus lateralis* Agassiz and Pickering is 237 mm long, or ⅔ natural size according to the original sketch, which says "caught with a hook in the Columbia River at Fort Vancouver. J. Drayton. May 31, 1841". Another drawing of one 110 mm gives "J. Drayton, Nesqually, Puget Sound, May 1841, from a fresh-water creek near the fort".

**Pogonichthys macrolepidotus* (Ayres). Fig. 18. Two drawings of specimens from Sacramento River, California.

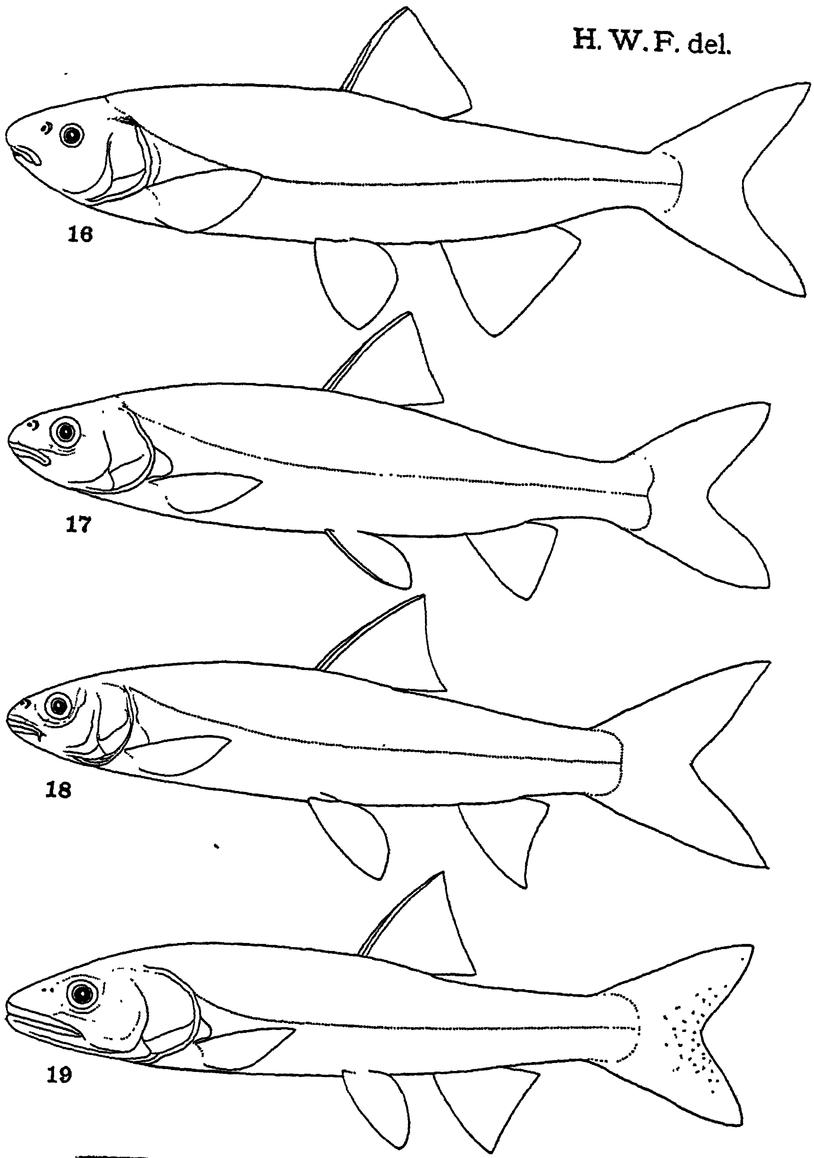
Ptychocheilus oregonensis (Richardson). Fig. 19 (Willamette Falls, June 13, 1841). Four 74 to 90 mm without data, likely from Willamette Falls? The figure of the type of *P. gracilis* Agassiz is based on a specimen 232 mm, according to the original sketch ⅔ natural size by Drayton, labelled Willamette Falls, June 13, 1841, and "caught with a hook". This figure is 245 mm long.

Richardsonius balteatus (Richardson). Sketch of small one "caught with a hook at Dr. Whitman's July 10, 1841, in Walla Walla River".

**Rasborella dubia* Fowler and Bean. Fig. 20. Types without data, likely Indo-Malayan?

Characidæ

**Astyanax fasciatus aeneus* (Günther). One 60 mm without locality, likely from Rio Janeiro?



16. *Acrocheilus alutaceus* Agassiz.
17. *Clarkina caurina* (Richardson).
18. *Pogonichthys macrolepidotus* (Ayres).
19. *Ptychocheilus oregonensis* (Richardson).

**Bryconamericus peruanus* (Müller and Troschel). Fig. 21. Five 78 to 105 mm, "Rio Rimac, Lima, Peru" according to figures, dated June 1839.

**Lebiasina bimaculata* Valenciennes. Four 82 to 122 mm from Peru.

MURÆNIDÆ

Muræna mauritiana (Bennett). One 287 mm without locality, possibly Manila?

**Muræna celebesensis* (Kaup). One 369 mm without locality, likely Polynesia?

**Muræna bicolor* (McClelland). One 263 mm without data, likely Philippines?

Muræna australis (Richardson). Three drawings of specimens from New Zealand and Tahiti.

MURŒNESOCIDÆ

Murœnesox arabicus (Schneider). Head of large example without data. Agate's painting of an example 1270 mm long from Fiji, August 5, 1840, may be this specimen.

CONGRIDÆ

Conger cinereus Rüppell. One 690 mm, Hawaiian Islands.

**Veternio verus* Snyder. Fig. 22. Drawing of "Sandwich Islands" (Hawaiian) example by Richard in 1858 shows the end of the tail with a caudal.

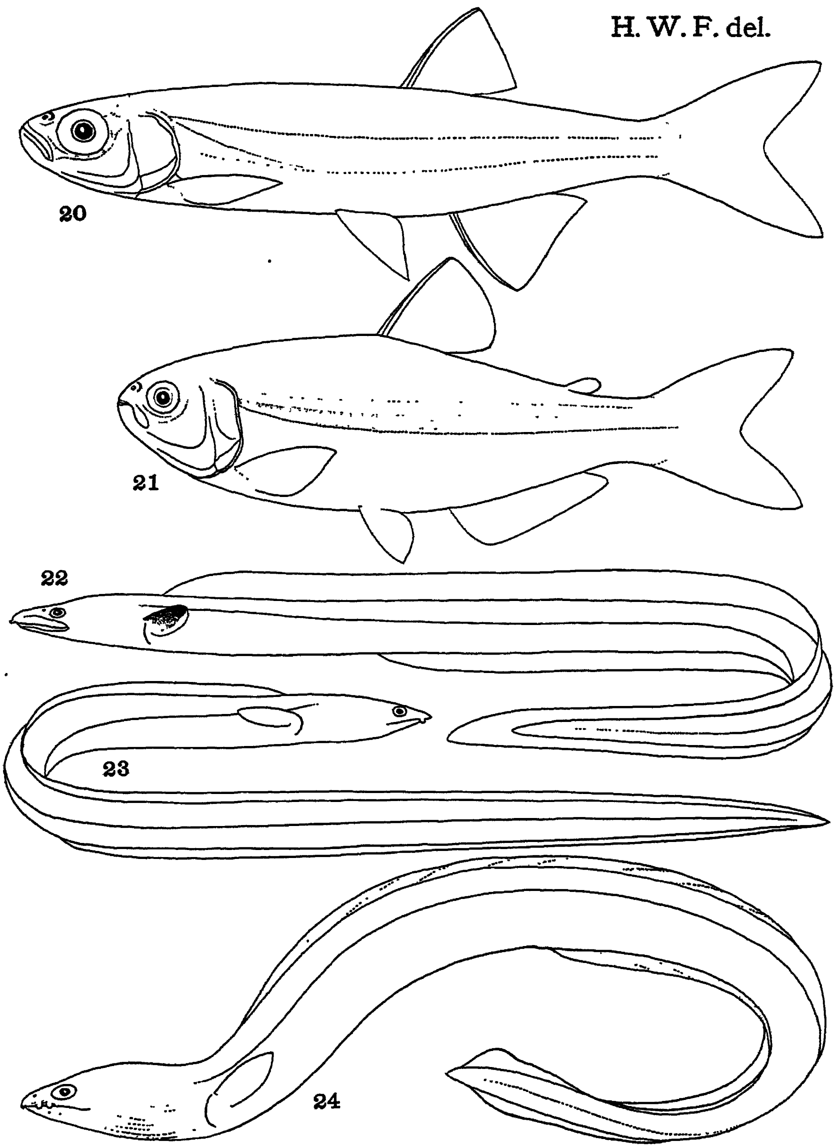
OPHICHTHYIDÆ

Chlevastes colubrinus (Boddaert). One 887 mm without locality, according to the drawing by J. H. Richard obtained in Samoa. Color sketch by Drayton from Opolu specimen November 1839.

Chlevastes colubrinus fasciatus (Ahl). One 558 mm. from Fiji. One without data may have been the subject of Richard's drawing in 1858 labeled Samoa.

Myrichthys maculosus (Cuvier). One 405 mm from Fiji. Drawing by Richard without data largely agrees.

**Ophichthus cephalozona* Bleeker. Two 468 to 490 mm without data likely from Fiji, which locality given on the drawing by Richard in 1858.



20. *Rasborella dubia* Fowler and Bean.
21. *Bryconamericus peruanus* (Müller and Troschel).
22. *Veternio verus* Snyder.
23. *Ophichthus gomesii* (Castelnau).
24. *Ophichthus callaensis* Günther.

**Ophichthus gomesii* (Castelnau). Fig. 23. One 413 mm without locality. Rio Janeiro on Richard's drawing in 1858.

**Ophichthus callaensis* Günther. Fig. 24. Three 320 to 454 mm without locality. Drawing by Richard in 1858 labeled Valparaiso.

**Ophichthus polyophthalmus* Bleeker. One 690 mm without data. Labeled "Sandwich Islands" on Richard's drawing in 1858.

ECHIDNIDÆ

**Murenophis pardalis* (Schlegel). One 230 mm without locality, likely from Polynesia?

**Murenophis pavonina* (Richardson). One 220 mm wrongly "Cape Verde Islands" likely from Polynesia?

Lycodontis meleagris (Shaw and Nodder). Two 245 to 325 mm, one without locality, the other from Fiji.

Lycodontis undulata (Lacepède). Seven 225 to 840 mm of which two from Fiji, one from Hawaii and the others without data.

Lycodontis picta (Ahl). Three 228 to 505 mm, one without locality and two from Fiji. Richard's drawing labeled Pomotu and may refer to the first. Two young 117 to 125 mm without locality.

**Lycodontis petelli* (Bleeker). Painting without data. Richard's figure in 1858 270 mm from specimen taken in Fiji.

Lycodontis anatina (Lowe). Drawing 220 mm of example from Cape Verde Islands.

Lycodontis ocellatus (Agassiz). One 453 mm without locality, doubtless from Rio Janeiro.

Echidna zebra (Shaw). One 383 mm from Samoa. According to Drayton's sketch from Opolu, November 1839.

**Echidna polyzona* (Richardson). Drawing 265 mm long by Richard of example without data.

Echidna nebulosa (Ahl). Four 150 to 288 mm without locality, and one of original drawings by Richard labeled Fiji.

Uropterygius marmoratus (Lacepède). Two, one from Fiji, other without data.

Uropterygius tigrinus (Lesson). One 133 mm without locality, doubtless from Polynesia.

**Uropterygius fijiensis* Fowler and Bean. Type from Fiji.

Synodontidæ

Synodus japonicus (Houttuyn). Four 77 to 180 mm, of which three from Fiji and one Hawaiian.

Saurida gracilis (Quoy and Gaimard). Six 67 to 110 mm, of which one from Manila, one from Samoa and three from Oahu.

Myctophidæ

Myctophum cocco (Risso). Drawing 133 mm long without data.

Cyprinodontidæ

**Fitzroyia lineata* (Jenyns). One 57 mm long from Rio Janeiro.

Pœciliidæ

**Phalloptychus januarius* (Hensel). Male 18 mm and two females 28 mm from Rio Janeiro.

**Phalloceros caudimaculatus* (Hensel). One from Rio Janeiro.

Pœcilia vivipara Schneider. Six, males 33 to 48 mm, females 47 to 60 mm, from Rio Janeiro.

Belonidæ

Belone platyura Bennett. Type of *Strongylura tahitiensis* Fowler and Bean from Tahiti.

Strongylura crocodila (Le Sueur). Two 330 to 610 mm without locality, one listed from Fiji or Samoa though label gives "south-east of Tahiti."

Strongylura strongylura (Van Hasselt). One from Singapore.

Strongylura timucu (Walbaum). Fig. 25. One from Rio Janeiro.

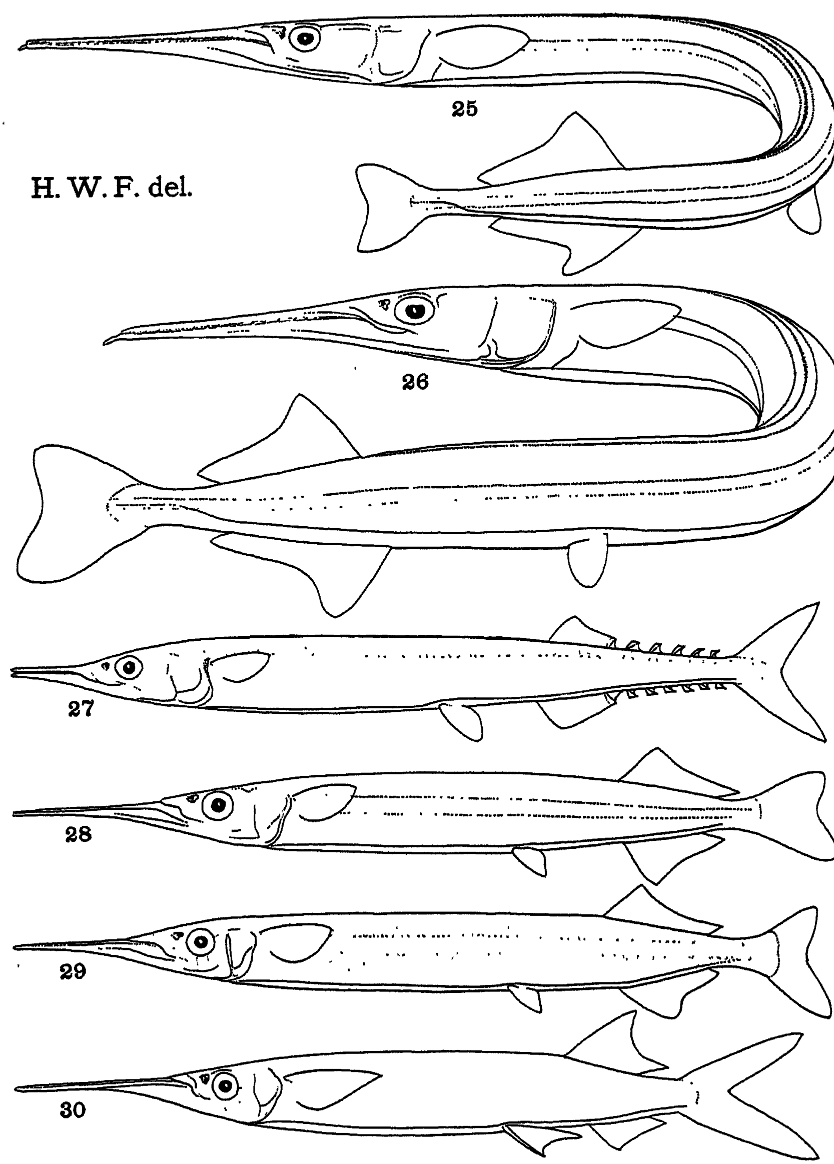
**Strongylura stolzmanni* (Steindachner). Fig. 26. One 620 mm from Peru.

Strongylura choram (Rüppell). Drawing of example from Tahiti by Richard.

**Strongylura auloceps* Fowler and Bean. Type from "Fiji" or Samoa" though original label gives Fiji.

Strongylura indica (Le Sueur). Two 155 to 188 mm from Hawaiian Islands. Painting by Drayton of example 660 mm long from Tahiti, September 1939.

**Strongylura fijiensis* Fowler and Bean. Type from Fiji.



25. *Strongylura timucu* (Walbaum).
26. *Strongylura stolzmanni* (Steindachner).
27. *Scomberesox equirostrum* Le Sueur.
28. *Hyporhamphus salvatoris* Fowler and Bean.
29. *Hemiramphus intermedius* Cantor.
30. *Hemiramphus laticeps* Günther.

Scomberesocidæ

Scomberesox equirostrum Le Sueur. Fig. 27. Drawing of example from Valparaiso, 252 mm.

Scomberesox saurus (Walbaum). Painting of example from Boston market, October 28, 1869.

Hemiramphidæ

**Hyporhamphus regularis* (Günther). Two 11 to 245 mm from Sydney.

**Hyporhamphus salvatoris* Fowler and Bean. Fig. 28. Type from Rio Janeiro.

**Hyporhamphus kronei* Ribeiro. Drawing of example 185 mm from Rio Janeiro.

Hemiramphus intermedius Cantor. Fig. 29. One 300 mm from New Zealand. According to Drayton's painting from Bay of Islands, March 1840.

Hemiramphus marginatus (Forskål). One 115 mm listed from "Sydney" erroneous as on original label Tongatabu. Richard's figure gives Tongatabu.

Hemiramphus far (Forskål). Two listed without locality, Fiji on original label.

Hemiramphus brasiliensis (Linnæus). Drawing of Hawaiian example by Richard.

**Hemiramphus laticeps* Günther. Fig. 30. Figure without data, likely from Fiji? and one from Opolu, November 1, 1839.

Euleptorhamphus viridis (Van Hasselt). One 400 mm from Hawaiian Islands.

Exocœtidæ

Oxyporhamphus argenteus (F. D. Bennett). One from the Phœnix coral group and obtained from the stomach of a gannet (*Sula*).

**Parexocœtus brachypterus* (Richardson). One 155 mm. from Hawaiian Islands. The uncolored plate gives Lahiana, Sandwich Islands, Rev. Mr. Richards.

Exocœtus volitans Linnæus. Figure with a poor dorsal view labeled South Pacific Ocean in "lat. 26° 7′ N., long. 174° 2′ E., April 18, 1840". Also specimen from between Sunday Island and Tonga and one from near Rosa Island.

Cypsilurus furcatus (Mitchill). Two colored figures, labeled off Cape Frio near Rio Janeiro.

**Cypsilurus cyanopterus* (Valenciennes). Sketch 390 mm long by Drayton, October 1838.

**Cypsilurus brachysoma* Bleeker. Outline drawing 170 mm long, unsigned, of example from Manila.

Gadidæ

**Microgadus proximus* (Girard). Two 250 to 254 mm from Puget Sound.

**Phycis latus* (Ribeiro). One 220 mm from Rio Janeiro.

Physiculus bachus (Schneider). One 305 mm from near Loba, New Zealand.

Eleginops maclovinus (Cuvier). One 290 mm from Valparaiso, also two from Orange Harbor, Tierra del Fuego.

Ophidiidæ

Genypterus blacodes (Schneider). One 224 mm from Valparaiso.

Pleuronectidæ

**Hippoglossina macrops* Steindachner. One 167 mm wrongly labeled "Fiji," more likely California?

**Paralichthys orbignyana* (Valenciennes). Types of *Xystreurys ribeiroi* Fowler and Bean, from Rio Janeiro.

**Paralichthys californicus* (Ayres). Two 93? to 110? mm labeled "Fiji," more likely from California?

**Paralichthys adspersus* (Steindachner). One 163 mm without locality, likely from Peru?

**Rhombosolea leporina* Günther. One 244 mm labeled "New Zealand," probably from Sydney? Two drawings 184 mm long by Richard labeled "New Zealand."

**Parophrys vetulus* Girard. One 83 mm labeled "Sandwich Islands," probably from Peru?

**Lepidopsetta bilineata* (Ayres). One 265 mm from Puget Sound.

Platichthys stellatus (Pallas). One 335 mm without locality, likely from Puget Sound?

Bothus mancus (Broussonet). Length 58 to 265 mm, one from Pomotous and two from Hawaiian Islands. Two drawings 164 mm long by Richard labeled Manila.

Bothus pantherinus (Rüppell). Length 40 to 130 mm, two Hawaiian, two from Mauii and three without data, likely from same region?

SOLEIDÆ

Achirus achirus (Linnæus). One 35 mm from Rio Janeiro.

CYNOGLOSSIDÆ

**Cynoglossus puncticeps* (Richardson). One 103 mm from Manila.

**Cynoglossus quadrilineatus* (Bleeker). One 203 mm without locality, probably from the Philippines?

Symphurus plagusia (Schneider). One 105 mm from Rio Janeiro.

BERYCIDÆ

**Trachichthodes affinis* (Günther). Fig. 31. One 265 mm from Sydney.

HOLOCENTRIDÆ

Corniger spinosus Agassiz. Fig. 32. One 168 mm from Rio Janeiro.

Holocentrus diadema Lacepède. One 100 mm long from Samoa?

Holocentrus spinifer (Forskål). Length 55 to 252 mm, one from Oahu and three from Samoa.

Holocentrus lacteo-guttatus Cuvier. One 135 mm long from Oahu.

Holocentrus sammara (Forskål). Length 42 to 170 mm, two from Samoa, one from Tahiti and two from Fiji.

Holocentrus opercularis Valenciennes. One 135 mm from Samoa.

Holotrachys lima (Valenciennes). Fig. 33. Drawing 158 mm long of example from Oahu.

**Myripristis adustus* Bleeker. Colored figure without data, likely from Polynesia?

**Myripristis intermedius* Günther. Eight 65 to 140 mm from Samoa.

Myripristis jacobus Cuvier. Drawing 168 mm of example from Porto Praya, Cape Verde Islands.

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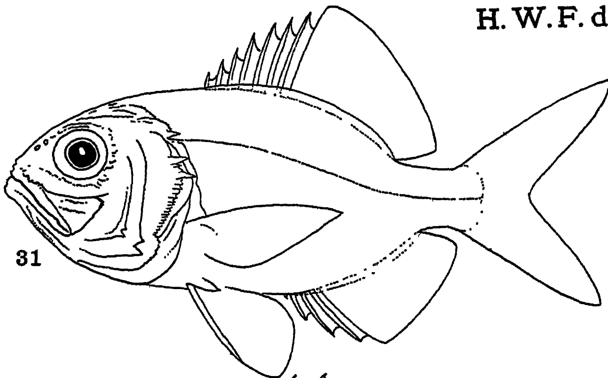

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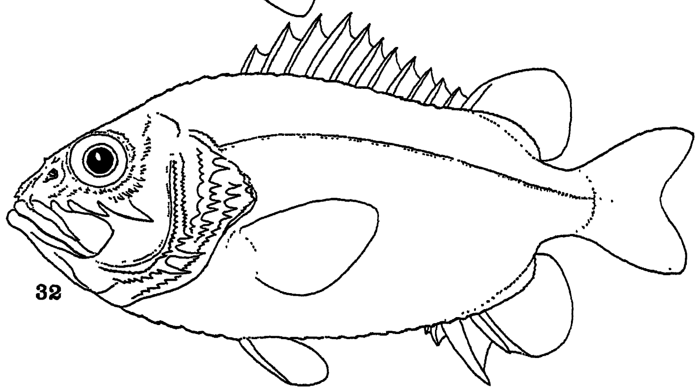

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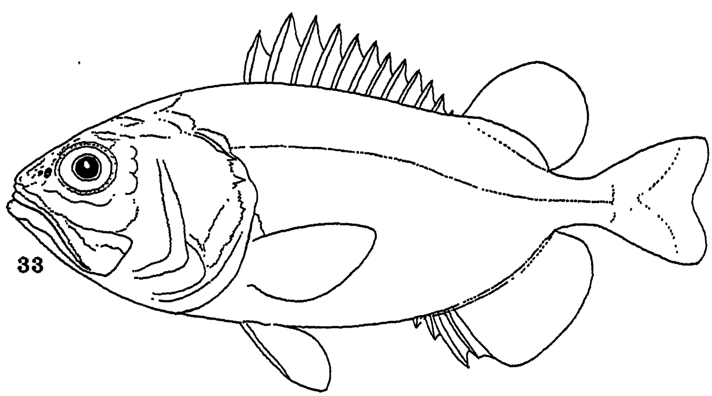

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31. *Trachichthodes affinis* (Günther).
32. *Corniger spinosus* Agassiz.
33. *Holotrachys lima* (Valenciennes).

GASTEROSTEIDÆ

Gasterosteus aculeatus Linnaeus. Six 45 to 52 mm from Oregon. Drawing 38 mm long of example labeled Columbia River.

SYNGNATHIDÆ

**Syngnathus rousseau* Kaup. Fig. 34. Four 79 to 115 mm from Rio Janeiro.

Syngnathus blainvilleanus Eydoux and Gervais. Fig. 35. One 180 mm from Valparaiso.

Syngnathus acus Linnæus. Fig. 36. Drawing of example from the North Atlantic (lat. 33° 13′ N., long. 49° 19′ W.).

Syngnathus pelagicus Linnæus. Fig. 37. Drawing of example from Orange Harbor, Tierra del Fuego.

Corythoichthys flavofasciatus (Rüppell). Seven 72 to 98 mm from Samoa. Color sketch by J. Drayton 105 mm long, says Opolu, November 1839.

**Micrognathus mataafæ* (Jordan and Seale). One from Tongatabu, 53 mm.

Solegnathus hardwickii (Gray). Three 285 to 385 mm labeled China, though the original sketch gives Japan.

Hippocampus abdominalis Lesson. Two 147 to 225 mm from Australia.

**Hippocampus villosus* Günther. Drawings of three examples from Rio Janeiro.

**Hippocampus histrix* Kaup. One 205 mm from Japan?

AULOSTOMIDÆ

Aulostomus chinensis (Linnæus). One from Oahu.

FISTULARIIDÆ

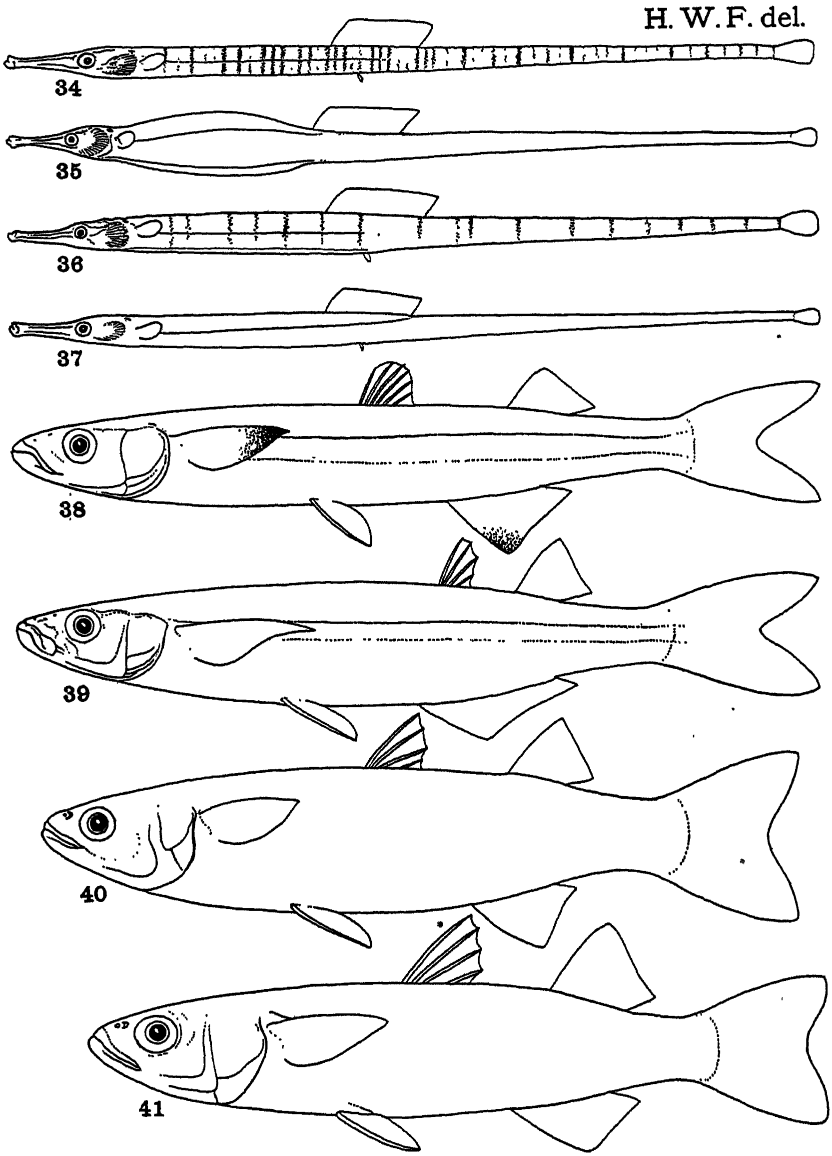
Fistularia petimba Lacepède. Three 204 to 413 mm to caudal tips, from Oahu, Pomotous and Fiji.

ATHERINIDÆ

Atherina pinguis Lacepède. Two 105 to 107 mm from Sydney.

**Atherina insularum* Jordan and Evermann. Three 53 to 87 mm from Maui.

**Cauque mauleanum* (Steindachner). Fig. 38. One 83 mm from Valparaiso.



34. *Syngnathus rosseau* Kaup.
35. *Syngnathus blainvilleanus* Eydoux and Gervais.
36. *Syngnathus acus* Linnaeus.
37. *Syngnathus pelagicus* Linnaeus.
38. *Cauque mauleanum* (Steindachner).
39. *Thyrina brasiliensis* (Quoy and Gaimard).
40. *Mugil elongatus* Günther.
41. *Agonostomus forsteri* (Valenciennes).

Austromenidia regia (Humboldt). Two 183 to 213 mm from Callao, Peru.

Thyrina brasiliensis (Quoy and Gaimard). Fig. 39. One 103 mm from Rio Janeiro. Color sketch 115 mm by Drayton of Rio Janeiro example, dated December 1838.

MUGILIDÆ

Mugil cephalus Linnæus. Length 175 to 230 mm, two from Lima and one from Callao, Peru; one from Valparaiso, Chile. One 54 mm in *Querimana* stage from Maui; one from Hull's Island; two 17 to 20 mm from Fiji.

**Mugil trichilus* Vaillant and Sauvage. Five 90 to 145 mm from Tongatabu, Fiji and Tahiti.

Mugil dussumieri Valenciennes. One 233 mm from Manila.

Mugil curema Valenciennes. One 170 mm from Rio Janeiro.

**Mugil elongatus* Günther. Fig. 40. One 221 mm from Sydney.

Neomyxus chaptalii (Eydoux and Souleyet). Two 185 to 263 from Wake Island.

Agonostomus forsteri (Valenciennes). Fig. 41. Two 125 to 205 mm larger labeled "Maui" though doubtless from New Zealand, like the smaller specimen.

SPHYRÆNIDÆ

**Sphyræna langsar* Bleeker. Length 120 to 200 mm, two from Manila and one from Fiji.

**Sphyræna tome* Fowler. One 245 mm from Rio Janeiro.

**Sphyræna helleri* Jenkins. Two 208 to 278 mm Maui, one said to be from fresh-water.

POLYNEMIDÆ

Polydactylus virginicus (Linnæus). Two 145 to 285 mm from Rio Janeiro.

Polydactylus sexfilis (Valenciennes). Two 170 to 227 mm from Oahu.

Polydactylus plebejus (Bonnaterre). One 130 mm from Manila.

Galeoides decadactylus (Bloch). Two 185 to 203 mm from Cape Verde Islands.

Scombridæ

Scomber japonicus Houttuyn. Length 218 to 230 mm, three from Mangsi Island and one without locality, likely from the Pacific?

Scomber chrysozonus Rüppell. Painting by Drayton of small example taken at Balabac Straits, February 1842.

**Scomber peruanus* (Jordan and Hubbs). Fig. 42. Drawing 215 mm long of example from Peru.

**Parathunnus mebachi* Kishinouye. Fig. 43 (smaller). Two paintings of specimens without locality appear to be this species, smaller November 10, 1838, by Drayton, and larger by Dougal in January 1847.

**Neothunnus macropterus* (Schlegel). Fig. 44 (length 1348 mm). Painting by Drayton of example 1348 mm long from lat. 6° 20′ S., long. 30° 30′ W. Also one 765 mm long by Drayton from lat. 9° 47′ S., long. 14° 03′ W., caught on a hook at sea. Both with a brilliant yellow lateral band.

Scomberomorus commerson (Lacepède). Drawing by Richard of example from Manila.

Acanthocybium solandri (Cuvier). Figure without data 1295 mm long.

Trichiuridæ

Trichiurus lepturus Linnæus. Outline 482 mm long, unsigned, of example from Rio Janeiro.

Carangidæ

Scomberoides lysan (Forskål). One 270 mm from Manila.

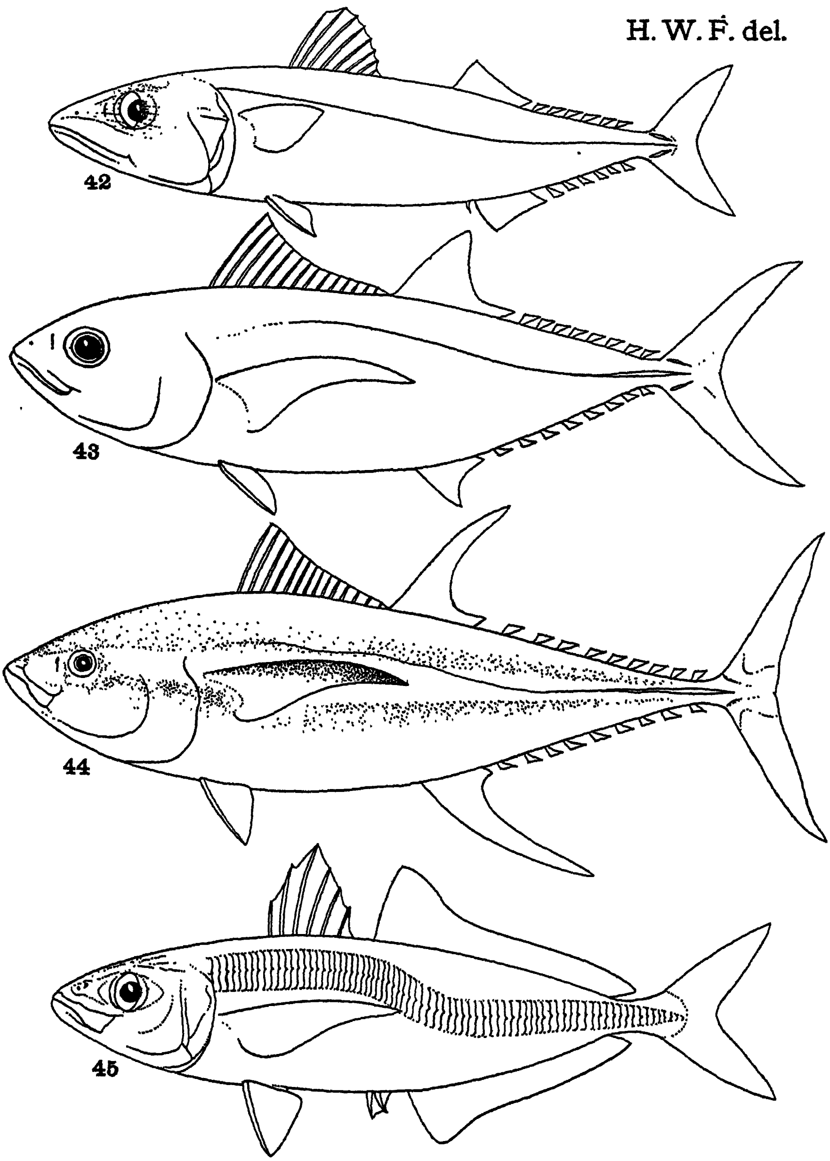
Scomberoides toloo-parah (Rüppell). Length 92 to 449 mm, two from Hilo and others without locality, likely Polynesia? Painting of example from Hilo Bay, June 19, 1841. Drawing of Hilo example and small one from Samoa by Richard.

Oligoplites saurus (Schneider). One without locality, evidently from Rio Janeiro?

Oligoplites saliens (Bloch). One without locality, apparently from Rio Janeiro? Drawing by Richard of example from Rio Janeiro.

Cæsiomorus glaucus (Linnæus). Figure by Richard of example from Cape Verde.

Naucrates ductor (Linnæus). One 223 mm from Oahu.



42. *Scomber peruanus* (Jordan and Hubbs).
43. *Parathunnus mebachi* Kishinouye.
44. *Neothunnus macropterus* (Schlegel).
45. *Trachurus declivis* (Jenyns).

Decapterus pinnulatus (Eydoux and Souleyet). Three 188 to 300 mm, one from Oahu and two without locality, latter probably from the Pacific? Drawing of Maui example by Richard.

**Trachurus declivis* (Jenyns). Fig. 45 (New Zealand). One 258 mm from New Zealand and one 140 mm from Sydney.

Selar crumenophthalmus (Bloch). Two without locality, possibly from Rio Janeiro? One 125 mm long drawn by Richard, from Cape Verde.

Alepes mate (Cuvier). One 44 mm long from Fiji.

**Alepes marginata* (Bleeker). One 38 mm from Cape de Verde Islands.

Alepes djedaba (Forskål). Drawing of example from Manila.

Caranx sexfasciatus Quoy and Gaimard. Length 98 to 138 mm, two from Fiji, one from Tahiti and one from Sulu. Also two 95 to 128 mm from Sulu. Three drawings by Richard, one each from Samoa, Fiji and Tahiti.

Caranx melampygus Cuvier. One 128 mm from Sulu, and two without locality 88 to 210 mm probably from Polynesia? Figure by Richard of one from Fiji and one from Tahiti.

Caranx ignobilis (Forskål). Two 145 to 200 mm, one Hawaiian and other without locality probably Pacific? Drawing by Richard of example from Hilo.

Caranx crysos (Mitchill). Two 77 to 88 mm without locality likely from Rio Janeiro?

**Caranx chilensis* Guichenot. One 41 mm long from Chili.

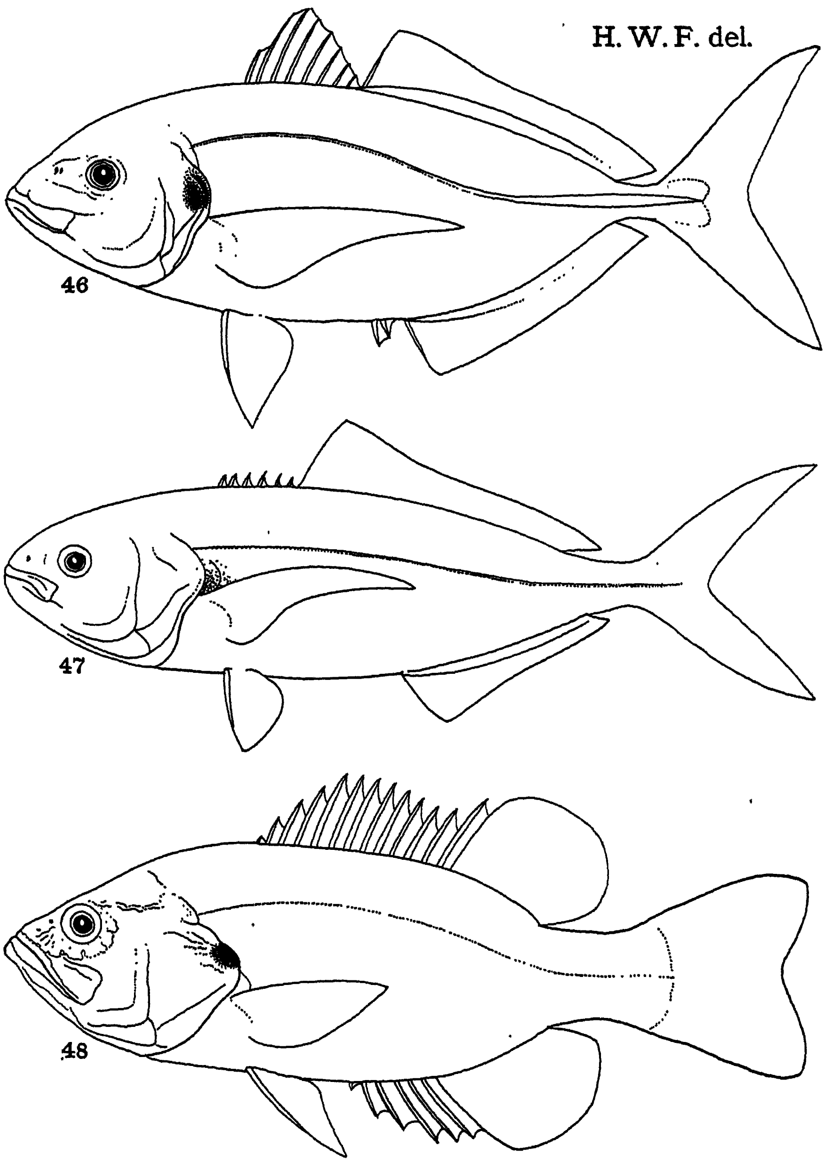
Caranx georgianus Valenciennes. Fig. 46 (New Zealand). One 195 mm from Sydney. Painting by Drayton labeled March 1840, Bay of Islands, New Zealand, "numerous". Drawing by Richard of example from Sydney.

**Caranx lugubris* Poey. One 285 mm without locality, likely from Rio Janeiro? Two very young from Rio Janeiro.

Gnathanodon speciosus (Forskål). Painting by Richard of Hawaiian example.

**Carangoides equula* (Schlegel). One 515 mm without locality likely Hawaiian?

Carangoides ferdau (Forskål). Figure of example from Tahiti by Richard. Painting by Drayton of specimen 1283 mm long with weight 50 lbs. from Lebukia, Fiji, May 1840.



46. *Caranx georgianus* Valenciennes.
47. *Seriolella crassus* (Starks).
48. *Archoplites interruptus* (Girard).

Vomer setapinnis (Mitchill). Length 185 to 275 mm, larger labeled South America? and the other Peru. Drawing by Richard of Rio Janeiro specimen.

Selene vomer (Linnæus). One 163 mm from Rio Janeiro.

Chloroscombrus chrysurus (Linnæus). Two 138 to 143 mm from Rio Janeiro.

Trachinotus blochii (Lacepède). Length 93 to 150 mm, one without locality and the other from Samoa? Drawing by Richard of Samoan example.

Trachinotus baillonii (Lacepède). One 330 mm without locality likely from Polynesia? Richard's drawing much smaller example from Pomotou.

Trachinotus falcatus (Linnæus). Drawing by Richard labeled Rio Janeiro.

Trachinotus carolinus (Linnæus). Two 20 or 21 mm from Rio Janeiro.

**Trachinotus paloma* Jordan. One 25 mm from Valparaiso, Chili.

Lactariidæ

Lactarius lactarius (Schneider). One 173 mm from Colombo, Ceylon.

Coryphænidæ

Coryphæna hippurus Lacepède. Three paintings of two adults and one young. One of adults figured by Drayton 635 mm long caught by natives in Hilo Bay in January 1841, called "mahimahi".

Stromateidæ

Seriolella brama (Günther). One 610 mm without locality doubtless from New Zealand?

**Seriolella crassus* (Starks). Fig. 47. Painting by Drayton of example from Callao, Peru, July 1839. Also sketch 210 mm long with same data.

Pampus chinensis (Euphrasen). Figure by Richard of Singapore example.

Seserinus paru (Linnæus). Two 85 to 185 mm without locality likely from Rio Janeiro? Drawing by Richard of specimen from Rio Janeiro.

LEIOGNATHIDÆ

Leiognathus splendens (Cuvier). Three 80 to 125 mm, one each from Manila, Mauda and Fiji.

Leiognathus equula (Forskål). Two 60 to 85 mm wrongly labeled "Rio Janeiro" more likely from the Philippines?

Leiognathus blochii (Valenciennes). Two 48 to 57 mm erroneously labeled "Rio Janeiro" are more likely from the Philippines?

Secutor insidiator (Bloch). Five 40 to 97 mm from Manila.

Secutor ruconius (Buchanan-Hamilton). Two 36 mm long from Singapore.

CHEILODIPTERIDÆ

Apogon frænatus Valenciennes. Two 115 to 150 mm, one from Oahu and the other "Oahu or Fiji".

**Apogon bandanensis* Bleeker. Five 54 to 75 mm, one from Samoa and two from Fiji, also one labeled "Fiji or Oahu" and the other "Fiji or Samoa".

**Apogon maculiferus* Garrett. Five 70 to 123 mm, three Oahu and two labeled "Fiji or Oahu".

Apogon novemfasciatus Cuvier. Four 47 to 77 mm, one from Philippines, or Fiji and two labeled "Fiji or Oahu".

**Apogon græffii* Günther. One 40 mm from Fiji.

Apogon auritus Valenciennes. Two 58 to 74 mm labeled "Fiji or Oahu" though the evidence unlikely that they were obtained in Hawaii.

CHANDIDÆ

Ambassis safgha (Forskål). One 68 mm from Fiji.

Ambassis nalua Buchanan-Hamilton. One 85 mm labeled "Fiji or Oahu" though surely not Hawaiian, more likely from Malaya?

**Ambassis batjanensis* Bleeker. One 68 mm labeled "Fiji or Oahu" more likely from Fiji?

DULEIDÆ

Dules rupestris (Lacepède). One 127 mm from Hawaiian Islands.

Dules marginatus Cuvier. Twelve 55 to 130 mm of which six Hawaiian? (more likely Pomotous), three from Tahiti, two from Samoa and one from Fiji.

**Dules sandvicensis* (Steindachner). Thirty-nine 77 to 258 mm from Hilo, Maui, Oahu, Hawaiian Islands, Wake Island and Fiji.

CENTRARCHIDÆ

**Archoplites interruptus* (Girard). Fig. 48 (Sacramento River). One 190 mm without locality likely from Sacramento River, California?

SERRANIDÆ

**Acanthistius pictus* (Tschudi). Fig. 49 (Callao). One 295 mm without locality doubtless from Valparaiso. Drawing 213 mm long of an example from Callao.

Acanthistius brasilianus (Cuvier). One 125 mm from Rio Janeiro.

Cephalopholis argus Schneider. One from Pomotou.

Cephalopholis urodelus (Schneider). One 178 mm from Pomotou.

Serranus fario (Thunberg). One 190 mm from Samoa.

Serranus fasciatus (Forskål). Three 148 to 270 mm, of which two from Pomotou and one from Tahiti.

Serranus merra (Bloch). Nine 51 to 205 mm, of which six from Fiji, one from Samoa and two without locality.

**Serranus socialis* Günther. Colored figure without data.

Serranus morio (Valenciennes). One 222 mm without locality likely from Rio Janeiro?

Mycteroperca ruber (Bloch). One 165 mm from Rio Janeiro.

Paralabrax humeralis (Valenciennes). One 174 mm from Callao, Peru.

**Paralabrax callaensis* Starks. Fig. 50 (Peru). One 193 mm without locality likely from Peru?

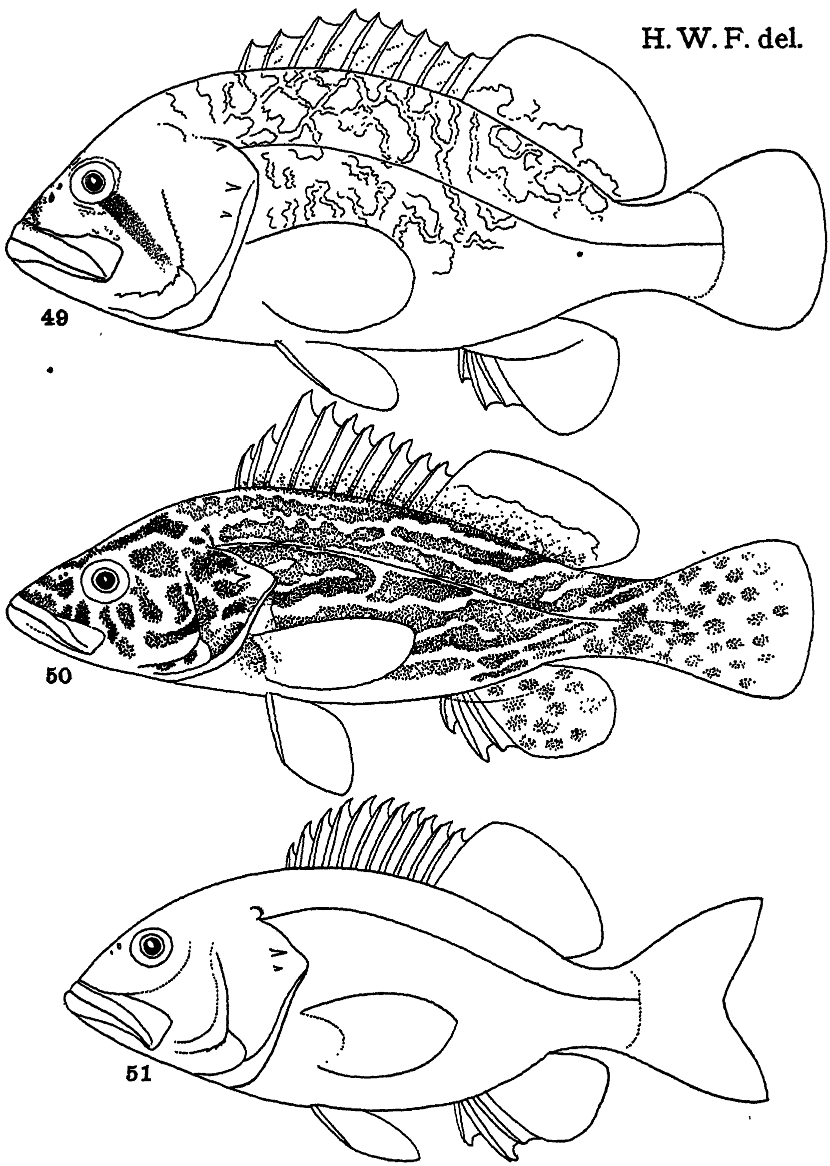
**Ellerkeldia huntii* (Hector). Fig. 51. Two 120 to 165 mm from Sydney.

Diplectrum radiale (Quoy and Gaimard). One 170 mm from Rio Janeiro.

Plesiops nigricans (Rüppell). Two 59 to 70 mm from Fiji and Balabac Passage.

ENOPLOSIDÆ

Enoplosus armatus (Shaw). One 117 mm from Sydney.



49. *Acanthistius pictus* (Tschudi).
50. *Paralabrax callaensis* Starks.
51. *Ellerkeldia huntii* (Hector).

Pseudochromidæ

**Pseudogramma polyacanthus* (Bleeker). One 43 mm from Tahiti.

**Pseudochromis melanotænia* Bleeker. One 36 mm from Balabac Passage.

Pempheridæ

Pemperis compressa (Shaw). One 107 mm from Sydney.

Priacanthidæ

Priacanthus arenatus Cuvier. One 163 mm from Rio Janeiro.

Priacanthus macracanthus Cuvier. Four 150 to 230 mm Hawaiian, one likely from Maui? Drawing 210 mm long of example from Maui.

Lutjanidæ

Lutjanus kasmira (Forskål). One 163 mm from Fiji.

Lutjanus vaigiensis (Quoy and Gaimard). Three 87 to 235 mm, two from Fiji and one from Samoa. Drawing 205 mm long of example from Pomotou and one 200 mm long by Richard from Fiji.

Lutjanus semicinctus (Quoy and Gaimard). Sketch 280 mm long by Drayton of example from Lebukia, Fiji, June 1840.

Pomadasyidæ

**Anisotremus scapularis* (Tschudi). Two 180 to 220 mm from Peru.

**Hæmulon steindachneri* (Jordan and Gilbert). Fig. 52 (Rio Janeiro). One 215 mm without locality probably from Rio Janeiro?

Conodon nobilis (Linnæus). One 145 mm from Rio Janeiro.

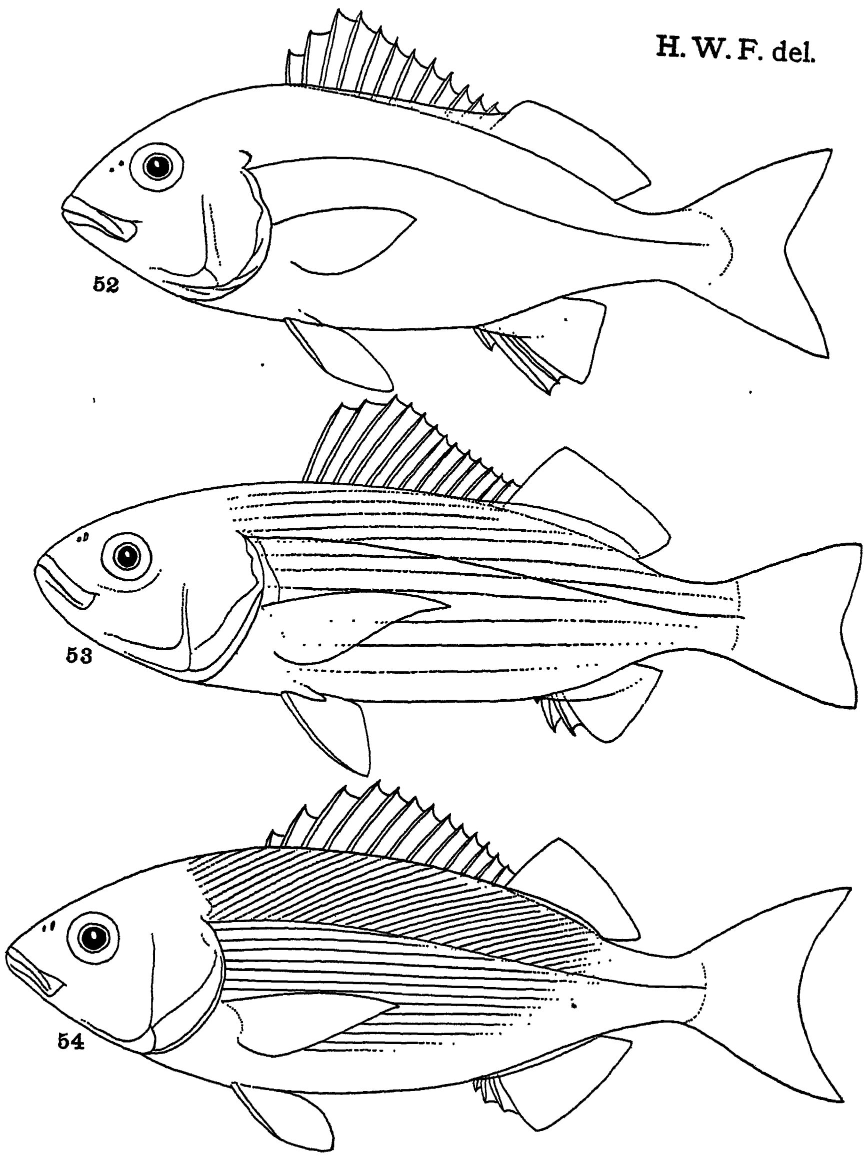
Orthopristis ruber (Cuvier). One 178 mm from Rio Janeiro.

**Orthopristis rhabdotus* Fowler and Bean. Fig. 53. Type without locality, thought to be from Peru?

**Pomadasys therapon* (Bleeker). One 133 mm from Manila.

Isacia conceptionis (Cuvier). Fig. 54 (Peru). Two 218 to 227 mm, one from Valparaiso and the other without locality likely from Chili? Drawing 230 mm long of specimen from Peru.

**Plectorhinchus polytænia* (Bleeker). One 323 mm from Sulu.



52. *Haemulon steindachneri* (Jordan and Gilbert).
53. *Orthopristis rhabdotus* Fowler and Bean.
54. *Isacia conceptionis* (Cuvier).

Plectorhinchus chætodonoides Lacepède. One 337 mm from Sulu.

Scolopsis monogramma (Cuvier). Two 190 to 268 mm, one from Fiji and another without locality likely from the Philippines?

TERAPONIDÆ

Terapon jarbua (Forskål). Three 167 to 245 mm, one from Fiji, one Samoa, and one "Sandwich Islands" more likely Polynesia?

LETHRINIDÆ

Lethrinus ramak (Forskål). One 410 mm without locality probably from Fiji?

Lethrinus mahsenoides Valenciennes. Three 115 to 322 mm from Fiji.

Lethrinus mahsena (Forskål). Drawing 180 mm long of example from Fiji.

SPARIDÆ

Symphorus forsteri (Schneider). Fig. 55. Painting by Drayton of example 635 mm long from Lebukia, Fiji, June 1840.

Pentapodus caninus (Cuvier). One 130 mm without locality, probably from the Philippines?

Nemipterus japonicus (Bloch). Five 45 to 158 mm, two labeled Manila and three "Sydney" may have been from the Philippines?

Dentex dentex (Linnæus). Drawing 93 mm of example from Cape Verde Islands.

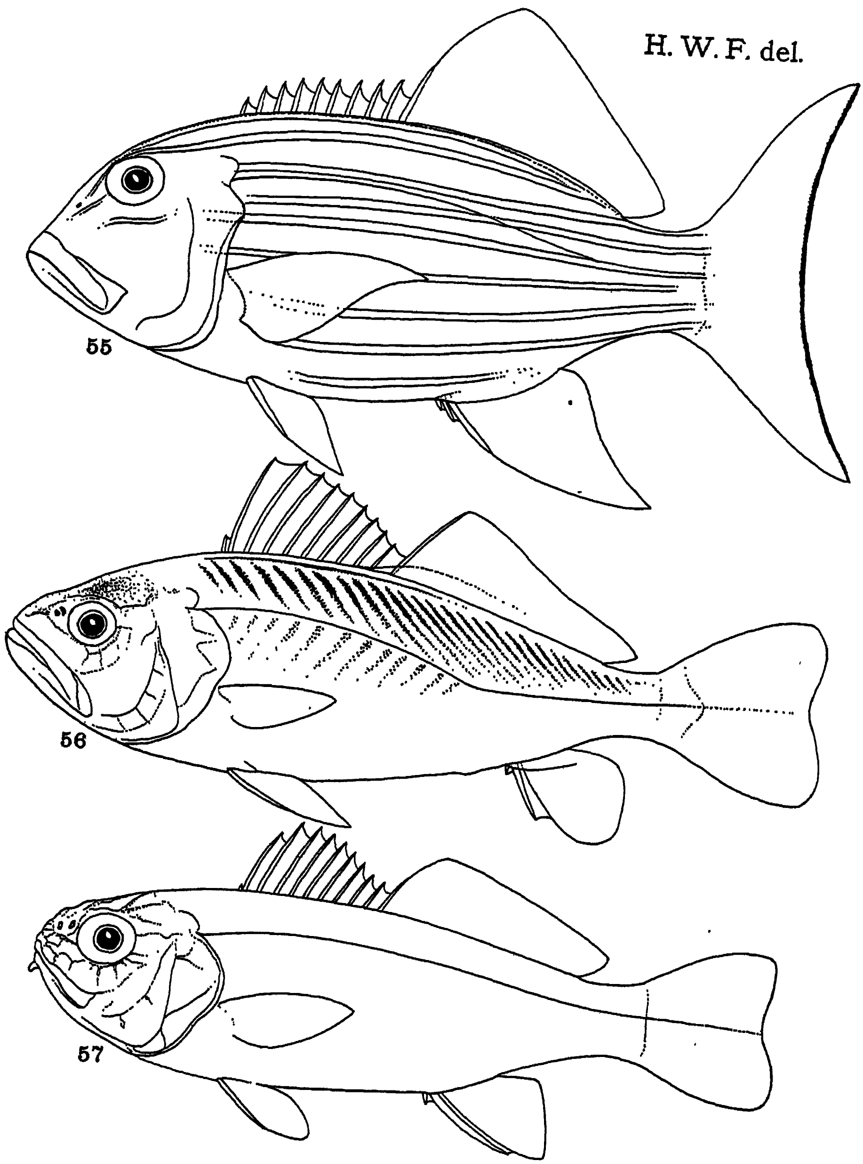
Dentex filosus Valenciennes. One 210 mm from the Cape of Good Hope and one labeled "New Zealand" may have been taken in South Africa?

Dentex macrophthalmus (Bloch). Two 48 to 52 mm wrongly labeled "Hawaii?" more likely from the Cape Verde Islands?

Pagrus pagrus (Linnæus). One 125 mm from Rio Janeiro.

Sparus auratus Linnæus. Four 103 to 290 mm from Sydney and Cape of Good Hope.

Pagellus mormyrus (Linnæus). One 155 mm from Cape de Verde Islands.



55. *Symphorus forsteri* (Schneider).
56. *Paralarimus patagonicus* Fowler and Bean.
57. *Sciaena dubia* Fowler and Bean.

Archosargus unimaculatus (Bloch). Three 38 to 160 mm from Rio Janeiro.

Diplodus argentus (Valenciennes). One 132 mm from Rio Janeiro.

GIRELLIDÆ

**Pachymetopon grande* Günther. Two 222 to 235 mm from Cape of Good Hope.

Girella tricuspidata (Quoy and Gaimard). One 238 mm from Sydney.

KYPHOSIDÆ

Kyphosus bigibbus (Lacepède). One 177 mm from Oahu.

**Doidyxodon laevifrons* (Tschudi). Six 31 to 98 mm without locality likely from Peru?

ARRIPIDÆ

Arripis trutta (Schneider). Two 250 to 268 mm from New Zealand.

SCIAENIDÆ

Cynoscion striatus (Cuvier). One 187 mm from northern Patagonia.

Cynoscion microlepidotus (Cuvier). One 171 mm from Rio Janeiro.

Larimus breviceps Cuvier. One 193 mm from Rio Janeiro.

**Paralarimus patagonicus* Fowler and Bean. Fig. 56. Type 162 mm from northern Patagonia.

Odontoscion dentex (Cuvier). Drawing 162 mm long from Patagonia.

**Stellifer minor* (Tschudi). One 168 mm without locality likely from Peru?

Micropogon opercularis (Quoy and Gaimard). One 145 mm without locality likely from Patagonia? Drawing 164 mm of example from Rio Janeiro.

**Johnius deliciosus* (Tschudi). Three 188 to 243 mm, one from Peru and two others without locality likely from same country?

**Johnius fasciatus* (Tschudi). Two 245 to 250 mm, one from Peru and other likely same?

Sciæna coroides (Cuvier). One 185 mm from Rio Janeiro.

**Sciæna dubia* Fowler and Bean. Fig. 57. Type 124 mm without locality may have been obtained in the Philippines?

Menticirrhus americanus (Linnæus). Drawing 165 mm of example from Rio Janeiro.

**Cirrimens ophiocephalus* (Jenyns). Fig. 58 (Peru). One 207 mm without locality may be from Chili? Drawing labeled as from Peru.

Eques acuminatus (Schneider). One 178 mm without locality may be from Rio Janeiro?

Sillaginidæ

Sillago sihama (Forskål). Four 62 to 238 mm, two from Singapore, one from Manila and one from Colombo, Ceylon.

Sillago ciliata Cuvier. Two 198 and 205 mm, one from Sydney and the other from Singapore.

Gerridæ

**Eucinostomus californiensis* (Gill). One 130 mm from Rio Janeiro.

**Eucinostomus melanopterus* (Bleeker). One 174 mm from Cape de Verde Islands.

**Gerres kappas* Bleeker. One 113 mm from Fiji.

**Gerres gigas* Günther. Four 83 to 180 mm, one each from Samoa, Tongatabu, Fiji and one without locality likely Polynesia?

Diapterus brasilianus (Cuvier). One 205 without locality likely from Rio Janeiro?

Mullidæ

**Upeneus arge* Jordan and Evermann. Two 275 to 303 mm, one from Hilo and the other without locality likely Hawaiian?

**Upeneus tragula* Richardson. One 138 mm from Sydney.

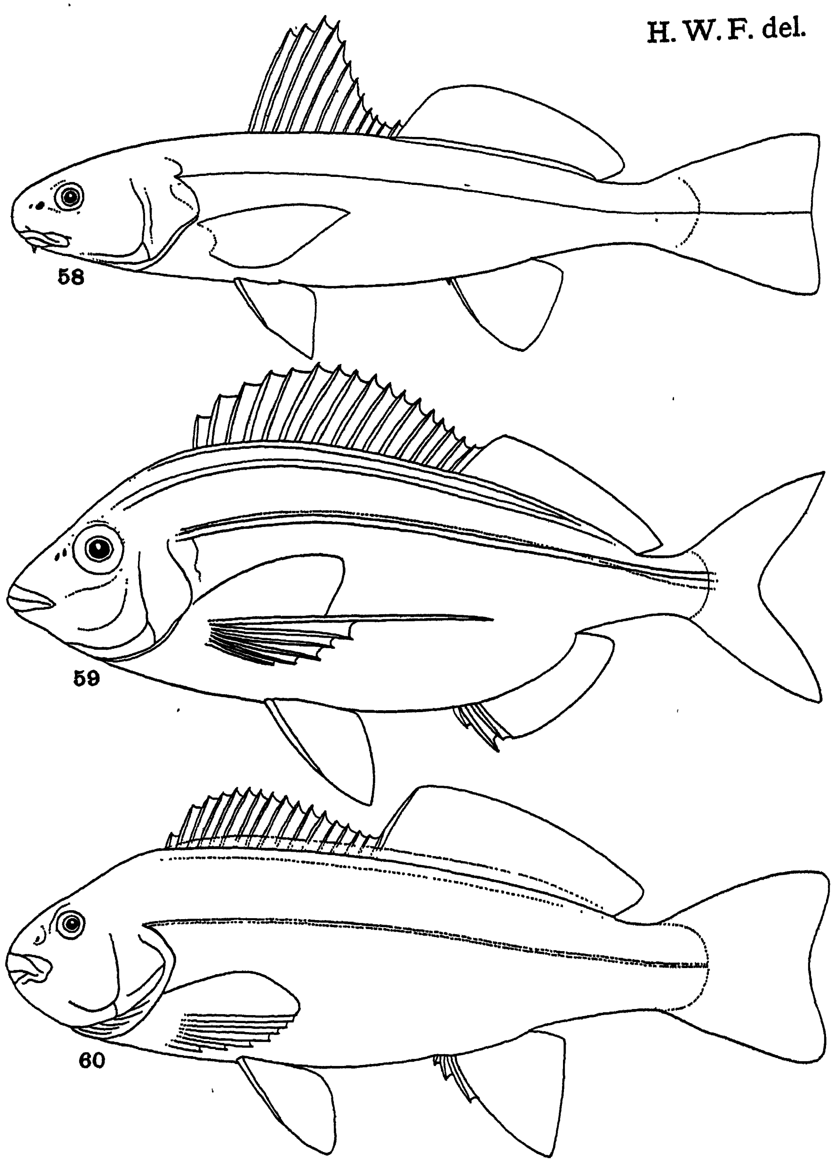
Upeneus sulphureus Cuvier. One 115 mm labeled "Singapore or Polynesia".

Upeneichthys vlamingii (Cuvier). One 160 mm from Sydney.

Pseudupeneus multifasciatus (Quoy and Gaimard). Two 209 mm, one from Maui and one from Tahiti.

Pseudupeneus pleurostigma Bennett. One 205 mm from Maui.

**Pseudupeneus jansenii* Bleeker. One 198 mm labeled Pacific? may be from the Philippines?



58. *Cirrimens ophiocephalus* (Jenyns).
59. *Sciaenoides macropterus* (Schneider).
60. *Aplodactylus punctatus* Valenciennes.

Pseudupeneus barberinus (Lacepède). Two 111 to 152 mm, one labeled "Singapore or Polynesia" and the other without locality may be from Fiji?

**Pseudupeneus porphyreus* Jenkins. Two 68 to 168 mm, one labeled "Singapore or Polynesia" and one without data, though all very likely Hawaiian.

**Pseudupeneus pleurospilos* (Bleeker). Colored engraving 220 mm long without data.

Pseudupeneus luteus (Valenciennes). Colored figure without data.

**Mulloidichthys samoensis* (Günther). Three 40 to 243 mm, one "Singapore or Polynesia," one without locality and one from Maui.

MALACANTHIDÆ

**Malacanthus hœdtii* Bleeker. One 280 mm from Maui.

CIRRHITIDÆ

Sciænoides macropterus (Schneider). Fig. 59. One 240 mm from Cloudy Bay, New Zealand.

Cheilodactylus variegatus Valenciennes. Three 220 to 248 mm, one from Callao and two labeled Peru.

Cirrhitus pinnulatus (Schneider). Four 114 to 253 mm, two from Oahu, one Maui and one without locality, last likely Hawaiian? Drawing 164 mm long by Richard of example from Pomotous.

Paracirrhites forsteri (Schneider). One from Fiji.

**Paracirrhites polystictus* (Günther). One 220 mm from Pomotou.

APLODACTYLIDÆ

Aplodactylus punctatus Valenciennes. Fig. 60 (Valparaiso). One 292 mm without locality may be from Peru? Drawing 195 mm long of example from Valparaiso.

EMBIOTOCIDÆ

**Tæniotoca lateralis* (Agassiz). Painting without data.

**Embiotoca jacksoni* Agassiz. One 145 mm from San Francisco, California.

**Amphistichus argenteus* Agassiz. One 103 mm from San Francisco.

**Hyperprosopon argenteus* Gibbons. One 76 mm from San Francisco.

**Hyperprosopon agassizii* Gill. Three 77 to 80 mm from San Francisco.

**Cymatogaster aggregatus* Gibbons. One 107 mm without locality likely from San Francisco?

**Holconotus rhodoterus* Agassiz. Colored engraving 220 mm long without locality.

Channidæ

Channa striata (Bloch). One 270 mm from Manila.

Scorpidæ

**Atypichthys strigatus* (Günther). Three 116 to 133 mm from Sydney. Drayton's sketch 125 mm long with date January 1840.

Platacidæ

Platax pinnatus (Linnæus). Head of large specimen labeled erroneously "South America," may be from Philippines?

Platax orbicularis (Forskål). Painting 185 mm long by Drayton of example from Lebukia, Fiji, June 1840.

Scatophagidæ

Scatophagus argus (Linnæus). One from Manila. Drawing by Richard 155 mm long.

Chaetodontidæ

Forcipiger longirostris (Broussonett). Drawing 113 mm long by Richard of example from Fiji.

Chætodon auriga Forskål. Three 55 to 180 mm without locality doubtless from Polynesia? Painting by Agate from a Peacock Island or Handon Island specimen. Drawing 140 mm long by Richard of example from Hull's Island and one 140 mm long also by Richard from Pomotous.

Chætodon ephippium Cuvier. One 25 mm without locality may be Hawaiian?

Chætodon lunula (Lacepède). Drawing 130 mm long from Oahu.

Chætodon unimaculatus Bloch. Painting 95 mm long by Drayton of example from Tahiti, September 1839.

Chætodon miliaris Quoy and Gaimard. Seven 84 to 110 mm from Maui.

Chætodon citrinellus Cuvier. Four 64 to 100 mm without locality, may be from Fiji or Samoa? Drawing 98 mm long by Richard labeled Tahiti.

Chætodon vagabundus Linnæus. Drawing 57 mm long of example from Fiji.

Chætodon trifasciatus Park. One 87 mm without locality doubtless from Polynesia? Painting of an example from Opolu, Samoa.

Chætodon striatus Linnæus. One 105 mm from Rio Janeiro.

Chætodon fremblii Bennett. Drawing 60 mm long by Richard of example from the "Sandwich Islands".

Microcanthus strigatus (Cuvier). Three 90 to 123 mm from Oahu. Drawing 33 mm long by Richard of example from Maui.

Heniochus acuminatus (Linnæus). Length 90 to 123 mm, three from Oahu and two from Fiji.

Centropyge bicolor (Bloch). One 110 mm from Manila? Drawing 110 mm long by Richard of example from Fiji.

Zanclidæ

Zanclus canescens (Linnæus). Four 60 to 125 mm, one Hawaiian and three from Samoa? Drawing 105 mm long by Richard from Pomotous.

Teuthididæ

Teuthis fuliginosus (Lesson). One 172 mm without locality likely Polynesia? Drawing 170 mm long by Richard from Oahu. Painting 230 mm long of specimen from Lebukia, Fiji. Also two 43 to 57 mm Hawaii.

Teuthis lineolatus Valenciennes. Drawing 150 mm by Richard from Tahiti.

**Teuthis leucopareius* Jenkins. One 83 mm without locality, and drawing 55 mm by Richard from "Sandwich Islands".

Teuthis bariene (Lesson). Two 43 to 57 mm from Hawaii.

Teuthis achilles (Shaw). Painting 235 mm by Drayton from Wake's Island labeled "Sydney Islands" August 25, 1840. Sketch 135 mm long by Drayton labeled Bellinghausen

Island September 1839. Drawing by Richard 175 mm from Pomotous and one 180 mm long from Hull's Island.

Teuthis triostegus (Linnæus). Painting of example from Raraka by Dougal. Drawing 130 mm long by Richard of example from Hull's Island, one same size from Fiji, and one 115 mm from Pomotous.

**Teuthis sandvicensis* (Streets). One 178 mm without locality evidently Hawaiian? Drawing 170 mm long by Richard of example from Oahu.

Ctenochætus strigosus (Quoy and Gaimard). Drawing 140 mm long by Richard of example from Maui.

Zebrasoma veliferum (Bloch). One 68 mm long from Samoa, and drawing 135 mm long by Richard from same locality. Also drawing by Richard 158 mm long labeled "Sandwich Islands".

Zebrasoma flavescens (Bennett). Drawing 78 mm long by Richard of example from Samoa and one 43 mm long from Fiji.

Naso lituratus Schneider. Drawing 190 mm long by Richard of example from Opolu, Samoa.

Naso annulatus (Quoy and Gaimard). Drawing 200 mm long by Richard of example from Fiji.

Naso unicornis (Forskål). One 73 mm long from Oahu. Sketch 235 mm long of example from Fiji, August 1840. Drawing 260 mm long by Richard of example from Tahiti and one 118 mm long from Maui.

SIGANIDÆ

Siganus rostratus (Valenciennes). One 203 mm from Fiji.

**Siganus vitianus* (Sauvage). Two 43 to 46 mm from Fiji.

Siganus argenteus (Quoy and Gaimard). One 55 mm from Fiji.

Siganus spinus (Linnæus). Five 85 to 150 mm, four from Fiji and one without locality. Drawing 123 mm long by Richard from Samoa and one 170 mm long from Fiji.

Siganus rivulatus (Forskål). One 143 mm from Sydney.

Siganus doliatus (Valenciennes). One 178 mm from Fiji.

SCORPÆNIDÆ

**Sebastosomus mystinus* (Jordan and Gilbert). Two 240 to 333 mm from northwest coast of North America, likely California or Oregon?

**Sebastomus rosaceus* (Girard). One 177 mm without locality, likely California or Oregon?

**Sebastodes chamaco* (Evermann and Radcliffe). One 215 mm from Callao.

Scorpænopsis gibbosus (Schneider). Drawing 208 mm long, unsigned, of specimen from Fiji.

**Sebastapistes bynœnsis* (Richardson). Four 35 to 55 mm from Fiji.

Scorpæna brasiliensis Cuvier. One 185 mm from Rio Janeiro.

**Scorpæna histrio* Jenyns. Four 63 to 114 mm, one from Callao, the others without locality likely from Peru?

**Scorpæna cruenta* Richardson. One 215 mm from Sydney.

Scorpænodes guamensis (Quoy and Gaimard). Painting 60 mm long by Drayton from reef at Tutuilla, October 16, 1839.

Pterois radiata Cuvier. One 168 mm without locality evidently from Polynesia? Drawing 97 mm long of specimen from Raraka.

HEXAGRAMMIDÆ

Hexagrammos decagrammus (Pallas). One 335 mm labeled "Pomotus" and one 235 mm labeled "Oahu" doubtless erroneous for California or Oregon?

COTTIDÆ

Cottus asper Richardson. Two 135 to 210 mm from the Columbia River, Oregon.

**Leptocottus armatus* Girard. Two 265 to 270 mm, one labeled "Callao" and another "probably Pacific" are doubtless from California or Oregon?

**Oligocottus maculosus* Girard. Three 50 to 73 mm from Puget Sound.

CARACANTHIDÆ

Caracanthus maculatus (Gray). Five 28 to 55 mm, two from Fiji and three from Tahiti.

TRIGLIDÆ

Prionotus punctatus (Bloch). Fig. 61 (Rio Janeiro). One 192 mm without locality likely from Rio Janeiro.

Chelidonichthys kumu (Lesson). One 377 mm labeled "Oahu" probably from New Zealand or Australia?

CEPHALACANTHIDÆ

Cephalacanthus volitans (Linnæus). One 140 mm from Rio Janeiro.

**Ebisinus cheirophthalmus* (Bleeker). Two 65 to 107 mm from lat. 28° S., long. 45° W.

CICHLIDÆ

Geophagus brasiliensis. (Quoy and Gaimard). One 122 mm from Rio Janeiro.

POMACENTRIDÆ

Amphiprion ephippium (Bloch). One 90 mm from Fiji. Drawing 88 mm long by Richard of Fiji example.

Dascyllus trimaculatus (Rüppell). Three 43 to 78 mm without locality likely from Polynesia? Drawing 98 mm long by Richard of Oahu example.

Dascyllus aruanus (Linnæus). Length 22 to 78 mm, one from Tonga and fourteen others without locality evidently Polynesia? Drawing 75 mm long by Richard of example from Samoa.

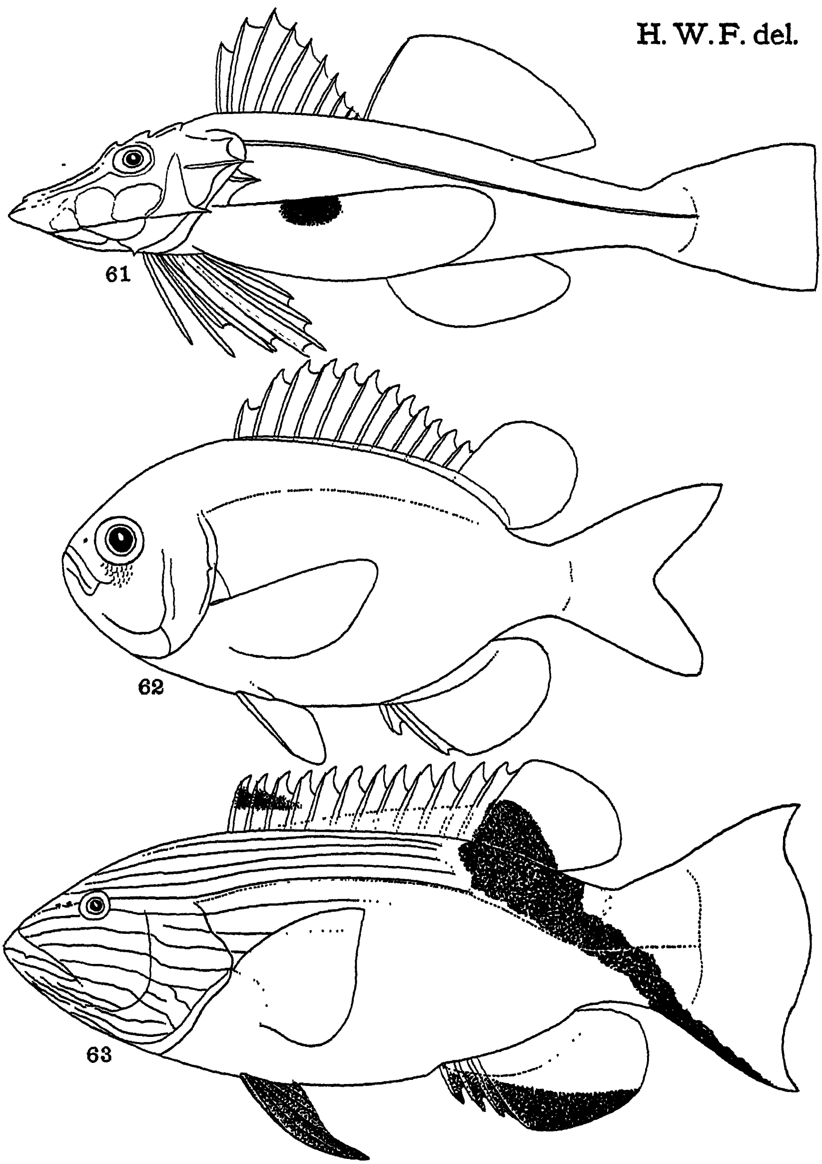
**Nexilosus latifrons* (Tschudi). One 196 mm without locality probably from Peru?

Chromis crusma (Valenciennes). Fig. 62. Drawing 160 mm long by Richard of example from Peru.

**Chromis cupreus* Fowler and Bean. Type 157 mm labeled "Singapore, Maui, Fiji or Samoa".

**Chromis ovalis* (Steindachner). One 143 mm without locality evidently Hawaiian? Drawing 144 mm long by Richard of Oahu specimen.

Chromis cæruleus (Cuvier). Four 28 to 100 mm without locality, likely from Fiji or Samoa? Drawing 55 mm long by Richard of Fiji specimen. Painting 58 mm long by Drayton of specimen from Fiji, May 1840.



61. *Prionotus punctatus* (Bloch).
62. *Chromis crusma* (Valenciennes).
63. *Lepidaplois hirsutus* (Lacepède).

Pomacentrus fuscus Cuvier. Drawing 125 mm long by Richard of example from Rio Janeiro.

Pomacentrus trilineatus Cuvier. Drawing 85 mm long by Richard of example from Samoa.

Pomacentrus lividus (Schneider). Five 30 to 107 mm of which two from Samoa, and others without locality likely from Polynesia? Drawing 103 mm long by Richard of example from Fiji.

**Pomacentrus jenkinsi* Jordan and Evermann. One 85 mm long without locality likely from Maui? Drawing 87 mm long by Richard of specimen from Maui.

**Pomacentrus albofasciatus* Schlegel and Müller. One 80 mm long without locality probably from Samoa or Fiji?

Abudefduf marginatus (Bloch). During 145 mm long by Richard of example from Rio Janeiro.

Abudefduf abdominalis (Quoy and Gaimard). Eleven 27 to 185 mm, of which 4 from Oahu and others without locality may be from Maui?

Abudefduf saxatilis (Linnæus). Drawing 90 mm long by Richard of example from Samoa.

Abudefduf sordidus (Forskål). Two 35 to 186 mm, one from Maui and other without locality also likely Hawaiian?

Abudefduf cœlestinus (Cuvier). Two 43 to 160 mm, one from Tongatabu and other without locality? Drawing 160 mm long by Richard of one from Pomotous.

Abudefduf uniocellatus (Quoy and Gaimard). One 32 mm labeled "Oahu" more likely from Fiji?

**Abudefduf imparipennis* (Vaillant and Sauvage). Two 54 to 143 mm, smaller from Maui and other without locality likely from same place? Drawing 57 mm long by Richard from the "Sandwich Islands".

Abudefduf biocellatus (Quoy and Gaimard). Sketch 55 mm long unsigned of specimen from Tahiti, September 1839.

Abudefduf leucopomus (Cuvier). Painting 55 mm long by Drayton of example from Tahiti, September 1839. Drawing of small examples from Fiji by Richard.

Abudefduf dickii (Liénard). Drawing 63 mm long by Richard of example from Fiji.

**Abudefduf amabilis* (De Vis). Two 51 to 60 mm without locality, likely from Fiji or Samoa? Drawing 60 mm long by

Richard of specimen from Fiji and another 55 mm long from same locality.

Abudefduf zonatus (Cuvier). Four 68 to 90 mm, one from Fiji and others probably from Samoa or Fiji?

LABRIDÆ

Lepidaplois bilunulatus (Lacepède). One 113 mm from Maui.

Lepidaplois hirsutus (Lacepède). Fig. 63. Type of *Lepidaplois trotteri* Fowler and Bean from Sertes Island, Pomotous, August 1839.

**Trochocopus unicolor* Günther. Drawing 225 mm long by Richard from "Beru".

Pseudolabrus celidota (Schneider). Five from New Zealand 99 to 205 mm, and painting by Drayton and Agate April 1840 is marked Bay of Islands and fish said to be very abundant. Also painting with same locality March 1840 by Drayton. Drawing 205 mm long by Richard from example from New Zealand.

Pseudolabrus miles (Schneider). One 172 mm from New Zealand.

**Pseudolabrus gymnogenis* (Günther). Three 42 to 250 mm from Sydney. Drawing by W. H. Dougal, January 1849. Drawing 230 mm long by Richard of example from Sydney.

**Pseudolabrus güntheri* Bleeker. Four 147 to 197 mm, of which three labeled Sydney, and painting by Drayton January 10, 1840. Drawings by Richard, one in 1858, one by Dougal January 1847.

**Labrichthys cyanotænia* Bleeker. One 127 from Samoa.

Anampses geographicus Valenciennes. One 192 mm from Pomotou. Drawing 190 mm by Richard from "Hillo".

Anampses cuvier Quoy and Gaimard. One 103 mm from Hawaiian Islands. Drawing 93 mm by Richard of example from Maui.

Anampses cæruleo-punctatus Rüppell. One 265 from Hull's Group.

**Achœrodus gouldii* (Richardson). One 463 mm from Sydney.

Epibulus insidiator (Pallas). Four 63 to 295 mm, of which three from Fiji and one from Samoa. Painting by Drayton dated June 1840, Lebukia, Fiji.

Hemigymnus melapterus (Bloch). Length 65 to 122 mm, one from "Hilo or Fiji" and two without locality likely Hawaii or Samoa?

Stethojulis strigiventer (Bennett). Drawing 80 mm long by Richard of example from Tongatabu.

Stethojulis axillaris (Quoy and Gaimard). Length 68 to 122 mm, eight from Oahu and Maui and two from Fiji. Drawing 123 mm long by Richard of specimen from Oahu.

Stehojulis albovittata (Bonnaterre). One 115 mm from Maui. Drawing 120 mm long by Richard of example from "Sandwich Islands".

Halichœres trimaculatus (Quoy and Gaimard). Length 48 to 53 mm, one from Fiji and another labeled "Fiji or Samoa".

Halichœres margaritaceus (Valenciennes). Drawing 75 mm long by Richard of example from Fiji.

**Halichœres ornatissimus* (Garrett). Two 103 to 115 mm from Oahu.

Halichœres argus (Schneider). One 75 mm without locality likely from the Philippines? Drawing 77 mm long by Richard of specimen from Fiji.

**Halichœres poeyi* (Steindachner). One 184 mm from Rio Janeiro.

Macropharyngodon geoffroy (Quoy and Gaimard). Drawing 80 mm long by Richard of example from Maui.

**Coris ballieui* Vaillant and Sauvage. Two 287 to 315 mm from Maui.

**Coris venusta* Vaillant and Sauvage. One 158 mm from Maui.

Coris gaimard (Quoy and Gaimard). One 208 mm from Fiji.

Coris flavovittata (Bennett). Length 98 to 330 mm, one from Oahu, one Maui and one Hilo. Painting 265 mm long by J. Drayton, Hilo Bay, caught by natives in net January 23, 1841.

Coris greenovii (Bennett). Drawing 80 mm long by Richard of specimen from Oahu.

Cheilio inermis (Forskål). Length 70 to 310 mm, three from Maui, five Oahu and two "Sandwich Islands". Drawing 236 mm long by Richard of example from Oahu.

**Thalassoma janseni* (Bleeker). One 203 mm from Fiji.

Thalassoma umbrostygma (Rüppell). Two 98 to 140 mm from Maui and Oahu. Drawing 106 mm by Richard on example from Maui.

Thalassoma purpureum (Forskål). One 322 mm from Pomotou, with two drawings. Sketch from on board the "Porpoise" at Lazuriff Island. Color sketch 218 mm from Honden Island, August 1839, by Drayton. Drawing 230 mm long by Richard of example labeled "Boumutha".

Thalassoma duperrey (Quoy and Gaimard). Length 110 to 197 mm, twelve, all Hawaiian, from Maui, Hilo and Oahu.

Thalassoma quinquevittatus (Lay and Bennett). Two 132 to 142 mm without locality, likely from Samoa or Fiji? Drawing 140 mm long by Richard on example labeled "Boumutha".

Gomphosus varius Lacepède. Drawing of 80 mm by Richard of Fiji example.

Gomphosus tricolor Quoy and Gaimard. Two 233 to 239 mm from Tahiti.

**Pseudocheilinus hexatænia* (Bleeker). Drawing 112 mm long by Richard of Maui specimen.

**Cirrhilabrus solorensis* Bleeker. One 55 mm from "Maui". Drawing 78 mm long by Richard of example labeled from "Maui".

Cheilinus bimaculatus Valenciennes. Four 90 to 98 mm from Maui. Drawing 98 mm long by Richard of Oahu example.

Cheilinus fasciatus (Bloch). One 200 mm labeled "Maui". Drawing 215 mm long by Richard marked Manila.

Cheilinus digrammus (Lacepède). Length 200 to 290 mm, two from Hilo and one from Fiji.

**Cheilinus unifasciatus* Streets. Drawing 205 mm long by Richard of example from Hilo.

Cheilinus chlorourus (Bloch). Length 85 to 230 mm, one from Fiji, one Pomotou, two Hilo, one "Hilo or Hawaii" and two "Samoa or Fiji". Four drawings by Richard.

Semachlorella bifer (Lay and Bennett). Drawing 98 mm long by Richard of example from Maui.

Novaculichthys tæniourus (Lacepède). One 160 mm from Hilo.

Iniistius pavo (Valenciennes). Two 184 to 245 mm from Maui. Drawing by Richard, 240 mm long of example from Maui.

Cymolutes lecluse (Quoy and Gaimard). One 135 mm from Hawaiian Islands.

Scaridæ

**Scaridea zonarcha* Jenkins. Two 65 to 98 mm from Oahu.

Leptoscarus viridescens (Rüppell). One 215 mm from Maui and one 240 mm from Oahu.

Scarus forsteri Valenciennes. Length 147 mm from Tahiti. Drawing 235 mm long of example from Fiji.

**Scarus caudofasciatus* (Günther). Drawing 142 mm long by Richard of example from Tahiti.

**Scarus jordani* (Jenkins). One 401 mm from Bellinghausen Island, Society Group.

Scarus dubius Bennett. Drawing 197 mm long of example from Tongatabu by Richard.

**Scarus dimidiatus* Bleeker. One 200 mm long from Tongatabu.

Scarus oviceps Valenciennes. Length 142 mm from Samoa.

Scarus bicolor Rüppell. Head 135 mm long from Pomotou.

Scarus pectoralis Valenciennes. Painting 255? mm long by Drayton of Fiji example, August 29, 1840.

Eleotridæ

Eleotris fusca (Schneider). Length 60 to 112 mm, three from Fiji and four without locality likely Samoa? Drawing 81 mm unsigned from fresh-water of Tahiti.

**Eleotris sandwichensis* Vaillant and Sauvage. Length 74 to 158 mm, one labeled Oahu, and five without locality doubtless Hawaiian. Drawing 156 mm long unsigned of example from Hilo.

Asterropterix semipunctatus Rüppell. One 30 mm from Maui.

Butis butis (Buchanan-Hamilton). Three 90 to 127 mm from Singapore.

Ophiocara macrolepidotus (Bloch). Three 145 to 187 mm from Fiji.

Gobiomorphus gobioides (Valenciennes). Two 117 to 148 mm without locality doubtless from New Zealand. Three drawings 63, 128 and 134 mm long, unsigned, from New Zealand. Drayton's painting 47 mm long from Bay of Islands, New Zealand, March 1840.

Gobiidæ

**Mugilogobius devisi* McCulloch and Ogilby. One 32 mm from Australia.

Awaous genivittatus (Valenciennes). Length 105 to 138 mm, one from Maui, four from Oahu.

Awaous stamineus (Eydoux and Souleyet). Length 95 to 260 mm, one from Manila, three Oahu, one Maui, one Hilo, one without locality.

Glossogobius giurus (Buchanan-Hamilton). One 218 mm from Manila.

Paragobiodon echinocephalus (Rüppell). Two 21 to 28 mm from Fiji. Painting 20 mm long by Drayton of example from Valui, Fiji, March 30, 1840.

**Garmannia hemigymna* (Eigenmann and Eigenmann). Two 28 to 30 mm from Rio Janeiro.

Cryptocentrus arabicus (Gmelin). One 140 mm without locality likely from Ceylon?

**Lentipes seminudus* Günther. Fig. 64 (''Sandwich Islands''). Length 63 to 84 mm, two from Maui and third labeled ''Manila'' doubtless Hawaiian. Drawing 75 mm long unsigned from ''Sandwich Islands''.

**Sicyopterus stimpsoni* (Gill). Length 67 to 119 mm, one from Oahu, three Maui and six Hilo. Drawing 152 mm long of Honolulu example, 1840.

**Sicyopterus tæniurus* (Günther). Series of 129, 30 to 95 mm from Tahiti.

Gobius fuscus Rüppell. Drawing 68 mm long unsigned of Tongatabu example, one 63 mm long of Fiji example, one 55 mm long of Maui example.

Gobius ornatus Rüppell. Drawing 74 mm long unsigned of Fiji example.

Periophthalmidæ

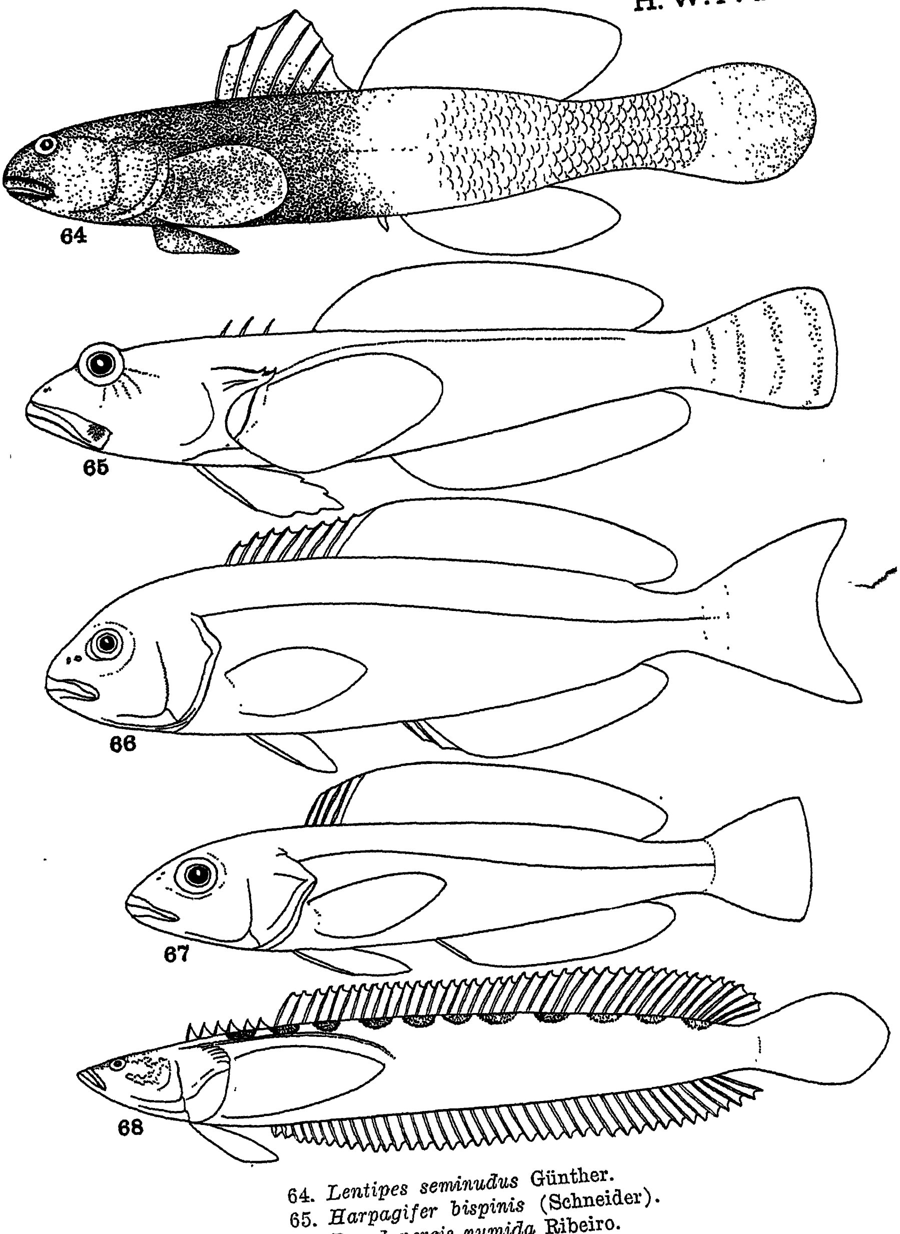
Periophthalmus koelreuteri (Pallas). Six 93 to 103 mm from Fiji.

Echeneidæ

Echeneis remora Linnæus. Length 73 to 153 mm, one from Hull's Sound, one Hull's Island, two in lat. 28° S. long. 45° W. and three from Cape de Verde Islands.

Percophidæ

Percophis brasiliensis Quoy and Gaimard. One 280 mm from Rio Janeiro.



64. *Lentipes seminudus* Günther.
65. *Harpagifer bispinis* (Schneider).
66. *Pseudopercis numida* Ribeiro.
67. *Pinguipes brasilianus* Cuvier.
68. *Gillellus australis* Fowler and Bean.

Parapercidæ

Parapercis hexophthalma (Cuvier). Three 95 to 230 mm, two from Fiji and one without locality.

Parapercis tetracanthus (Lacepède). Two 130 to 135 mm from Fiji.

Bovichthyidæ

**Cottoperca gobio* (Günther). Five 120 to 315 mm from Orange Harbor, Tierra del Fuego.

Nototheniidæ

**Notothenia cornucola* Richardson. Twelve 50 to 104 mm from Tierra del Fuego.

**Notothenia squamiceps* Peters. One 95 mm from Tierra del Fuego.

**Notothenia tessellata* Richardson. Nine 65 to 168 mm from Orange Harbor, Tierra del Fuego.

**Notothenia longipes* Steindachner. Five 100 to 130 mm from Tierra del Fuego of which one labeled Orange Harbor.

Harpagifer bispinis (Schneider). Fig. 65. Twenty-two 40 to 70 mm from Tierra del Fuego, of which all but two from Orange Harbor.

Latilidæ

**Pseudopercis numida* Ribeiro. Fig. 66. One 300 mm without locality doubtless from Rio Janeiro. Drawing 200 mm long by Richard without locality.

Pinguipes brasilianus Cuvier. Fig. 67. One 234 mm from Rio Janeiro.

Latilus jugularis Valenciennes. Length 115 to 168 mm, one from Valparaiso and two from Peru.

Dactyloscopidæ

**Gillellus australis* Fowler and Bean. Fig. 68. Types 68 to 87 mm from Valparaiso.

Acanthoclinidæ

Acanthoclinus littoreus (Schneider). Fig. 69. One 110 mm. Drawing 123 mm long unsigned and labeled ''Sydney''.

Blenniidæ

Forsterygion varius (Schneider). Eight 33 to 73 mm from New Zealand.

Myxodes viridis Valenciennes. Fig. 70. One 110 mm without locality doubtless from Valparaiso? Drawing 109 mm long unsigned and labeled Valparaiso.

**Labrisomus philippi* (Steindachner). Four 218 to 245 mm without locality likely from Peru? Two drawings 210 and 225 mm long, unsigned, and labeled Peru.

Labrisomus nuchipinnis (Quoy and Gaimard). One 147 mm from Rio Janeiro.

Labrisomus guttulatus (Valenciennes). Length 80 to 120 mm, two from Valparaiso, one other labeled "Fiji" which erroneous though it is also marked on the original unsigned drawing.

Labrisomus microcirrhis (Valenciennes). Fig. 71 (Peru). Seven 90 to 290 mm without locality. According to the original drawings four are from Valparaiso and one from Peru.

Hypleurochilus geminatus (Wood). One 45 mm without locality likely from Rio Janeiro?

**Hypleurochilus periophthalmoides* (Macleay). Three 14 to 18 mm from Sulu.

Blennius galerita Linnæus. Drawing 73 long, unsigned, Madeira.

Blennius pilicornis Cuvier. One 135 mm from Rio Janeiro.

Salarias alboguttatus Kner. Drawing 53 mm long unsigned from Fiji specimen.

Salarias marmoratus (Bennett). Ten 26 to 65 mm without locality, likely Hawaiian? A drawing is marked "Sandwich Islands", 53 mm long and unsigned.

**Salarias geminatus* Alleyne and Macleay. Two 105 to 138 mm from Fiji.

Salarias lineatus Valenciennes. One 95 mm from Fiji? Drawing 97 mm long unsigned of specimen from Fiji.

**Salarias caudolineatus* Günther. One 65 mm from Fiji.

Salarias fasciatus (Bloch). Two 78 to 79 mm from Fiji. Drawing 125 mm long unsigned of specimen from Fiji.

Salarias edentulus (Schneider). Length 69 to 118 mm, three

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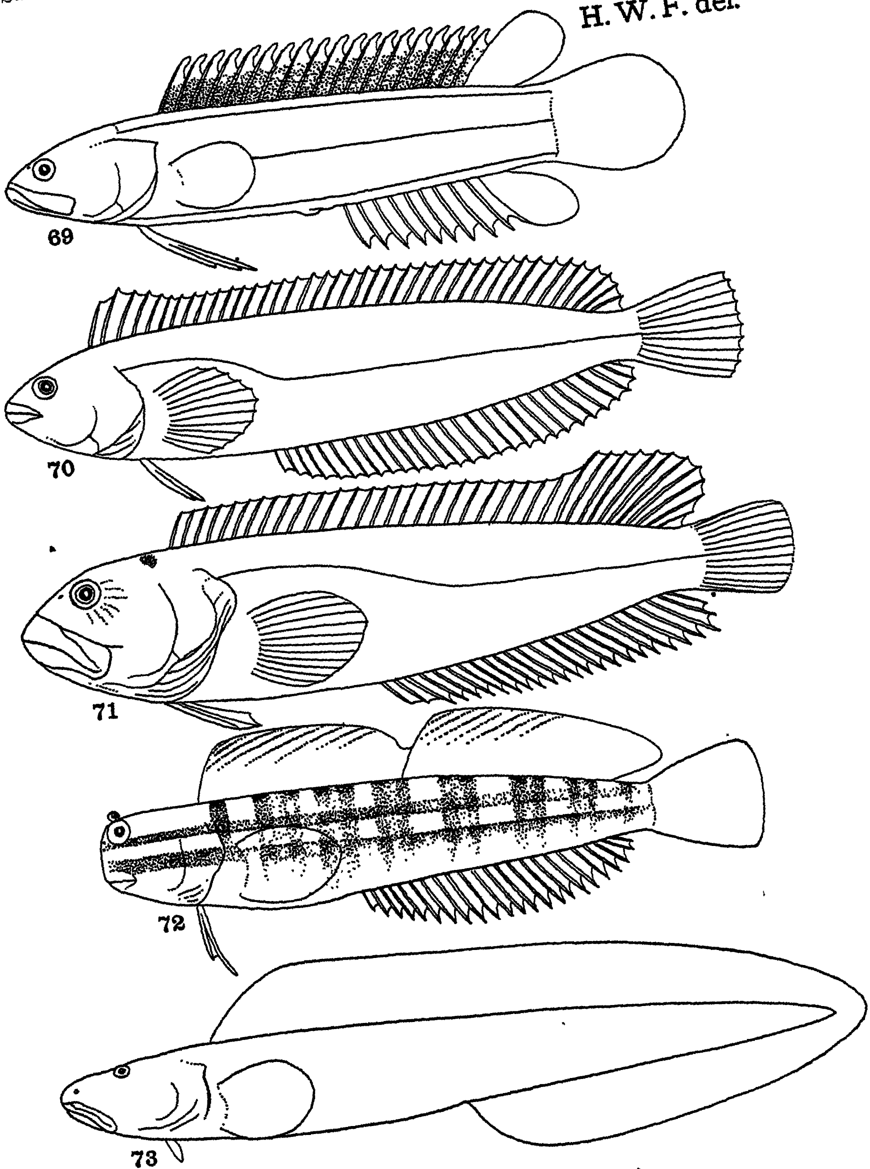


69. *Acanthoclinus littoreus* (Schneider).
70. *Myxodes viridis* Valenciennes.
71. *Labrisomus microcirrhis* (Valenciennes).
72. *Salarias mccullochi* Fowler and Bean.
73. *Austrolychus depressiceps* Regan.

from Fiji and one Balabac Passage. Drawing of young example 53 mm long marked Tongatabu.

Salarias fuscus Rüppell. Two 65 to 78 mm from Fiji. Drawing 48 mm long of example from Fiji.

**Salarias zebra* Vaillant and Sauvage. Three 42 to 71 mm from Maui.

Salarias viridis Valenciennes. Length 42 to 238 mm, two from Valparaiso, three Callao, three Peru and two without locality.

**Salarias mccullochi* Fowler and Bean. Fig. 72. Type 90 mm from Billinghausen Island.

Cirripectes sebæ (Valenciennes). Four 45 to 58 mm without locality possibly from the Philippines?

Emblemariidæ

**Emblemaria atlantica* Jordan and Evermann. Two 34 or 35 mm from Rio Janeiro.

Pholidæ

**Pholis ornatus* (Girard). Drawing 123 mm long unsigned from example obtained at Puget Sound.

**Xiphister mucosus* (Girard). Two 172 to 243 mm without locality probably from Oregon or California? Drawing 237 mm long unsigned and from Puget Sound example.

Zoarcidæ

**Austrolychus depressiceps* Regan. Fig. 73. Length 83 to 398 mm, twenty from Orange Harbor and three labeled Tierra del Fuego.

Batrachoididæ

**Porichthys notatus* Girard. Three 128 to 232 mm without locality likely from Puget Sound?

Nautopaedium porosissimum (Valenciennes). Four 73 to 85 mm from Rio Janeiro.

Gobiesocidæ

**Caularchus maeandricus* (Girard). Two 70 to 75 mm from Puget Sound.

**Gobiesox sanguineus* (Müller and Troschel). Length 89 to 197 mm, four from Peru and one from Valparaiso.

Balistidæ

Balistes carolinensis Gmelin. Sketch 113 mm long unsigned and without locality.

Balistes capistratus Shaw. Sketch of specimen 660 mm long unsigned from Lebukea, Fiji, June 1840.

Balistes chrysopterus (Schneider). One 135 mm from Fiji, also drawing same size and locality by Richard.

Balistes flavimarginatus Rüppell. Fig. 74. Drawing 218 mm long by Richard without locality.

Balistes vidua Richardson. Drawing by Richard labeled "Sandwich Islands".

Hemibalistes bursa (Lacepède). One 158 mm long from Fiji.

Balistapus aculeatus (Linnæus). Two from Fiji, one Samoa, one "Pacific" and one without locality, length 138 to 173 mm. Painting 175 mm long by Drayton from Karlesoff Island example, obtained September 3, 1839.

Balistapus rectangulus (Schneider). One 171 mm from Pomotous.

Balistapus undulatus (Park). Drawing 197 mm long by Richard from "Boston" and one 75 mm long without locality.

Melichthys buniva (Lacepède). Two 173 to 235 mm long from Pomotous. Sketch 220 mm long from example obtained at "Clermont Tonnier Island" in August 1839.

Monacanthidæ

Cantherines sandwichiensis (Quoy and Gaimard). One 234 mm from Tahiti.

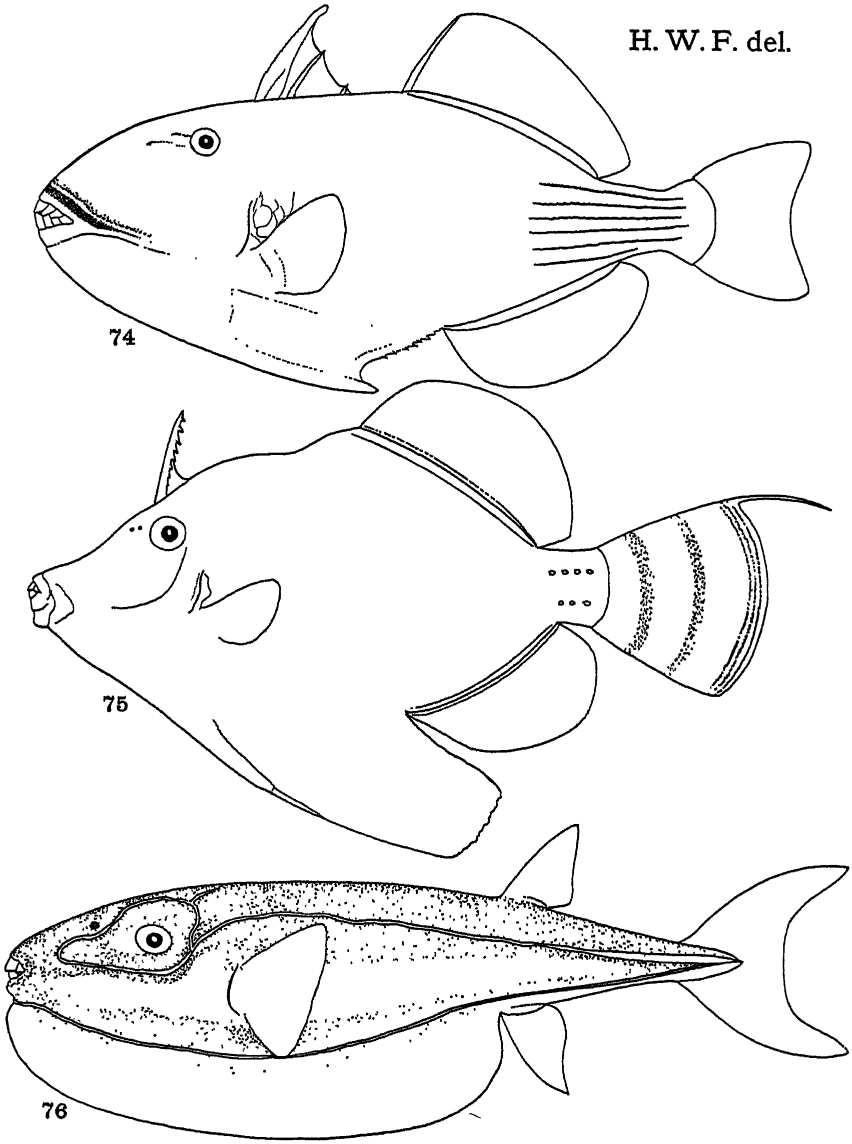
Monacanthus megalourus Richardson. Fig. 75. One 233 mm from Sydney. Drawing 172 mm by Richard of Sydney example.

Monacanthus ciliatus (Mitchill). One 58 mm without locality. Drawing 48 mm by Richard labeled "Atlantic Ocean".

**Monacanthus melanocephalus* Bleeker. One 48 mm without locality, likely from Polynesia? Drawing 60 mm long by Richard without locality.

Monacanthus hispidus (Linnæus). Two 59 to 86 mm from Rio Janeiro.

Monacanthus ayraudi (Quoy and Gaimard). One 87 mm from Sydney?



74. *Balistes flavimarginatus* Rüppell.
75. *Monacanthus megalourus* Richardson.
76. *Sphœroides scleratus* (Gmelin).

**Monacanthus trachylepis* Günther. Drawing 193 mm by Richard from Sydney example.

**Monacanthus rudis* Richardson. One 265 mm from Sydney.

Monacanthus spilosoma Bennett. Three 63 to 77 mm from Hawaiian Islands.

Alutera scripta (Osbeck). Drawing 258 mm by Richard from Honolulu specimen. Sketch by Drayton from Honolulu specimen dated November 1840.

Ostraciidæ

Ostracion cornutus Linnæus. Length 144 to 158 mm, one from Tahiti and one from Fiji.

**Ostracion sebæ* Bleeker. One 108 mm from Hawaiian Islands. Painting 155 mm long by Drayton on example from Lebukia, Fiji, May 1840.

Ostracion lentiginosum Schneider. Length 67 to 140 mm, two from Fiji and one Hawaiian.

Ostracion diaphanus Schneider. One 314 mm from Hawaiian Islands.

**Ostracion fornasini* Bianconi. Length 38 to 64 mm, one Hawaiian and three without locality.

Canthigasteridæ

Canthigaster margaritatus Rüppell. Painting 65 mm long by Drayton of example from "Tutuhia", October 17, 1839.

**Canthigaster amboinensis* (Bleeker). Five 33 to 77 mm from Fiji.

Tetrodontidæ

Sphæroides scleratus (Gmelin). Fig. 76 (no locality). Drawing 265 mm by Richard without locality. Painting 270 mm long by Drayton, from specimen caught on hook at Sandalwood Bay, Fiji, July 16, 1840.

Sphæroides spengleri (Bloch). Two 90 to 125 mm from Rio Janeiro.

**Sphæroides hypselogenion* (Bleeker). Length 110 to 134 mm, one Hawaiian and two without locality likely from same place.

Sphæroides hamiltoni (Richardson). Two 88 to 115 mm from Sydney, and a third labeled "Sandwich Islands" may also be from Sydney?

Sphœroides marmoratus (Ranzani). Two 34 to 95 mm without locality doubtless from Rio Janeiro?

Tetrodon immaculatus Schneider. One 319 mm from Tongatabu. Sketch by Drayton gives date as May 1840.

Tetrodon hispidus Linnæus. Length 178 to 240 mm, one from Fiji and one from Hawaiian Islands. Sketch 255 mm long by Drayton gives "Gardners Gr. to near the line S. L. Long. 175° W." August 19, 1840.

Tetrodon nigropunctatus Schneider. Length 70 to 148 mm, one without locality and two from Oahu. Painting 155 mm long by Drayton of example from Karlesorff Island, Raraka, September 3, 1839. Sketch 178 mm long by Agate of example without locality obtained September 6, 1839.

Diodontidæ

Diodon holacanthus Linnæus. One 105 mm from Tahiti and one 150 mm from Oahu.

Cyclichthys spinosus (Linnæus). One 185 mm without locality probably from Rio Janeiro?

Antennariidæ

Antennarius commersonii (Lacepède). Two 31 to 33 mm from Maui.

THE FLIGHT OF ADMIRAL BYRD TO THE SOUTH POLE AND THE EXPLORATION OF MARIE BYRD LAND

CAPTAIN HAROLD E. SAUNDERS[1]

Construction Corps, United States Navy

(Read February 23, 1940, in Symposium on American Polar Exploration)

INTRODUCTION

AFTER the return of Lieutenant Charles Wilkes from the southern oceans, almost a century rolled by before the American flag was again carried to the Antarctic by an exploring expedition. In 1927 and 1928, when Byrd made his plans for extensive flying in the Antarctic, the region around the South Pole was still the largest unexplored area then remaining on the surface of the earth, but it was by no means the great unknown that it had been one hundred years before. Much of the coast line had been traversed, men had wintered there, and two separate parties had succeeded in pushing their way to the south geographical pole.

Just enough, in fact, was known of the Antarctic continent to arouse the enthusiasm and inspire the zeal of a young modern like Byrd. He had already proved that airplanes could fly the polar regions successfully. He felt, and perhaps rightly so, that the secrets of certain regions of the Antarctic could be unlocked only by airplane. He was confident, furthermore, that he could vastly extend the accomplishments of an exploring party and greatly enhance the value of their scientific achievements by the use of two modern developments denied to his predecessors—the radio and the aerial mapping camera.

Much has been written[2,3] of Byrd's plans and preparations, his journey south in 1928, and his establishment of a base in the Bay of Whales, very appropriately called "Little America," so

[1] Chief Cartographer of the First and Second Byrd Antarctic Expeditions.

[2] *The Work of the Byrd Antarctic Expedition, 1928–1930,* by W. L. G. Joerg, American Geographical Society, New York, 1930.

[3] *Little America,* by Rear Admiral Richard Evelyn Byrd, U. S. N. (Ret.), New York, 1930.

that little of it need be repeated here. Suffice it to say that his staff contained meteorologists to forecast flying weather and that with his largest airplane he could fly for over 1,200 miles[4] with four men and their equipment up to altitudes of 11,000 feet. He could from his airplanes carry on two-way radio transmission with all members of his party, whether they were at Little America or in the field, and he could take motion pictures and aerial survey photographs.

The Flight to the South Pole

While Byrd was intent primarily upon scientific investigation and geographical discovery, as shown by his numerous flights in the early months of 1929, by his discovery of the Rockefeller Mountains toward King Edward VII Land, and by the occupancy of a base in those mountains by Gould in March of that year, he was also keenly interested in flying over the Queen Maud Range and the Polar Plateau and reaching the South Pole by air.

His preparation for this flight of approximately 1,600 miles to the Pole and return embodied, among a multitude of technical details, the safeguarding of his flying weather and his fuel supply. The first was based upon forecasts made by Haines at the Little America weather station where pilot balloons could be sent up to the upper atmosphere.[5]

These weather data were supplemented by weather reports from Gould,[6] then on a sledge journey to the Queen Maud Range, and only some 100 miles from the edge of the Polar Plateau. The second involved a preliminary base-laying flight to the Queen Maud Range, to lay down a fuel supply which would suffice for the last leg of the Polar Flight.

This base-laying trip, undertaken on November 18, 1929, with the Ford tri-motored airplane which was to make the Polar Flight, made it possible to check fuel consumption and airplane performance and to establish a suitable temporary landing field on the Ross Shelf Ice near the mountains.

[4] All distances are given in statute miles.

[5] Haines, the senior meteorologist of the expedition, developed the argument, found by experience to be correct, that good flying weather would exist when both surface and upper winds were of moderate velocity, and when the general direction of these winds was the same, blowing from the south and southeast.

[6] Dr. Laurence M. Gould, Senior Scientist and Second in Command of the First Byrd Antarctic Expedition.

With all preparations completed, and with good flying weather predicted, Byrd, Balchen, June and McKinley took off from Little America at 3:29 P.M. on November 28, 1929. The time of day was selected so that the sun would be on the beam going and returning, to facilitate reading the sun compass from a window in the side of the airplane. When approaching the Pole, the sun would be about south, and the lines of position from solar observations would run east and west, an advantageous direction for determining the latitude.[7]

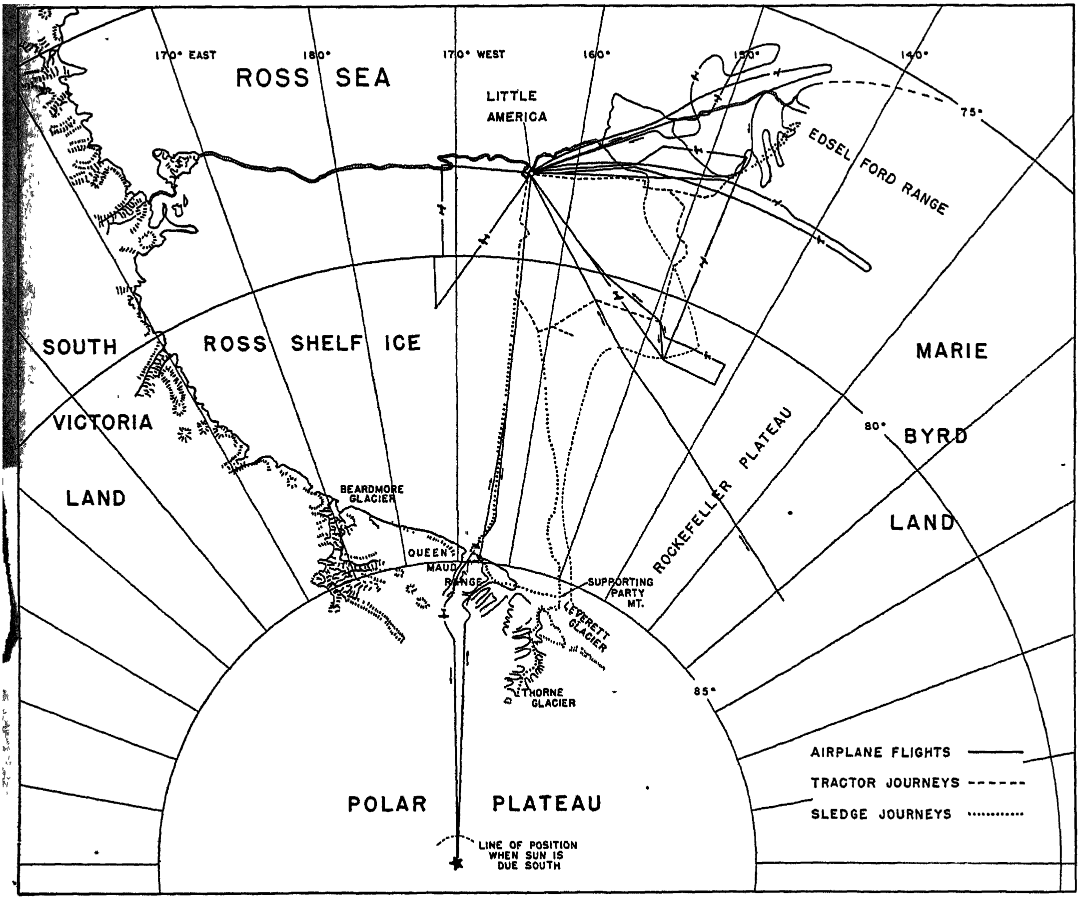


FIG. 1. The flights and journeys of the Bryd Antarctic Expeditions, 1928–1930 and 1933–1935. The map shows the sector between Long. 160° E. and Long. 130° W., extending from the South Pole to the Ross Sea, in the part of the Antarctic continent nearest New Zealand. The Queen Maud Range, about 450 miles from Little America, forms here the boundary of the Polar Plateau. The area explored by Lieutenant Wilkes is to the west (to the left) of that shown here.

[7] One such line of position is shown near the South Pole in Fig. 1.

Following generally the meridian of 163° 45′ W., Byrd and his companions passed over the Geological Party at about 8:30 P.M., where they were able to drop by parachute photographs of the Queen Maud Range made on the base-laying flight ten days before. They then headed somewhat west of south, and reached the foot of Liv Glacier, just west of Mt. Fridtjof Nansen, at 9:15 P.M. They had by then climbed to about 9,000 feet, but

FIG. 2. Mt. Fridtjof Nansen, looking east over the Queen Maud Range. The airplane is here climbing up over Liv Glacier, flying toward the right in the photograph. To the left are the James Duncan Mountains and just over them may be seen the horizon of the Ross Shelf Ice. Mt. Nansen rises to a height of nearly 15,000 feet above sea level. *Photograph by Capt. Ashley C. McKinley.*

they needed 2,000 feet more height to get over the pass and onto the plateau.

After a half hour of fighting for altitude and control which none of the airplane crew are ever likely to forget, they climbed through the gusty and forbidding gorge between Mt. Nansen and Fisher Mt. and came out on the level Polar Plateau.

In the southwest and west, as they gained the plateau, they

could see the peaks of a new mountain range [8] and the farther summits of the high mountains fringing Beardmore Glacier.

In the east and to the southeast they could see the back of the Queen Maud Range and the front of the Thorvald Nilsen Range, illustrated here in Figs. 3 and 4 respectively. Arrayed before them were mountains which loomed large and imposing, even behind the foreground of a 9,000-foot pleateau.

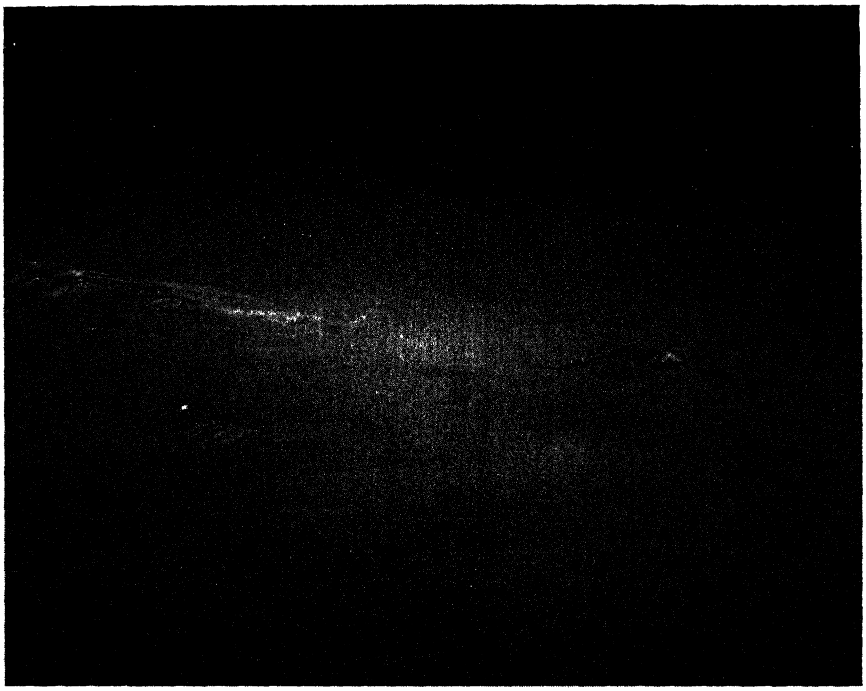


FIG. 3. The Queen Maud Range and the Polar Plateau. The long ridge-shaped mountain in the right center of the photograph is Mt. Don Pedro Christophersen and to the right of it the peak of Mt. Alice Wedel-Jarlsberg is just visible. The long slope to the left is the southern side of Mt. Nansen. Filled crevasses in the Polar Plateau appear in the foreground. *Photograph by Capt. Ashley C. McKinley.*

Within an hour they passed what appeared to be low hills to the southeast of the Thorvald Nilsen Range, which we now know to be the mountains at the head of Thorne Glacier, and they looked down upon the crevasses of what Amundsen so aptly termed "The Devil's Ballroom".

For two hours more they flew on to the south over a flat,

[8] Later named the Gilbert Grosvenor Range.

limitless plateau, unbroken even at the South Pole by the slightest hillock; see Fig. 5. They found nothing to distinguish the bottom of the world save a horizon of white snow, as vast and as level as that of the sea.

After checking his navigation and flying some additional miles south, east and west to cover possible errors in determin-

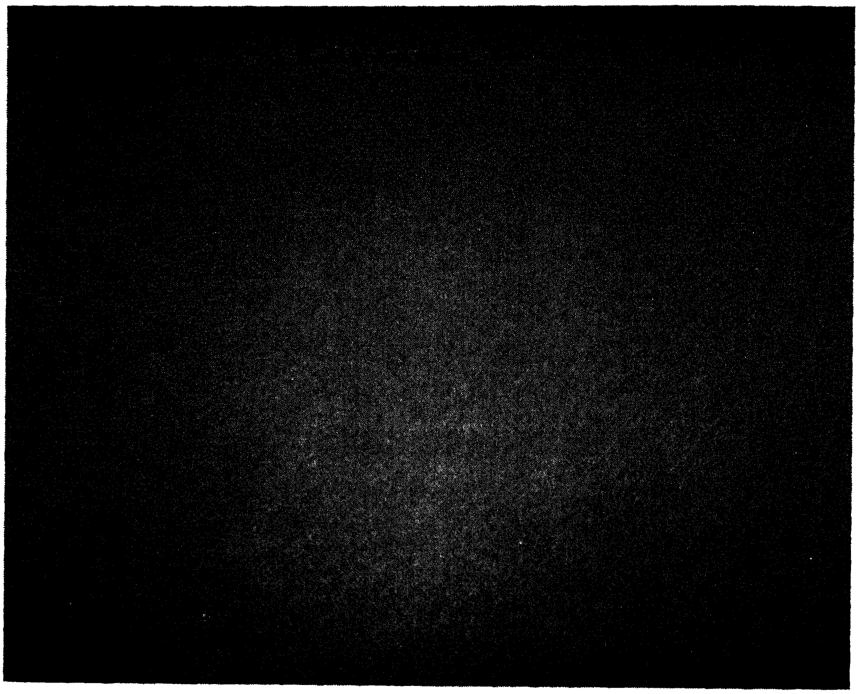


FIG. 4. The Thorvald Nilsen Mountains, looking east across the Polar Plateau. The long, rugged range at the right is the Thorvald Nilsen Mountains. The slight depression in the surface of the plateau near the left side of the photograph is the head of Amundsen Glacier. To the right of the range, below distant mountains obscured by clouds, are the crevasses of "The Devil's Ballroom." At the extreme right may be seen Mt. Katharine Paine, which lies beyond the head of the Thorne Glacier, over 80 miles away. *Photograph by Capt. Ashley C. McKinley.*

ing his position, Byrd turned north again at 1:25 A.M. on November 29 and headed for Mt. Nansen, flying on a meridian about two degrees to the eastward of his southern route and passing the Thorvald Nilsen Mountains closer aboard.

From the time that they approached the Queen Maud Range on the way south, McKinley [9] had been making a complete photo-

[9] Captain Ashley C. McKinley, Aerial Photographer and Third in Command of the First Byrd Antarctic Expedition.

graphic record of the country with his mapping camera. To the east low clouds obscured the mountains at the edge of the Polar Plateau from time to time but he obtained a series of photographs, some 250 in all, with large degrees of overlap, showing all the principal and minor landmarks to excellent advantage.

When he was flying 2,000 feet in the air above the Pole, Byrd was not only in continuous communication with his base, but the messages sent from the airplane telling of his achievement were

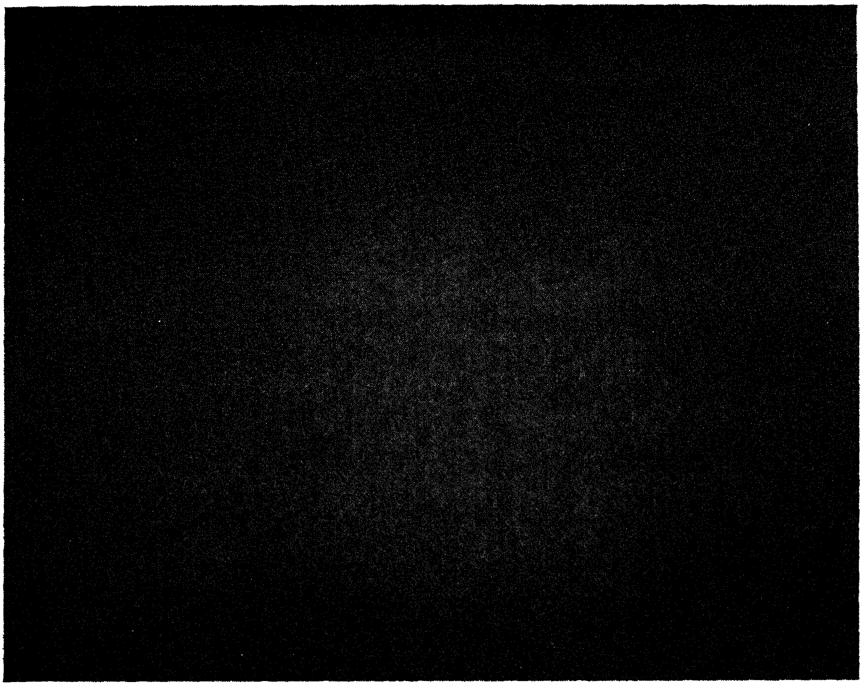


FIG. 5. The South Pole as seen from the air. Of all the photographs taken in the vicinity of the South Pole, none of them show very definitely the flat horizon where snow meets sky. *Photograph by Capt. Ashley C. McKinley.*

received direct by radio stations in the United States. Although hundreds of miles from Little America and from his field party, his mechanics and his ground crews could hear the constant hum of his engines as he flew mile after mile of his journey.

Returning to the Queen Maud Range and approaching it from the plateau side, it was easier for Byrd to judge the relative heights of the passes at the head of the large glaciers in this region. He selected the Axel Heiberg Glacier, to the east of

Mt. Nansen, and descended it to the Ross Shelf Ice, flying over the route made famous by Amundsen in 1911. The latter's famous landmark, Mt. Don Pedro Christophersen, shows to excellent advantage in Fig. 6.

From the foot of Axel Heiberg Glacier, Byrd turned east, cruising along the face of the range and looking for Amundsen's Carmen Land. A dwindling fuel supply forced him to turn back

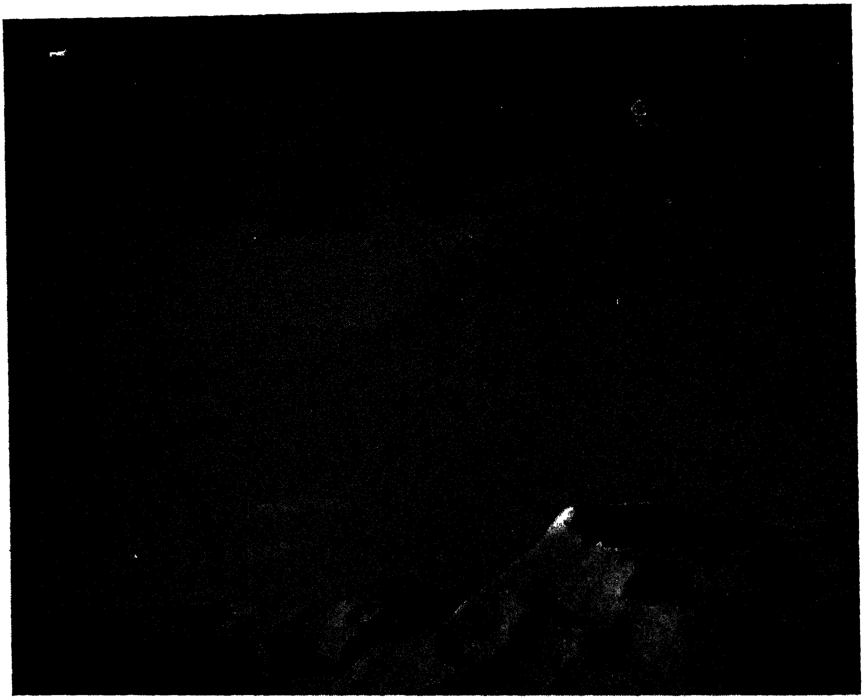


FIG. 6. The Queen Maud Range, from above Axel Heiberg Glacier. The high ridge-shaped mountain at the right is Mt. Don Pedro Christophersen as viewed from the northwest. In the center may be seen a mountain rising above the Polar Plateau and on the left are the peaks of Mt. Ruth Gade and Mt. Alice Wedel-Jarlsberg.
Photograph by Capt. Ashley C. McKinley.

to the westward at about 158° W., at the mouth of Amundsen Glacier, after having flown approximately 30 miles to the eastward.

On this swing to the eastward, Byrd had not seen anything more than the rugged Queen Maud Range, extending on and on to the southeast, and the relatively low Harold Byrd Mountains, which were later to be visited by both Gould and Blackburn.

An uneventful flight of less than four hours after landing and refueling at the Josephine Ford Fuel Depot saw him back at Little America.

From this flight Byrd brought back the honor of having first carried the flag of any nation to both poles, and of having first flown over both ends of the earth by airplane. However, of far more importance to the world and lasting value to science, he came back with the knowledge that vast stretches of rugged unknown country could be safely flown with the proper preparation, and he brought back the information necessary to correlate all other discoveries in this region. Strangely enough, these include not only the previous discoveries by other explorers, and the more recent work of his own men, but the work of many others who will tramp these wastes and fly these routes in the years to come.

An interesting case in point is represented by one aerial photograph made by McKinley on the Polar Flight from a point near the mouth of Amundsen Glacier, reproduced here as Fig. 7. After the return of the Second Byrd Antarctic Expedition to the United States, and an examination of the admirable photographs taken by Blackburn [10] and his party on their Thorne Glacier trip in late 1934 and early 1935, preparatory to making new maps of this region, it was discovered that a certain distinguished-looking mountain on a number of Blackburn's photographs appeared also on one aerial photograph of the more than 250 constituting the Polar Flight series.

That this mountain should appear on two sets of photographs seems not surprising, since both parties were photographing in the same range. But Byrd was photographing from the air; Blackburn from the ground. Blackburn passed the mountain close aboard; see Fig. 8. Byrd's farthest east on the Polar Flight was many miles from it. Remembering that Amundsen had had great difficulty with identification of his mountains, and that Mt. Don Pedro Christophersen had four different names in his English and Norwegian reports and on his published photographs, it was indeed a most valuable aid to make positive identification of Mt. Darryl Zanuck, the name later given to this landmark.

[10] Quin A. Blackburn, leader of the Queen Maud Range Geological Party of the Second Byrd Antarctic Expedition.

It should be emphasized particularly that one of the most remarkable features of the mapping work on the Queen Maud Range following the return of the Second Byrd Antarctic Expedition in 1935 is the wealth of additional information gained by a second analysis of the photographs taken by the first expedition in 1929 and 1930.

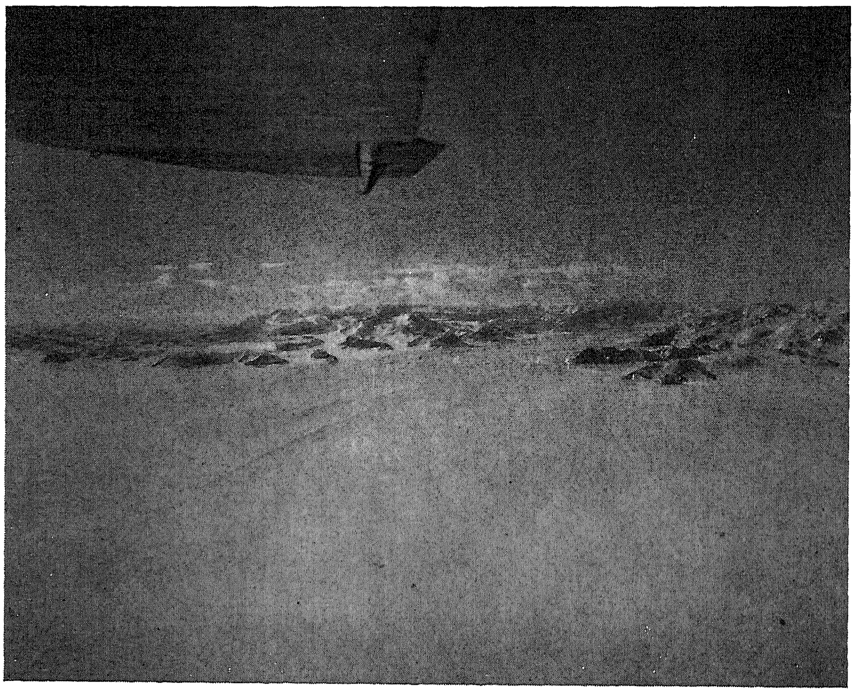


FIG. 7. Amundsen Glacier and the Queen Maud Range. This photograph was taken toward the south, near the point at which Byrd reached his farthest east position on the Polar Flight. In the center of the photograph is the Amundsen Glacier, one of the longest glaciers in the world, extending from the Ross Shelf Ice to the Polar Plateau. At the extreme left of this particular photograph may be seen a large mountain with three separate peaks in its ridge, lying beyond the Thorne Glacier.
Photograph by Capt. Ashley C. McKinley.

It may confidently be expected that the group of more than 300 aerial photographs taken by McKinley on the Base-Laying Flight and on the Polar Flight will, like the tailings of a placer mine, continue to yield a measure of gold every time they are worked over by improved methods and whenever they can be correlated to additional explorations of the future.

FIG. 8. Mt. Darryl Zanuck, looking south. Mt. Darryl Zanuck was photographed by Blackburn in 1934 from a point near the confluence of Phillips Glacier with Thorne Glacier. *Photograph by Quin A. Blackburn.*

The Flight to Marie Byrd Land

Not content with his remarkable flight to the South Pole, which still stands out in an epoch of history-making in the air, Byrd set off a week later on a second long flight of exploration, traveling far to the northeast of Little America. This flight, while yielding vastly more in the way of geographical discovery, has hardly attained the recognition it deserves; it was too greatly overshadowed by the publicity attending the flight to the South Pole.

With Haines again predicting good flying weather, Byrd, Parker,[11] June, and McKinley took off in the tri-motored Ford airplane on the morning of December 5, 1929, intending to travel far beyond the then known limits of King Edward VII Land to the northeast. This region had previously defied all the attempts of other explorers, and of themselves, to get "eastward beyond the horizon".

As shown in Fig. 9, they flew along the edge of the Ross Shelf Ice to Okuma Bay, thence over King Edward VII Land, passing Scott's Nunataks and the Alexandra Mountains on their starboard hand. They continued to the northeast over Arthur Sulzberger Bay, where for 35 miles they flew over open water. After less than 50 miles of flying past Scott's Nunataks, which represented the eastern outposts of land as previously known, they were rewarded by actual sight of what they had often viewed in their imagination—a long range of mountains extending across their path. Looming up out of the haze, Marie Byrd Land and the Edsel Ford Range reached from the coast line to the east and north of them in an irregular line as far as they could see to the south.

For nearly three hours they flew up and back along the front of this range, photographing its peaks and valleys, snow fields and glaciers. Fig. 10 is taken from the series made at this time. They circled over the frozen sea to photograph the numerous "ice islands" off the coast in this vicinity, then cut south and west back over King Edward VII Land, behind the Alexandra Mountains, to La Gorce Mountain, which had been discovered on previous flights to the Rockefeller Mountains. From there they headed north and west to Okuma Bay and thence back to Little

[11] Captain Alton B. Parker, airplane pilot.

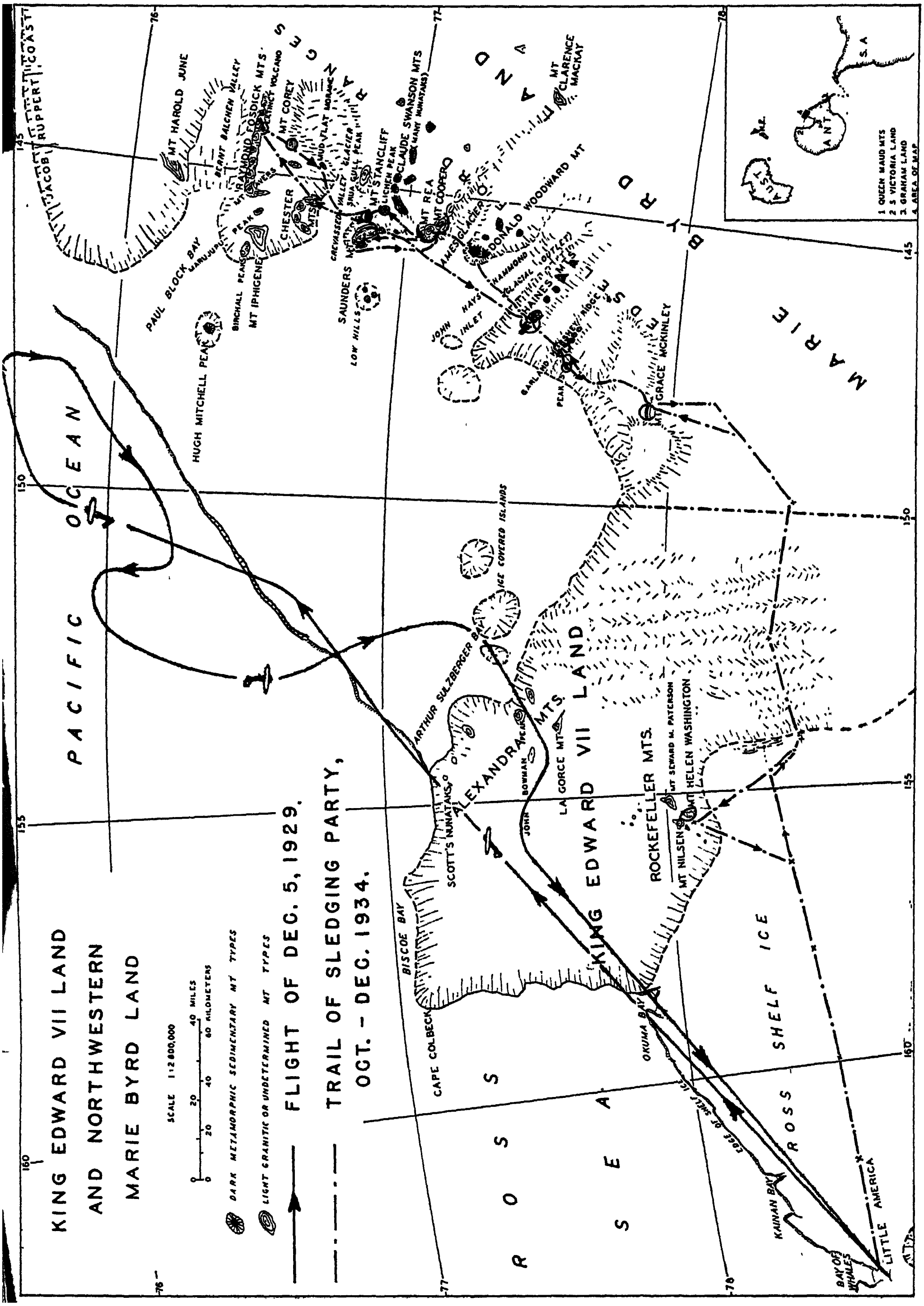


FIG. 9. Exploration of Marie Byrd Land. On this map are indicated Byrd's flight of December 5, 1929 (full lines) and Siple's sledge journey of 1934 (dot and dash lines).

America, finding the Ross Sea in this region clear of ice to the visible horizon.

From this flight Byrd brought back the story and the pictures of an entirely new land—a land and a massive mountain range added to the map of the known world in less than eight hours of flying.

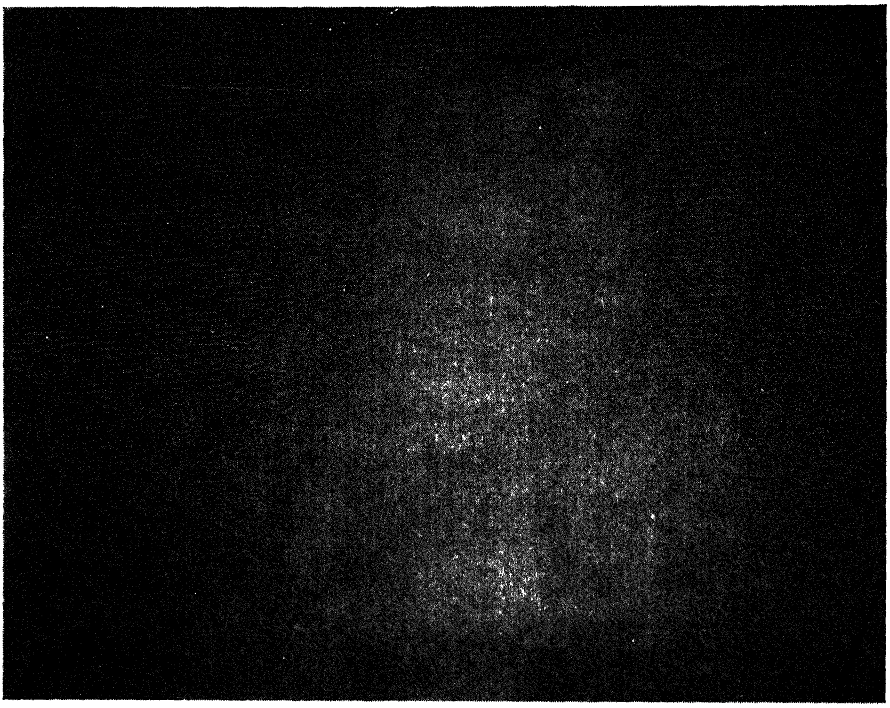


FIG. 10. The Edsel Ford Ford Range in Marie Byrd Land. At the left is Mt. Harold June, and to its right Bernt Balchen Valley. The range in the center is the Raymond Fosdick Mountains, and to its right is the Adolph Ochs Glacier. The snow-covered peak at the right is Mt. Iphigene.

Photograph by Capt. Ashley C. McKinley.

The presence of ice islands and large quantities of sea ice off the north shore of Marie Byrd Land, as well as the difficulty of depicting the shore line accurately from the 1929 photographs, led Byrd to explore farther afield in this region on his second expedition. A flight to the northeast on December 15 and 16, 1934, was turned back at Mt. Harold June [12] because of clouds

[12] Named for Harold I. June, airplane pilot of both first and second expeditions.

but Pelter[13] was able to obtain some excellent photographs of the Edsel Ford Range as a whole, showing a multitude of peaks interspersed with snow reaches, crevassed valleys and glaciers; see Fig. 11.

Another flight on November 18, 1934, along the parallel of 78° S., revealed that the high plateau back of Mt. Grace Mc-

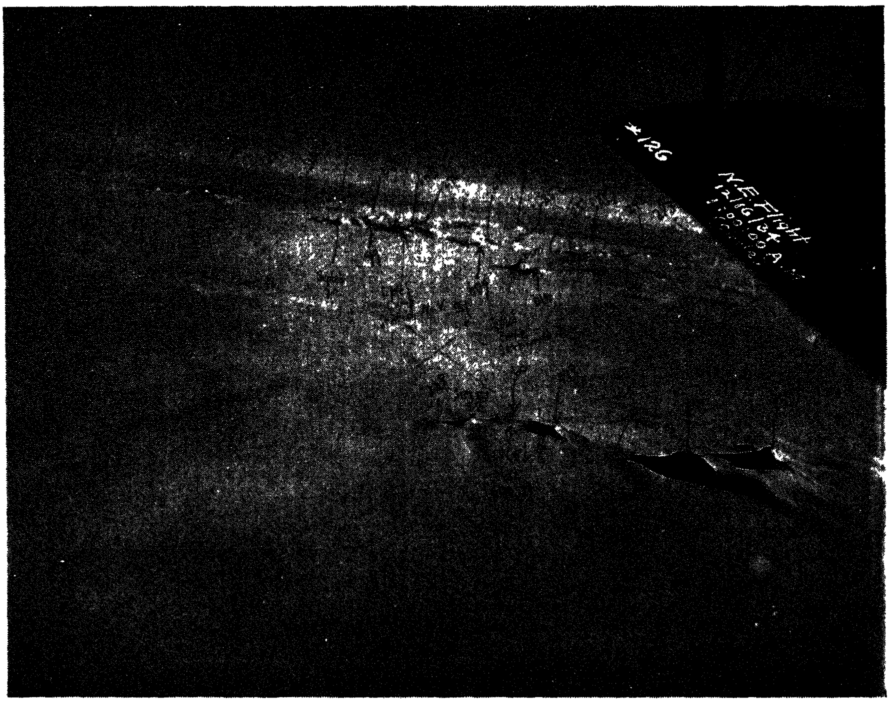


FIG. 11. The Edsel Ford Range, looking northeast. In the right foreground are the Haines Mountains and beyond them, on either side of the mountain marked 99, is the John Hays Hammond Inlet. This photograph shows the identification numbers and marks added by the map maker in the course of his analysis.

Photograph by Joseph A. Pelter.

Kinley extended far to the south and east in Marie Byrd Land. Two solitary peaks rising over the edge of this plateau, Mt. Hal Flood and Mt. Mabelle Horlick Sidley, beckoned on the men of another expedition who could lay down a base of operations nearer than Little America.

At this point it may be of interest to describe another simple

[13] Joseph A. Pelter, aerial photographer of the Second Byrd Antarctic Expedition.

but useful feature of exploration flying developed by the Second Byrd Expedition. This was known as airplane sounding—a method of obtaining elevations of the surface below the airplane track without the necessity of special observations or the risk of landing in rough and crevassed areas. At intervals the plane would descend to within a few feet of the surface, skim over it long enough to make a record on the meteorograph, then rise again to the desired flying altitude.

It was in this manner that Byrd succeeded in establishing an approximate contour line representing the eastern edge of the

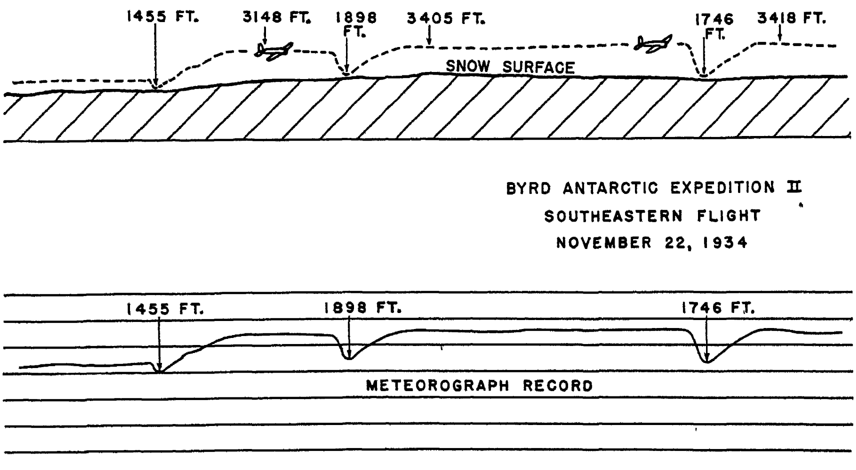


FIG. 12. Surface sounding by airplane. This diagram was developed from an actual meteorograph record during a sounding flight of this kind. A trace of the instrument record itself appears at the bottom.

Ross Shelf Ice, about which little was formerly known. This 300-foot contour, or "shore line," as it has conveniently been called, runs close to Mt. Helen Washington in the Rockefeller Mts., and then in a south and southeasterly direction all the way to the Queen Maud Range.[14]

No recital of Byrd's discoveries would be complete without at least a brief account of the five major sledge and tractor expeditions and without a tribute to the fortitude and ability of his men who made them. Their accomplishments were none the less valuable because they were less spectacular than the airplane

[14] This so-called "shore line," which runs from the Queen Maud Range to the Ross Sea, is shown as a broken line at the right of Fig. 1.

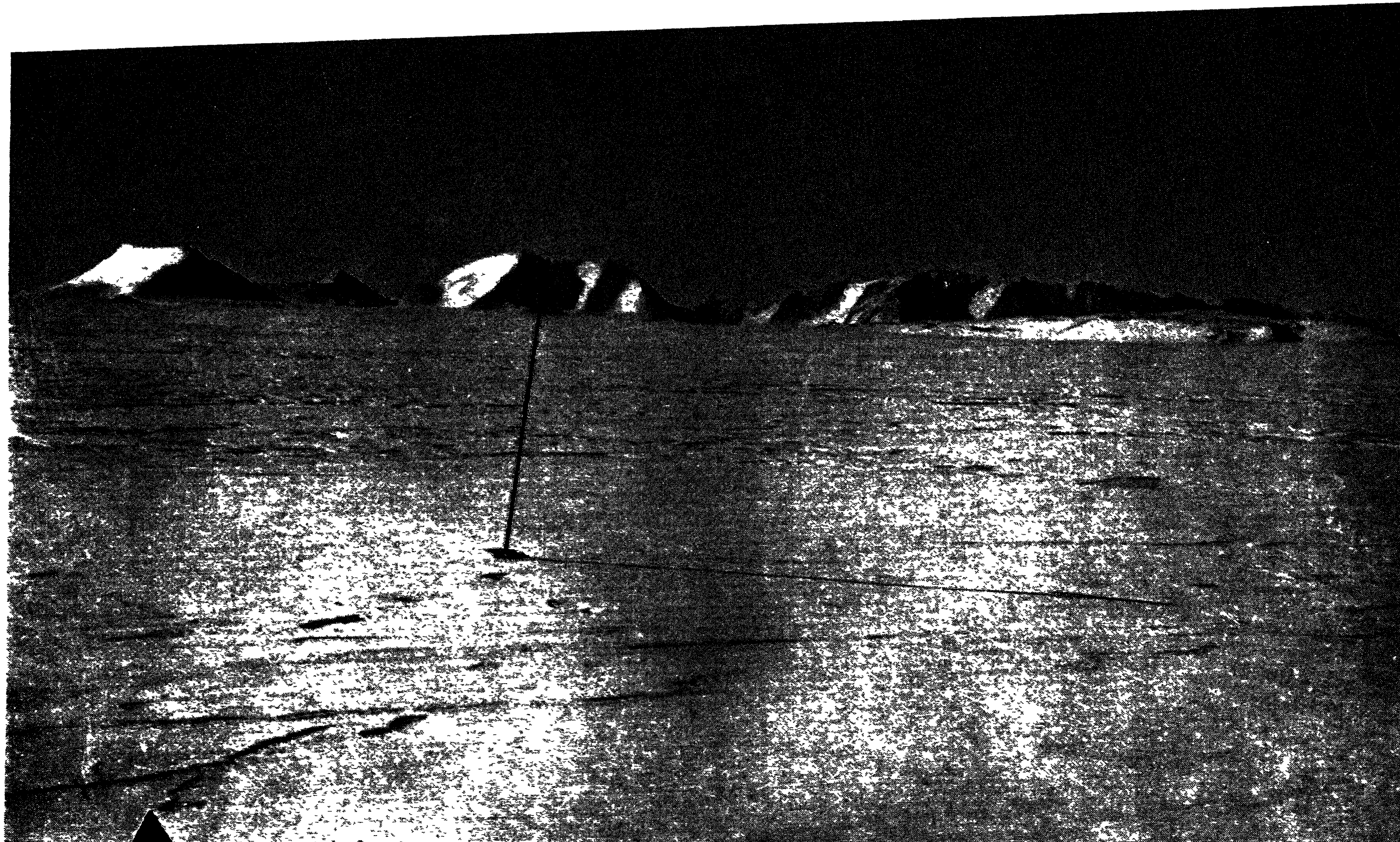


FIG. 13. The La Gorce Mountains at the head of Thorne Glacier. At the right of the photograph is Mt. Katharine Paine, the upper portion of which shows definitely on the aerial photographs taken by Byrd on the way to the South Pole, one of which is reproduced as Fig. 4.

Photograph by Quin A. Blackburn.

flights. In truth, without the ground control established by the surface parties as a background, many of the rolls of aerial survey film would be little more than strips of pretty pictures.

Gould's Geological Party of 1929 sledged to the Queen Maud Range, traveled many miles along and among the foothills of this range, from Liv Glacier to beyond Thorne Glacier, representing some 20 degrees of longitude, and occupied a camp in Marie Byrd Land, at Supporting Party Mountain.[15] Gould photographed the face of the range from the shelf ice and from the top of Supporting Party Mountain he made a photographic panorama extending from Fisher Mountain on the west to far beyond the 140th meridian on the east.

Blackburn's Geological Party of 1934 journeyed to the Queen Maud Range by a slightly different route.[15] They visited Gould's camp in Marie Byrd Land; then proceeded up the entire length of Thorne Glacier and climbed Mt. Weaver, a mountain seamed with coal at the edge of the Polar Plateau. From this vantage point they viewed and photographed that part of the Polar Plateau which had been flown by Byrd five years before. On their return they triangulated and took panorama photographs of the rugged mountains flanking both sides of Thorne Glacier.

In September of 1934 June and a party made a hazardous pioneering tractor trip of 500 miles to the east of Little America, where they gained the Rockefeller Plateau at an elevation of over 3,000 feet. They fixed the position of Mt. Grace McKinley, the principal ground control point in that region, and left a depot of supplies for Siple's sledge party.

Later in the season Bramhall[16] and another party made a long tractor journey to the south of Little America. Turned back in their attempt to reach the Queen Maud Range by the belt of crevasses at 81° S., they turned to the east and climbed the Rockefeller Plateau, then cruised north and west back to Little America, taking scientific observations and triangulation data and making seismic soundings through the ice.

From October to December 1934 Siple[17] took a sledge party past Mt. Grace McKinley, across the John Hays Hammond Inlet

[15] See Fig. 1.

[16] E. H. Bramhall, Physicist of the Second Byrd Antarctic Expedition.

[17] Paul A. Siple, member of both First and Second Byrd Antarctic Expeditions.

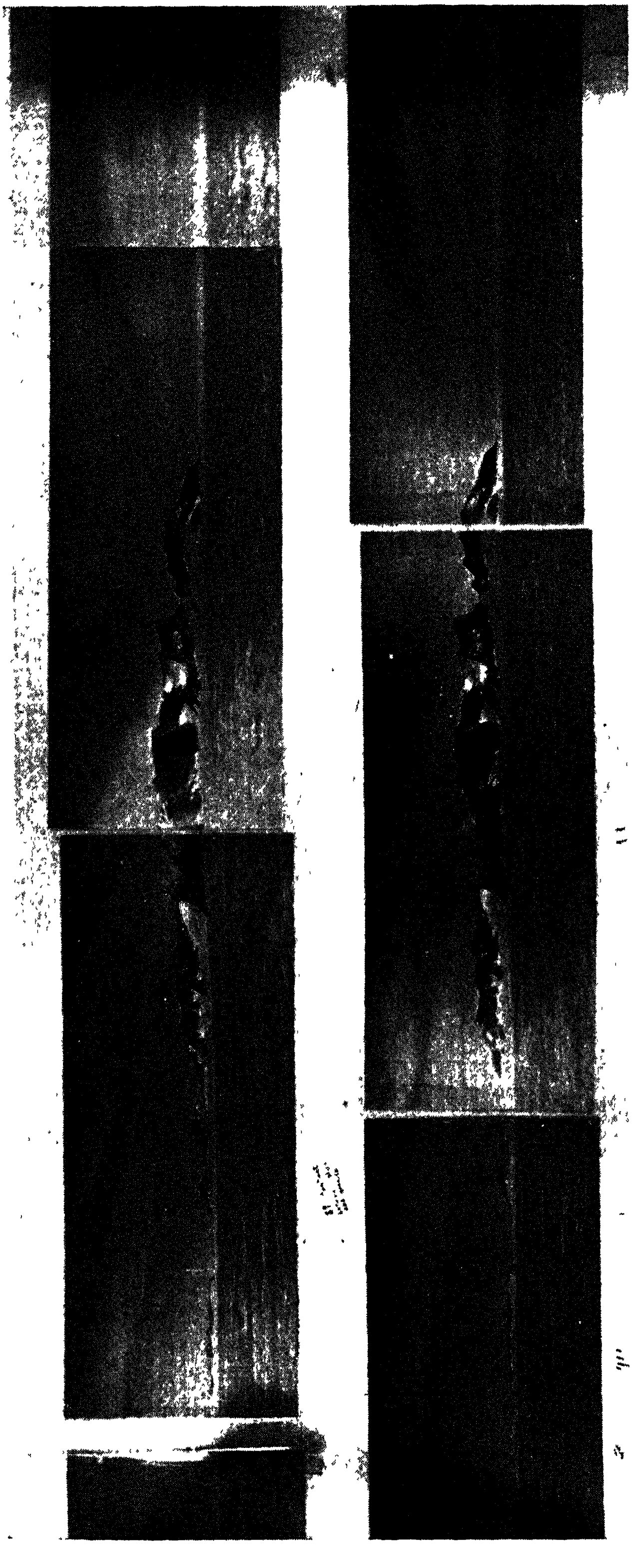


FIG. 14. Mt. Cooper and Mt. Rea in the Edsel Ford Range. This is a section of a 360-degree panorama made by Siple in the interior of the Edsel Ford Range, showing the Mt. Cooper-Mt. Rea group. *Photograph by Paul A. Siple.*

and through the western part of the Edsel Ford Range, reaching a point beyond the limits of the two previous airplane flights to the northeast.

As shown in Fig. 9, the route taken by Siple and his party from Little America extended eastward to Mt. Grace McKinley, at the edge of the Rockefeller Plateau, thence past the Haines Mountains, across the John Hays Hammond Inlet and through the Edsel Ford Range to the extreme eastern end of the Raymond Fosdick Mountains. On this trip Siple was able to penetrate to the farthest point plotted on the maps from the aerial photographs of both the first and second expeditions.

Time will not permit even a resume of the accomplishments of the Byrd expeditions in the Antarctic, corresponding to those presented in previous papers for the Wilkes Exploring Expedition. Neither will a table of statistics setting forth the amazing figures for new areas explored, number and length of flights made, distances covered by sledge and tractor parties and so forth, give an adequate picture of the leadership, the pioneering, the utilization of the most modern technologic developments and the experience of his predecessors which have characterized the explorations of Byrd in the Antarctic.

The criticism has often been made that the Byrd expeditions have been unduly large, expensive, cumbersome and complicated. This is of course a matter of judgment on the part of a man organizing and leading any expedition. It cannot be denied, however, that by employing the ski, the sledge and dog team, the tractor and the airplane, in the manner best adapted to each; by making use of the latest features in diet and clothing; by establishing a weather forecasting system; and by employing radio to coordinate the efforts of all exploring parties at bases and in the field, Byrd has covered an enormous amount of new and little-known territory and has accumulated a vast amount of scientific data in two seasons of exploration. By making photographic records of his various activities and of the surrounding topography on his journeys of exploration he has made available a wealth of information which will become increasingly valuable as, in the years to come, the world becomes more and more familiar with the Antarctic.

DEMONSTRATION OF THE PENINSULARITY OF PALMER LAND, ANTARCTICA, THROUGH ELLSWORTH'S FLIGHT OF 1935

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(*Read February 23, 1940, in Symposium on Polar Exploration*)

THE end of the year 1935 witnessed the remarkable flight of 2,200 miles made by Lincoln Ellsworth from the extreme tip of Antarctica on its Atlantic side opposite South America to Byrd's polar base Little America on the Ross Sea on its Pacific side. The flight, which took about twenty hours of flying time, was made between November 23 and December 5, being interrupted by four landings on the ice cap of the interior. That the airplane was able to land and take off repeatedly from the ice and that the goal was accurately reached after so long an oblique course in high latitudes constitute major achievements in the field of aviation. That 1,200 miles of the total distance were flown over wholly unknown territory and that the salient features of this terrain were photographed from the air, permitting the construction of a map, are outstanding results in the field of geography.[1]

Two days before the final through flight, on November 21, a flight had been attempted but it had had to be abandoned after an advance of about 600 miles. That this flight at the time appeared unsuccessful to the leader of the expedition, eager to attain his distant goal, is understandable. However, it is this

[1] W. L. G. Joerg: "The Topographical Results of Ellsworth's Trans-Antarctic Flight of 1935," *Geogr. Rev.*, 26, 1936, pp. 454–462; idem: "The Cartographical Results of Ellsworth's Trans-Antarctic Flight of 1935," *Geogr. Rev.*, 27, 1937, pp. 430–444, with Pl. III containing Maps *A, B, C, D* on one sheet.

The map and the identification of localities in the preliminary 1936 article are superseded by those in the 1937 article. On the maps on Pl. III in the 1937 article the location is indicated along the flight routes of the points from which the photographs were taken, and on maps *A* and *B* a table is given of the photographs published in both articles. The identifications in the 1936 article that are incorrect can therefore be readily amended by means of this table, in addition to which quite a number of them are specifically set right in the text of the 1937 article.

flight and the corresponding segment of the final flight, with their partly parallel and partly crossing courses covering a belt of terrain some 150 miles wide,[2] that have yielded the most fruitful part of the cartographic harvest. It is on the results of this flight of November 21 and of the beginning of the flight of November 23 that the thesis of the present paper is based.

Before proceeding to a discussion of these flights it will be necessary, however, to gain a picture of the status of knowledge of the area in question immediately preceding the Ellsworth expedition. At this point a word is in order as to terminology. May it be stated, therefore, that in the present paper the term Palmer Land is used to designate in its entirety the finger of land projecting from Antarctica south of South America, or from its tip in about latitude 63° S. to its roots in about latitude 71°. This name, instead of Graham Land, is used in conformity with the suggestion made by Colonel Martin in a recent paper,[3] a suggestion which carries forward the efforts of an eminent member of the American Philosophical Society, the late Edwin Swift Balch, to set in their true light the explorations of Captain Nathaniel B. Palmer of Stonington, Connecticut.[4]

Wilkins' Discoveries in 1928

Turning now to what was known of Palmer Land before Ellsworth's flights, we find that the cartographic delineation in the stage next preceding that expedition dates from the year 1928. In that year, on December 20, Captain, now Sir Hubert, Wilkins made a memorable non-stop flight from Deception Island in the

[2] See especially Maps *A* and *D*, Pl. III. The present paper as read was illustrated by 14 lantern slides of photographs and maps. It has not been possible to reproduce these here, except for Figs. 1 and 2. To follow the argument it is necessary for the reader to have most of them before him. He is hence urged to consult at least the four maps cited in footnote 1 and the map cited in footnote 9 accompanying the article cited in footnote 5.

[3] Lawrence Martin: "An American Discovered Antarctica," *Comptes Rendus du Congr. Internt. de Géogr. Amsterdam 1938*, 2, 1938, pp. 215–218, Leiden. It may also be pointed out that the name Palmer Land has been applied to this feature on U. S. Hydrographic Office charts since 1912 (world, No. 923, changed in 1922 to No. 1262A, and SW Atlantic, No. 1132, changed in 1932 to No. 958) and that a decision in that sense was rendered Nov. 6, 1912, by the official United States Geographic Board (Fourth Report for 1890 to 1916, p. 197; Fifth Report for 1890 to 1920, p. 246).

[4] "Stonington Antarctic Explorers," *Bull. Amer. Geogr. Soc.*, 41, 1909, pp. 473–492; "Palmer Land," *ibid.*, 43, pp. 282–283; "The First Sighting of Antarctica," *Geogr. Rev.*, 15, 1925, pp. 650–653. The subject was first discussed by the author in his books, *Antarctica*, Philadelphia, 1902, pp. 85–95, and *Antarctic Addenda*, Philadelphia, 1904.

off-lying South Shetlands southward across the northern end of Palmer Land and then along the eastern side of that land to a point estimated on his map as about in latitude 71° S.—a distance of about 600 miles out and nearly 600 miles back.[5] Prior to the flight the east coast, difficult of approach by vessel because of the dense pack ice, had been outlined, in part sketchily, only as far south as the 66th parallel (by Nordenskjöld in 1902),[6] although the wide belt of shelf ice girding that coast and constituting its outer fringe had been skirted as far as the 68th parallel (by Larsen in 1893).[7]

In the southern part of his outward flight Wilkins found high mountains bordering the coast south to an estimated latitude of about 69⅔°. Thereupon he believed he discerned the following features in order from north to south to the turning point of his flight: a through channel; a 30-mile wide island; another through channel; a group of six or more islands; a major through strait; and a wide gently sloping land terminating in a seaward cape forming the southern portal of the eastern end of the strait.

In view of the bearing of these features upon the problem under consideration in this paper, Wilkins' statements will now be quoted verbatim:[8]

> . . ."By 12.32 we were opposite high steep mountains, one of which we named Mt. Ranck, and these seemed to terminate the second part of Graham Land. A channel, which narrowed in its center but appeared to hold only level ice, widened again; and we felt sure that the ice in the channel rests on land below sea level. We named it Casey Channel, and the island south of it Scripps Island.
>
> "South of South Graham Land the mountains are not generally as precipitous as on the more northerly islands. Scripps Island is about thirty miles wide. At 12.50 I noted in my log 'We are now quite certain that Graham Land is not connected with the mainland continent.' We felt sure of this before, but at this point it could be distinctly shown

[5] Sir Hubert Wilkins: "The Wilkins-Hearst Antarctic Expedition, 1928–1929," *Geogr. Rev.*, 19, 1929, pp. 352–376.

[6] Otto Nordenskjöld: "Die schwedische Südpolar-Expedition und ihre geographische Tätigkeit," *Wiss. Ergebnisse der Schwedischen Südpolar-Expedition, 1901–1903*, 1, No. 1, Stockholm, 1911, and Map 1 on scale of 1: 1,000,000 accompanying *ibid.*, 1, No. 2, Stockholm, 1917.

[7] C. A. Larsen: "Nogle optegnelser af sael- og hvalfanger Jasons reise i Sydishavet 1893 og 1894," *Norske Geogr. Selsk. Aarbog*, 5, 1893–94, pp. 115–131; Johannes Petersen: "Die Reisen des Jason und der Hertha in das Antarktische Meer 1893–1894, und die wissenschaftlichen Ergebnisse dieser Reisen," *Mitt. Geogr. Gesell. Hamburg*, 1891–1892 (published in 1895), pp. 245–298, with separate map, scale 1: 3,750,000 in 60° S., with accompanying text, pp. 299–305.

[8] *Geogr. Rev.*, 19, 1929, pp. 367–369.

in a photograph. We named the cape at the northern side of that channel Cape Walcott, the channel Lurabee Channel, and the islands to the south the Finley Islands. In a few minutes we could see that the group of islands did not extend very far. They were divided by level, smooth ice. There appeared to be a group of six or more of these islands scattered in a wide strait. We could not clearly define their outlines because we were then flying at two thousand feet, to which height we had descended for observation of the snow surface. Beyond the islands extended a level strait; and farther south a smooth slope, wide and unbroken, reached southward. It was not marked by even a speck or shadow and would not show on a photograph. We called the strait Stefansson Strait, and the land beyond it Hearst Land.

"The edge of Hearst Land, which we believe to be part of the great Antarctic continent, was distinguishable to the eye by a comparatively low ice cliff, which, because of the angle of the sun, failed to show distinctly in the photograph. And unfortunately at this important point I had difficulty with the camera. To the eastward the edge of the mainland was marked with a few small, low nunataks about a cape which I called Cape Eielson; and from there the low ice cliff swung slightly southward. Far beyond to the eastward, in the dim distance, too far to show on a photograph, I could see high tabular icebergs standing up above the level of the surrounding ice and concluded that they must have been at one time or were even then afloat."

It is on these statements and on the map representing them graphically [9] that the belief was based that Palmer Land is an archipelago separated from the main body of the Antarctic Continent. It is with these concepts in mind and with this map as a mental image that Ellsworth entered upon his 1935 expedition.

Ellsworth's November 21, 1935, Flight

On November 21, 1935,[10] then, Ellsworth, flying from Dundee Island at the northern tip of Palmer Land, proceeded south-southwest on a course which led him in general east of the edge of the shelf ice and hence considerably east—by some 70 or 80 miles—of Wilkins' outward track. On striking the coast, probably in about 69½° S. latitude, he continued inland in a southwest direction for about 100 miles, which course led him into a great mountain range trending north-northwest and south-southeast which attained elevations of over 10,000 feet and the crest of which was visible to the southeast for at least 75 miles. Meet-

[9] *Ibid.*, Pl. IV, facing p. 374, scale 1: 6,000,000.
[10] *Geogr. Rev.*, 27, 1937, Pl. III, Maps *A* and *D*.

ing with violent headwinds and a cloud ceiling he was forced to turn back. On reaching the coast about where he had crossed it on flying inland, he followed the coast in a northwesterly direction for about 80 miles and then, finding that advance up a promising major depression opening towards the west was likewise barred by clouds, he set his course northeast and returned to Dundee Island.

During the flight it was believed that among the landfalls certain of Wilkins' features had been recognized, such as Mount Ranck, Stefansson Strait, and Cape Eielson, and they were thus referred to in the first published accounts of the flight. But in view of the uncertainty as to the exact location of these features and the necessarily indistinct character of Wilkins' photographs of some of them it is not surprising that these names were not in all cases allocated to the same features on both flights. Later, after the return of the expedition, the map constructed from the photographs taken by Ellsworth for the first time provided a realistic framework to which the described features could be related.[11]

Ellsworth's November 23, 1935, Flight

The November 23 flight, to the relevant northern part of which [12] we now give brief attention, proceeded in a southwest-

[11] The first published accounts appeared in the *New York Times,* November 22, 24, 26, 1935, and January 11, 19, 22, 23, 1936 (some of which are available in more permanent form in the *Polar Times,* published by the American Polar Society, New York, No. 2, January 1936, pp. 1–9). The definitive map and the revised identifications of the photographs appeared in July 1937 in the article (Cartographical Results, etc.) in the *Geographical Review.* In the subsequently published volume by Mr. Lincoln Ellsworth, entitled *Beyond Horizons* (Garden City, N. Y., copyright date January 21, 1938), the section in the body of the text dealing with the 1935 expedition (pp. 298–363) and the captions of the related photographs have not been editorially coordinated with the July 1937 article, although a condensed version of the article appears in Appendix V of that volume. In the case of discrepancies between identifications in the July 1937 article and those in the volume, the reader should therefore not necessarily assume that the latter, being of more recent date of publication, are correct.

The account of the expedition in the *Geographical Journal* (Lincoln Ellsworth: "The First Crossing of Antarctica," 89, 1937, pp. 193–213 with map in 1: 12,000,000) based on Mr. Ellsworth's address in London on November 30, 1936, and published in March 1937 of course appeared too early to take into account the July 1937 article, although use was made of the July 1936 article and of material subsequently furnished by the *Geographical Review* to the editor of the *Geographical Journal.* The account in the *National Geographic Magazine* ("My Flight Across Antarctica," 70, 1936, pp. 1–35) also appeared too soon.

[12] *Geogr. Rev.,* 27, 1937, Pl. III, Maps *A, B,* and *D.*

erly direction from Dundee Island and crossed the coast about half way between the outward and return tracks of the November 21 flight, about in latitude 69°. The axial range of Palmer Land was again met with, and this time it was crossed. Its culminating peaks here are slightly less than 10,000 feet. Continuing in a southwesterly direction the mountains merged into a great snow-plateau surface about 6,000 feet high and about 100 miles wide. This was followed by what during the flight seemed to be two extensive north-south trending parallel mountain ranges but which, after plotting the map, turned out to be the bounding scarp walls of an enormous rift valley or fault depression about 15 miles wide, whose shelfice-filled bottom lies at sea level. Beyond this rift valley the plateau character began again and continued beyond the bounds of the area with which we are here concerned.

The Resulting Maps

The results of these two flights were plotted on two maps, one of the east coast published on the scale of 1:400,000, or about 6½ miles to the inch, and the other of the rift valley in 1:800,000, or about 13 miles to the inch.[13] These are relatively large scales, and the corresponding detail is considerable, for reconnaissance exploration. That this degree of close representation was possible is due to the fact that Ellsworth, as already mentioned, took numerous photographs from the air during the two flights. A record was kept of the time when each exposure was made and of the side of the plane from which it was made. The flying height of the plane was as a rule available from the log. The photographs were made with a Leica camera, each exposure being hence very small (about 1 × 1½ inches), since it had not been possible to bring the air-mapping camera with which the expedition was to be equipped. Owing to these and other circumstances there were definite limitations to the construction of a map from the photographs, and that they were overcome successfully is due to the special technique and the special instruments in photogrammetry that have been developed at the American Geographical Society in New York under the direction of O. M. Miller, head of the Society's Department of Technical Training.

[13] The 1:400,000 is the Map *A*, the 1:800,000 the Map *B* previously cited.

LOCATION OF STEFANSSON "STRAIT"

Once the data had been plotted it became possible, as regards the east coast, to attempt with some prospect of success to correlate Wilkins' route with the features laid down on the map, and hence to derive some clue as to the location of Stefansson Strait, now that its existence as a transverse through channel seemed clearly disproved by Ellsworth's discovery of a major axial range. Fortunately it was possible to identify features on three of Wilkins' published photographs on photographs taken by Ellsworth and, through Sir Hubert's kind cooperation, it was possible to study for the first time a motion picture he made on his 1928 flight. This yielded one of the most obvious identifications, since both flight tracks here lay near the coast and the photographs by both explorers had hence been taken relatively close to the features they represented.[14]

With these identifications established, the sought-for clue for locating Stefansson Strait was available.

Stefansson Strait was shown on Wilkins' so-called photographs *P* and *Q*, both looking southwest, the former taken on the outward flight, the latter on the return, but both identified by an overlapping feature as one continuous panorama.[15] The positions of *P* and *Q* now being known in relation to the positions of Wilkins' preceding three photographs and the motion picture, the general location of that strait could be indicated. According to these data it lies beyond, *i.e.* to the southeast, the last coastal features that can be mapped from Ellsworth's photographs.[16] The farthest penetration of Ellsworth's November 21 track probably parallels it on its northwestern flank, and the discovery of the high axial range at the head of that penetration definitely limits its upper extension. The probabilities are that Stefansson Strait is a major glacier or ice stream of low gradient that flows northeasterly and, like others farther north, breaks through to the coast.

[14] The four matching sets and the figure numbers in the July 1937 article are as follows: Wilkins' L with Ellsworth's 3 and 5 (Fig. 1 and Figs. 2–3 respectively); Wilkins' N with Ellsworth's A–3 (Figs. 5 and 4); Wilkins' O with Ellsworth's 8 (Figs. 6 and 7); exposure from Wilkins' motion picture with Ellsworth's A–2 (Figs. 9 and 8).

[15] July 1937 article, Figs. 10 and 11, combined and clarified in Fig. 12, a line sketch.

[16] See Map *D*.

The Peninsularity of Palmer Land

It has seemed necessary to go into detail to this extent in order to pass in review the essential topographic items that affect the problem under consideration. It is now the moment to gather the evidence together, amplify it, and arrive at a conclusion.

1. It is definitely established that there are no through channels at the location of Wilkins' Casey and Lurabee Channels and Stefansson Strait.

2. The weight of the evidence points to the fact that what were taken to be through channels are the mouths of large glaciers or ice streams of low gradient. This has been definitely established with regard to the "major valley depression" of Ellsworth's map, which is probably Wilkins' Casey Channel, by the ground surveys of the efficient and productive British Graham Land Expedition under Rymill, a sledge party of which worked in this area one year later than Ellsworth's flight and in crossing Palmer Land from west to east in this latitude followed along the southern edge of this depression and traced it from the crest of the axial range to the sea (Figs. 1 and 2).[17]

Even Wilkins in 1928 on several occasions, with regard to depressions farther north, corrected his first impressions that they were through channels. To quote him:[18]

[17] J. R. Rymill: "British Graham Land Expedition 1934–37," *Geogr. Journ.*, 91, 1938, pp. 292–312 and 424–438; W. L. S. Fleming, A. Stephenson, B. B. Roberts, and G. C. L. Bertram: "Notes on the Scientific Work of the British Graham Land Expedition, 1934–37," *ibid.*, 91, 1938, pp. 508–532. The results were later published in book form, *Southern Lights: The Official Account of the British Graham Land Expedition, 1934–1937*, London, 1938. The account of the sledge trip across Palmer Land and back, November 11 to December 29, 1936, is on pp. 430–431 of the *Geographical Journal* version and in Chapter 11, especially pp. 217–239, of the book version.

Both the journal and book versions are accompanied by three separate maps, of which the one of Palmer Land (Graham Land) as a whole on the scale of 1: 3,500,000 is the one that concerns us here, since it is the only one that portrays the west-east crossing and also the exploration of the rift valley. Larger-scale maps of these two areas were forecast (*Geogr. Journ.*, 91, 1938, p. 515) for a later number of the *Geographical Journal* but, so far as the present writer is aware, they have not yet appeared.

The results of the Rymill expedition were briefly reported, so far as available at the time through radio messages published in the *London Times*, December 12, 1936, and January 27, 1937 (abridged in weekly edition of December 17, 1936, and February 4, 1937), in the July 1937 *Geographical Review* article and there correlated with Ellsworth's work.

[18] *Geogr. Rev.*, 19, 1929, p. 364.

"At ten o'clock we were opposite a deep inlet, Evans Inlet [about 65° S.], which appeared at first to cut right through Graham Land. In fact I actually made a note to that effect in my log but corrected it later when we could see the mountains at the end of the level ice. Opposite Jason Land was a deep indentation which also threatened to sever the north of Graham Land but again did not reach quite through. This apparently is Nordenskjöld's Richthofen Valley [about 66° S.]."[19]

3. In conformity with this view, Stefansson Strait appears to be one of these ice streams flowing down to the coast.

4. The discovery that the axial range continues across the previously assumed through channels and extends at least 75 miles southeast of Ellsworth's track of November 21, or to about latitude 72°, establishes the fact that the great belt of young folded mountains represented by the Andes of South America, the continuation of which has been traced petrographically through the South Sandwich loop into the northern part of Palmer Land, persists at least this far. It seems unlikely that this range, in the vigor of its 10,000 foot elevation, should terminate abruptly here. It appears more likely, if a guess may be hazarded,[20] that it continues southeastward, along the western side of Weddell Sea, for 500 miles more until it impinges on the oldland mass of East Antarctica.

Be that as it may, it seems improbable that, as one proceeds south, there be any further opportunity for the development of through channels, since the landmass rapidly grows wider in this direction. As to the drowned rift valley on the west side of Palmer Land, both Ellsworth's map and Rymill's report indicate that it trends toward the southwest at its southern end, or away

[19] In about latitude 66½° S. Wilkins thought he saw another through channel. ("Just south of there we saw what we believed to be a circuitous channel dividing Graham Land. . . . It was named Crane Channel," *ibid.*, p. 366.) The non-existence of this channel seems almost certainly established through the skirting, by the *Discovery II* on its 1931 cruise, of a continuous coast at what would be the channel's western end (H. F. P. Herdman: "Report on Soundings Taken During the Discovery Investigations," *Discovery Reports* (Discovery Committee, Colonial Office, London), 6, pp. 205–236, with charts 1–7 in case, reference on chart 7, Soundings in Marin Darbel Bay; *British Antarctic Pilot, 1930, Supplement 1938,* p. 38) and through Rymill's airplane reconnaissance of the west side of Palmer Land in this area on February 13, 1937, during which the coast was seen to be continuous at this point (*Southern Lights,* p. 250). A large glacier, however, descended to the sea; this may be the one taken by Wilkins for a through channel.

[20] In consonance with Rudolph Staub: *Der Bewegungsmechanismus der Erde,* Berlin, 1938, pp. 117–118 and map in pocket. On the underlying concepts of geotectonics see Staub, pp. 7–21, and W. H. Bucher: *The Deformation of the Earth's Crust,* Princeton, N. J., 1933.

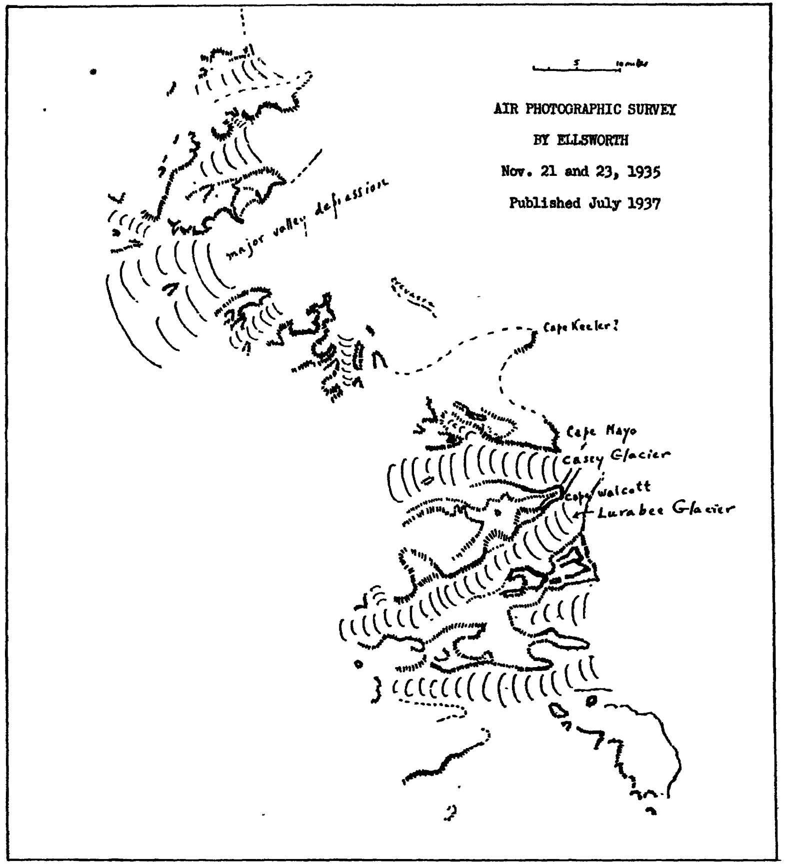

FIG. 1.

FIGS. 1 AND 2.—Comparison between Ellsworth's and Rymill's representations of the east coast of Palmer Land in approximate latitude 68° 50′ to 71° 0′ S.

Note the correspondence between the two in the sequence, from north to south, of glacier-filled valleys and intervening mountain ridges ending seaward in capes. (The apparent discrepancy in the representation of the coastline northwest of Cape Keeler is due to the fact that Ellsworth's map shows the indented outlines of the spurs forming the glacier walls whereas the smooth outline of Rymill's map represents the contact of glacier ice with sea ice, a segment of which is shown on Ellsworth's map as 'crevasse.')

The depression extending from Wordie Shelf Ice on the west to Weddell Sea north of Cape Keeler on the east, occupied by two major glaciers of low gradient flowing in opposite directions, is probably Wilkins' Casey Channel. By analogy, his Stefansson Strait is probably likewise a glacier (see p. 827 above).

Fig. 1 is based on Map *A* cited in footnote 1. Fig. 2 is U. S. Hydrographic Office Chart 5411 rendering, here slightly modified, of the 1: 3,500,000 map cited in footnote 17. All names on the east coast except Mt. Wakefield, which is Rymill's, are Wilkins'. The names on Map *A* (reflected in Fig. 1) were assigned by Wilkins in attempting to identify his 1928 features in terms of Ellsworth's topography. Among these Lurabee Glacier and Cape Walcott were positively identified, through linkages referred to in footnote 14 above; less assurance was felt as regards the other names. At that time (spring 1937) Rymill's map was not yet available. If it had been, it is believed that 'Casey Glacier' would have been applied to Ellsworth's 'major valley depression' and not to its present location. In accordance with Wilkins' original account (see p. 823 above and map cited in footnote 9) Cape Keeler (and Mt. Ranck) and Cape Mayo should then probably be shifted respectively to the northwestern and southeastern portals of Casey Glacier (Cape Mayo where Cape Keeler is now placed).

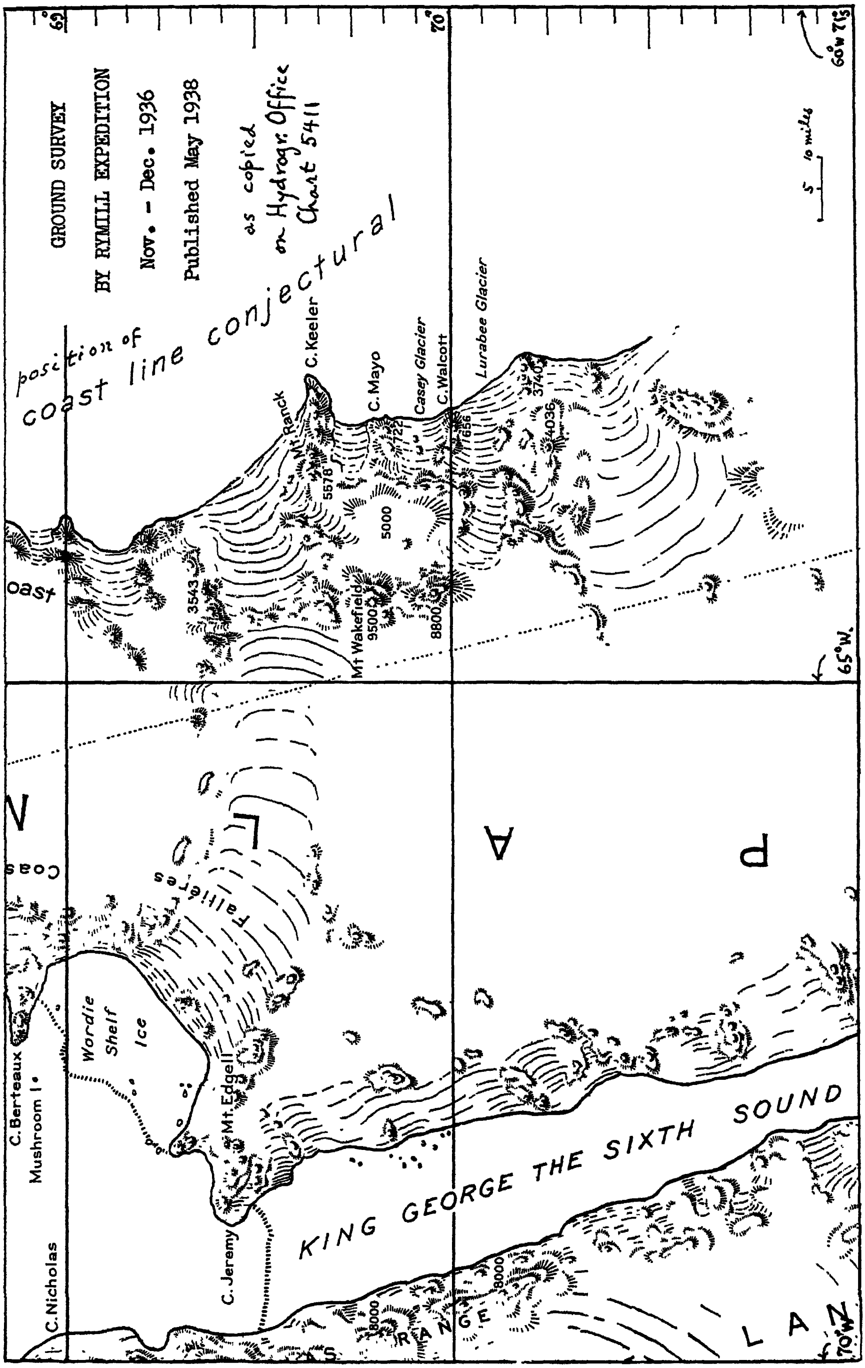
GROUND SURVEY
BY RYMILL EXPEDITION
Nov. - Dec. 1936
Published May 1938
as copied
on Hydrogr. Office
Chart 5411
position of
coast line conjectural
C. Keeler
C. Mayo
Casey Glacier
C. Walcott
Lurabee Glacier
Mt Wakefield
9500
8800
5000
5578
3543
3740
4036
Fallières
Coast
Wordie
Shelf
Ice
Mt Edgell
C. Berteaux
Mushroom I.
C. Jeremy
C. Nicholas
KING GEORGE THE SIXTH SOUND
RANGE
8000
LAND
65°W.
70°W.
70°
69°
5 10 miles

FIG. 2.

from the Weddell Sea coast, and it therefore hardly seems probable that it would constitute a severance of the main structural axis, although it may completely separate a fragment block (Alexander I Land or, in this case, Island) on the convex side of that arcuate axis.

5. Finally, it may be pointed out that, even if certain transverse channels might exist, they would not effectively alter the situation, since the shelf ice and other forms of persisting ice would weld the land into one continuous whole. The view of ice as potential water is valid only in climates where seasonal change does bring about melting. But where the change does not have this effect, the ice is more closely akin to land than to water. This is not only a theoretical consideration, but one of practical import, especially in the fields of aviation and water navigation and with implications affecting the concept of territorial sovereignty.[21]

Such is the evidence and such are the probabilities. Although positive proof is not possible short of a complete survey of the region, it seems apparent that the results of Ellsworth's flights of 1935 demonstrate that Palmer Land is not an archipelago but a fingerlike peninsula projecting from the landmass of the Antarctic Continent.

[21] See section entitled 'Consideration of Geographical Features in the Settlement of Political Sovereignty' in W. L. G. Joerg: "Brief History of Polar Exploration Since the Introduction of Flying," *Amer. Geogr. Soc. Spec. Publ. No. 11*, 2nd revised edition, 95 pp., New York, 1930; reference on pp. 77–78.

ANIMAL LIFE IN THE ANTARCTIC

(Illustrated with Motion Pictures)

EARLE B. PERKINS

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(Abstract of paper read February 23, 1940, in Symposium on Polar Exploration)

THE Antarctic ice barrier is not a zoologist's paradise. As far as the eye can reach there is nothing but a vast expanse of ice and snow. Near the open sea groups of penguins are frequently seen, and the seals congregate in the summer season to rear their young on the old sea ice. Overhead the terns and petrels are most abundant, although never conspicuous for their great numbers.

Inland, the continent seems utterly devoid of life. Actually there are wind-borne spores in the air, and a few bacteria in the snow. In the high mountains near the polar plateau there are exposed black rocks which absorb enough heat during a few weeks when the sun is highest to melt the snow and form pools of water. Here a cycle of life is begun, with algæ supporting some microscopic protozoans, rotifers, and tardigrades. There is no obvious animal life at any great distance from the open sea.

In the depths of the frigid Antarctic waters, and in shallow areas where the coastal rock is exposed, there are representatives of every animal phylum. Under a dozen feet of ice, and even in the darkness of the winter night there live vast numbers of organisms. The low temperature reduces organic decomposition to a minimum so food is abundant. Furthermore, although but few species are represented, there are a great many individuals. Apparently it is difficult for a species to become established, but once it has gained entry, it becomes tremendously numerous due to the abundant food supply and few enemies.

The upper waters are especially rich in small crustaceans, and a few species of fish. Dredge hauls bring up a rich collec-

tion of bottom fauna representing most of the invertebrate groups. But it is on the ice floes, the fast sea ice and the barrier itself that we find the conspicuous and characteristic animal life of the Antarctic—the seals and the penguins. Beyond is the barrenness of a nearly lifeless world.

GLACIERS OF ANTARCTICA

LAURENCE M. GOULD

Professor of Geology, Carleton College; Senior Scientist and Second in Command, First Byrd Antarctic Expedition

(*Evening Lecture, February 23, 1940, in Symposium on American Polar Exploration*)

CONTENTS

ILLUSTRATIONS

Introduction

"The land was so overwhelmed with ice that even at sea-level the rock was all but entirely hidden. Here was an ice age in all earnestness." Thus did Mawson describe his first impressions of Adelie Land and such in general is Antarctica. Except for very limited areas about the coastal fringe and some of the mountain masses of West Antarctica, there are no ice free areas on the whole continent. How great the glacierization is even on such high rugged coastal margins as the Queen Maud Mountains may be inferred from Pl. I.

Though Antarctic ice masses are waning, the great continental ice sheet with its seaward extensions is still thousands of square miles larger than the continent which supports it. The total area approximates at least five millions of square miles—a very much larger area than was occupied by any of the great ice sheets which covered parts of North America and Eurasia at the culmination of Pleistocene.

The main features of Antarctic glaciation lend themselves to a three-fold grouping: first, the continental glacier or the inland ice; second, the marginal features of this great ice sheet; and third, the independent or detached ice forms of the coastal margins (Fig. 1).

Inland Ice

The inland ice of Antarctica is generally regarded as the ideal type or example of a continental ice sheet. This supposes that the ice is thick enough throughout to mask completely the underlying terrain and to achieve a flat dome shape with rapidly increasing curvature as the margin is approached. Only near the borders of such ice sheets may the rocky substructure reveal itself and exert some control over the shape and movement of the ice.

The flight of Ellsworth and Hollick-Kenyon across West Antarctica from Dundee Island on the Palmer Land side to the vicinity of the Bay of Whales demonstrated that the concept of this part of the inland ice, at least, as being an ideal example of a continental ice sheet needs to be modified. Joerg (1936, p. 460–461) who has carefully examined the data from their flight points out that not only are the 11,000 foot peaks of Eternity

Range which lie far from the coast a direct continuation of the main axis of Palmer Land, but that nunataks and even mountain ranges were discovered along the flight track almost half way across West Antarctica. These culminate in the Sentinel Range

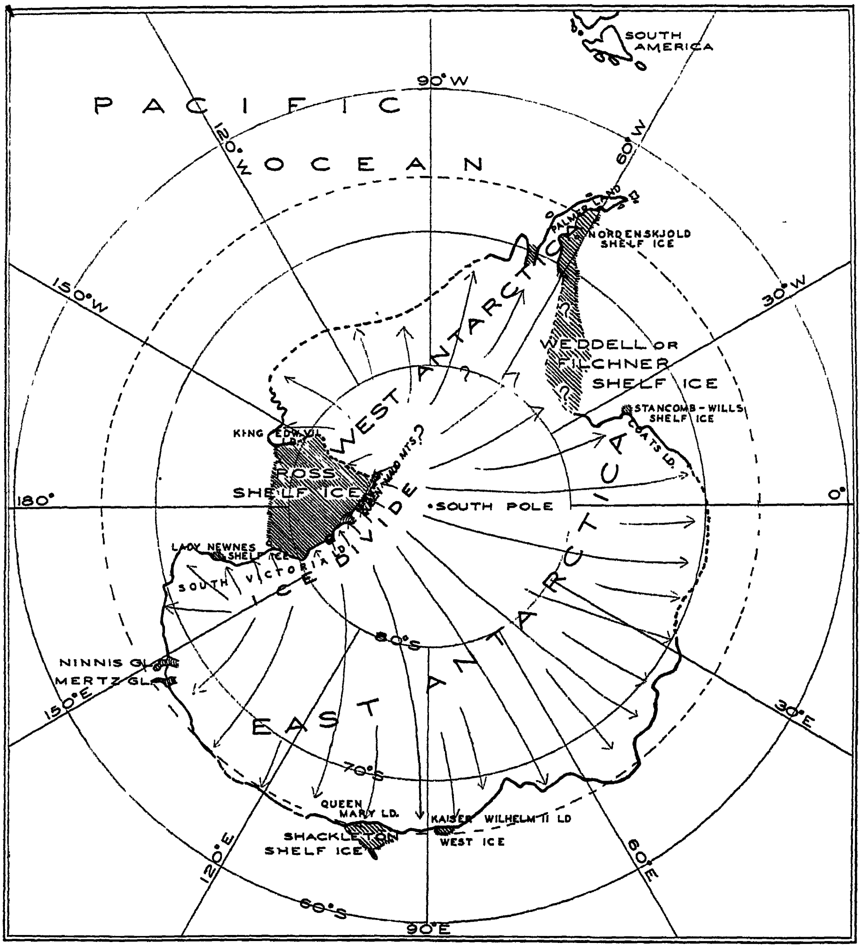

FIG. 1. Major features of Antarctic glaciers.

which rises to heights of 12,000 feet in the approximate position of 86° west longitude and 76° and thirty minutes south latitude. Joerg concludes that "the interior of the ice cap unlike its counter part of the same size in Greenland does not completely mask the underlying topographical features". His reference is

clearly to the ice cover of West Antarctica only. This ice sheet, therefore, has much in common with the Spitzbergen type of ice or "highland ice" as Wright and Priestley (1922, p. 183) call it.

So far as East Antarctica is concerned our knowledge due to direct observation is confined to a few hundred miles within the margins of South Victoria Land and the land beyond the Queen Maud Mountains at the head of the Ross Sea. Sledge journeys of Mawson, Shackleton, Scott, Amundsen and sledge journeys and flights of the Byrd Expeditions have, all told, penetrated but a small part of this vast reservoir of ice. It still remains to be explored.

It is perhaps significant of the shape of the main mass that all the sledge journeys beyond the coastal margin toward the interior reveal an ice divide relatively near the great Antarctic horst which forms this part of the margin of the continent. Figure 2 shows profiles of the routes of Amundsen and Scott from

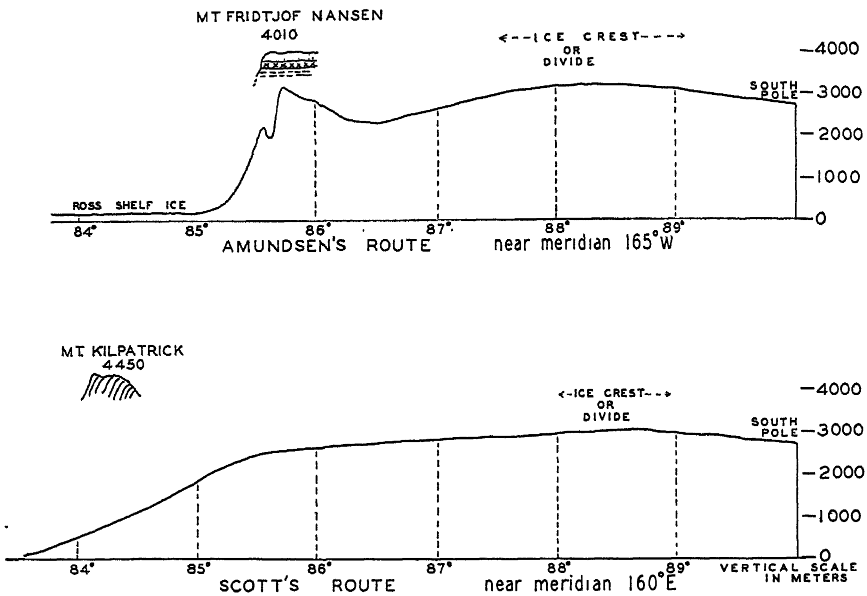

Fig. 2. Profiles of inland ice south of Ross Shelf Ice.

the head of the Ross Shelf Ice to the South Pole. If the ice divide which both crossed is the center of accumulation and dispersal for the major part of the continent, it naturally follows that the ice must thin greatly in the directions of the Atlantic,

African and Indian margins of the continent. It may be so greatly thinned indeed that here as in West Antarctica the ice near the margins at least possesses an undulating surface which reflects the topography underneath.

It will be noted from these profiles that the structure of the continental margin about the head and the west boundary of the Ross Sea, at least, is not one that leads to the conception of a continental ice sheet of great thickness (Fig. 3). Mt. Kilpatrick near Beardmore Glacier and Mt. Fridtjof Nansen near Amundsen's route are parts of the great Antarctic horst which extends from Cape Adair to the southeast apex of the Ross Senkungesfeld and perhaps farther. Though these mountains are higher than many of their neighbors, the general altitude of the horst zone across the head of the Ross Shelf Ice exceeds 3000 meters

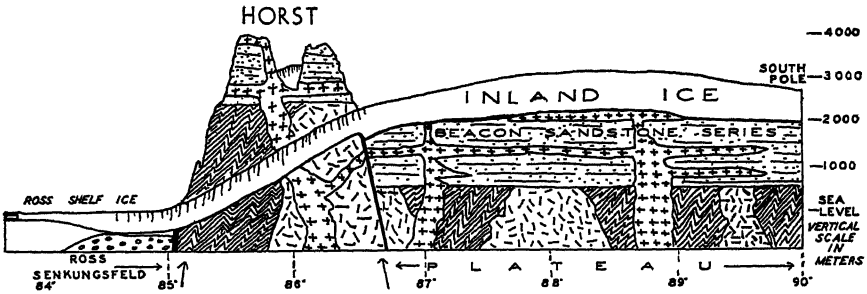

FIG. 3. Section across Queen Maud Mts. and Polar Plateau.

so that except where it is broken by the outlet glaciers it is generally much higher than the inland ice to the south of it. On his ascent to the South Polar Plateau, Amundsen climbed over a part of this horst as is evident from the profile of his route. That this great horst is backed by an extensive plateau is clear, but to assume that the thickness of the ice can be guaged on the assumption that such a plateau stretches away as a great relatively flat surface does not necessarily follow. If such is the case then the ice probably does not exceed three or four thousand feet in thickness. However, if the surface of the continent has been depressed by its load of ice in proportion to the places of greatest accumulation, then the lower surface of the ice will be saucer-shaped as is true of the inland ice over Greenland. Thickness equalling or surpassing those measured in Greenland (8000 ± feet) may then be found.

Though there seems to be no longer good reasons for speculating upon an actual below sea structural or topographic connection across the continent connecting the Ross and Weddell Seas, the deep penetrations of these two embayments from nearly opposite sides of the continent does divide it into two major geographic regions which are generally referred to as East Antarctica and West Antarctica. The decreased area for the support of the inland ice cover where this narrowing occurs may well be reflected in a sag in the surface of the overlying inland ice. Somewhat after the fashion of Greenland then there may be at least two major centers of dispersion—a much larger and much higher one over East Antarctica which exhibits all the characteristics of the ideal continental ice sheet and a lower, thinner one over West Antarctica which conforms to some extent to the rock terrain below.

The true glacial ice of which the continental ice sheet must be composed is everywhere covered with an unknown but probably very great thickness of firn and snow. The actual surface is everywhere one of snow—traces of granulation are rare. The snow is characteristically hard and tough because it has been packed by the terrific winds which are an inevitable result of a continental ice sheet. Over much of the surface the snow is roughened into hard ridges from a few inches to as much as three or four feet high. These ridges or sastrugi betray the direction of the prevailing winds. Amundsen and Scott reported areas of soft snow about the South Pole but such occurrences are clearly exceptional.

Marginal Features of the Inland Ice

Depending upon the character of the coast, three principal types of marginal features are developed: first, in places the inland ice flows into the sea as vast undulating or terraced sheets which are normally heavily crevassed; second, where the outward moving ice is directed by the coastal topography, glacial tongues are formed which are projected into the sea as floating ice tongues; third, under conditions which offset the normal destructive activities of wind, waves and currents, there develop extensive areas of relatively thin ice masses, largely afloat, known as shelf ice. Naturally the lines of demarcation between these three types are gradational.

Margin Unconfined by Mountain Ramparts

Though East Antarctica seems to be tectonically essentially a great plateau bounded in part by high mountains, there are considerable areas where the inland ice moves into the sea without the directive and confining influence of mountain ramparts.

Where the inland ice descends to Weddell Sea in Coat's Land, it is said to rise up very gradually in undulating slopes and fade away in height and distance into the sky. The actual seaward margin of the inland ice here was an ice cliff from one hundred to one hundred and fifty feet high. Again in Kaiser Wilhelm II Land the inland ice ends in the sea with cliffs of about the same height. It rises in undulating crevassed slopes toward the south over the land mass which is beneath it. From such regions as this come the blue Antarctic bergs in contradistinction to the much more common dazzling white tabular bergs derived from shelf ice. From observations in Queen Mary Land, near Shackleton Shelf Ice, Wild states that the inland ice "appeared to rise rapidly to about three thousand feet and then to ascend more gradually as the great plateau of the Antarctic Continent".

The inland ice of the coastal margin of Adelie Land according to Mawson (1914, vol. 1, p. 64) "was heavily crevassed and fell sheer to the sea in cliffs, sixty to one hundred and fifty feet in height".

It is to be expected that the unconfined margin of the inland ice should be heavily crevassed. Furthermore such marginal undulations as those noted above may be reflections of a rolling topography beneath the ice and so they have been generally interpreted by the explorers who have observed them. However, David (1914, p. 60) noted that the surface of the inland ice within the South Victoria horst in the direction of the South Magnetic Pole undulated in broad billows about forty to fifty feet deep and many hundreds of yards from crest to crest. These great wrinkles flattened out and disappeared seventy miles from the inner edge of the horst or ninety miles from the coast. David believed these to be the result of the marginal movement of the inland ice. In like fashion the undulations described by others about the margins of the continent may be but visible evidences of the thrust pressures inherent in the outward motion of the inland ice.

Whether the ice from West Antarctica moves into the Pacific Ocean in great sheets after the fashion just described, unhindered by nunataks or mountain barriers, is quite unknown. Ships have never been able to penetrate the heavy ice of this sector far enough to make even approximate guesses as to the position of the coast line. The fact that Hollick-Kenyon noted a water sky to the north of his flight with Ellsworth across West Antarctica indicates that the coast is not as far north as was previously believed. It also suggests that the coast line is probably not deeply embayed and therefore not bordered by extensive areas of shelf ice.

From the observations made by the sledge and tractor parties of the Second Byrd Antarctic Expedition and from my own observations as to the behavior of the ice in the eastern section of the Ross Shelf Ice, it seems likely that the inland ice from West Antarctica moves westward into the Ross Senkungesfeld as great sheets over a large part of the section between King Edward VII Land and the Queen Maud Mountains.

Margin Modified by Mountain Ramparts

It is where the margin of the inland ice is more or less directed or controlled by the coastal topography that glacial forms achieve their greatest variety. Depending upon the extent of terrestial control, features, all the way from lobate margins to outlet glaciers, shapes of which and directions of movements are clearly defined by their rocky boundaries, are formed.

From observations made on a sledge journey into the Edsel Ford Mountains which form a portion of the Ross Sea Margin of West Antarctica, Wade (1937, p. 584–591) demonstrated that this range is transected by great outlet glaciers which drain the inland ice into the shelf ice of Sulzberger Bay.

Where the Atlantic coast of Palmer Land joins or becomes the main continental land margin, it is cut up into headlands and capes by a number of great outlet glaciers. Some of these streams of ice spread out to form piedmonts and others extend directly into the Weddell or Filschner Shelf Ice.

The long skeletal peninsula of Palmer Land is still so heavily burdened with ice that it sends large outlet glaciers down from its plateau top both to its Atlantic and Pacific margins. On the Pacific coast many such glaciers observed by Fleming (1938, p.

508–512) flow along parallel to the coast before turning to enter the sea. Wilkins (1929, p. 360–369) points out that among the most conspicuous features of the mainland as seen from his flight along the Atlantic side of the peninsula are great ice-filled fiords, that is, outlet glaciers which almost sever the peninsula.

The Shackleton Shelf Ice is in part fed and supported by great outlet glaciers from the inland ice. Indeed if Terminal Tongue is the actual continuation of one such outlet glacier, Deman Glacier, it represents quite the longest glacial tongue known anywhere for it projects beyond the coast more than one hundred miles.

Among the most notable outlet glaciers extensions into the sea as floating ice tongues are the Ninnis and Mertz Glaciers of Adelie Land discovered and mapped by Mawson (1914, vol. 2, p. 58–60).

That the projections of the inland ice into the sea as such great tongues as these two glaciers as well as those of the Shackleton Shelf Ice are reflections of valley depressions in the rocky floor of the continental margin seems clear. But it is where the inland ice is dammed by the great Antarctic horst which forms both the southern and western boundaries of the Ross Senkungesfeld and which is much higher than even the greatest known heights of the inland ice that outlet glacier features are most sharply defined and exhibit their greatest variety.

Glaciers of the Great Antarctic Horst.—From McMurdo Sound northward, glaciers that drain the inland ice of South Victoria Land flow directly into the sea to develop a variety of coastal features whereas southward along the western and southern margins of the Ross Sea, all ice streams that flow outward from or through the horst lose their identity in the Ross Shelf Ice.

Where the outlet glaciers enter the open sea that most characteristic of Antarctic shore ice forms, the *ice tongue afloat,* is extensively developed. Such floating ice tongues were first discovered from their seaward ends and are so far from their sources that in many cases other names were given to the floating ice tongues than to the outlet glaciers from which they were projected. Nordenskjöld glacier tongue for instance is an extension of Mawson Glacier. Drygalski tongue likewise has its origin in David Glacier. Such floating ice tongues characteristically develop a long, triangular shape (Fig. 4).

The outlet glaciers normally flow down from the plateau through horst mountains in inherited valleys or in valleys which are in part at least of their own making. In either case their shape depends upon the depressions which they occupy. In a few rare and apparently unimportant cases, streams of ice fed from the plateau flow downslope toward the sea unrestricted by valley walls. Taylor (1922, p. 106) referred to such forms as "curtain" glaciers and believed them to be mostly stagnant, and of little significance as drainage channels from the inland ice.

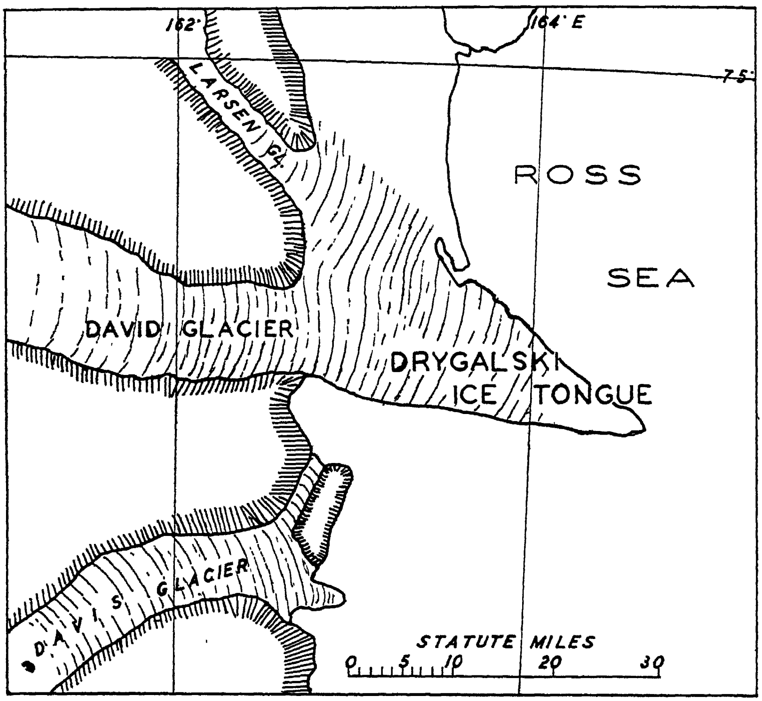

FIG. 4. The Drygalski ice tongue (after Mawson).

Wright and Priestley (1922, p. 153) included them in their classification as "wall-sided" glaciers.

Where the ice from two or more outlet glaciers coalesces over a comparatively level plain at the foot of the mountains which they have left, *piedmont glaciers* develop. Such ice masses characteristically have a great length along the coast in comparison with their width. The seaward margin is usually lobate opposite the glaciers which have formed the piedmont. Whether

such ice masses are wholly or in part aground or whether they are afloat has no bearing upon their origin or classification. One of the most notable and typical glaciers of this sort is the Wilson Piedmont. This piedmont fringes the coast to an average depth of five miles and extends outward over the gently sloping shelf to achieve a total width of thirty-six miles. It is advancing almost everywhere along its breadth into the sea.

Where the seaward projections of a number of ice tongues are held in and given a definite form by marginal islands or reefs the "Confluent ice" of Wright and Priestley (1922, p. 160) develops. This is in reality but a variation of the true piedmont. Naturally glaciers fed from local ice fields may also develop piedmonts.

Poleward from McMurdo Sound the outlet glaciers increase in size culminating in the Beardmore which was discovered in 1909 by Shackleton and still remains the largest glacier known. But of all known parts of the horst boundary of the Ross Sea the Queen Maud Mountains present the greatest array of outlet glaciers. They are all of great size and one, at least, The Leverett, may prove when fully explored and mapped to be even larger than the historic Beardmore.

Outlet Glaciers of the Queen Maud Mountains.—Within the area explored by the geological party of the First Byrd Expedition (Gould, 1931, p. 148–234) which extends from about 145° west to 170° west, no less than seven major outlet glaciers were mapped. Liv (Pl. I) and Axel Heiberg Glaciers were discovered and named by Amundsen on his sledge journey to the South Pole in 1910–1911. The remaining outlet glaciers of Kent Cooper, Isaiah Bowman, Amundsen, Thorne and Leverett were discovered by members of the First Byrd Expedition (Gould, 1931, p. 180–194). Blackburn led a sledging party on the Second Byrd Expedition which followed Thorne Glacier to its head. His party mapped this glacier and added details to the map made by the geological party of the First Expedition in the vicinity of Supporting Party Mountain.

Naturally Fig. 5 shows but the main outlines of these great streams of ice. Their detailed patterns are somewhat more complex than could well be indicated for, in addition to the main streams of ice from the polar plateau, there are many tributaries derived from catchment areas within the mountains themselves.

The least explored and yet the most interesting of the Queen Maud Mountain Glaciers is Leverett. The volume of ice poured into the Ross Shelf Ice by this glacier exceeds that from any of the rest examined, yet its observed portion had a flatter gradient and its boundaries were less sharply defined than any of the rest. The southern boundary is well defined by an escarpment, but the northeastern margin is imperfectly defined by nunataks. A number of small tributaries dissipate some of the ice from the main current as they squeeze down between these nunataks. The very mouth of the glacier itself where it enters the Ross

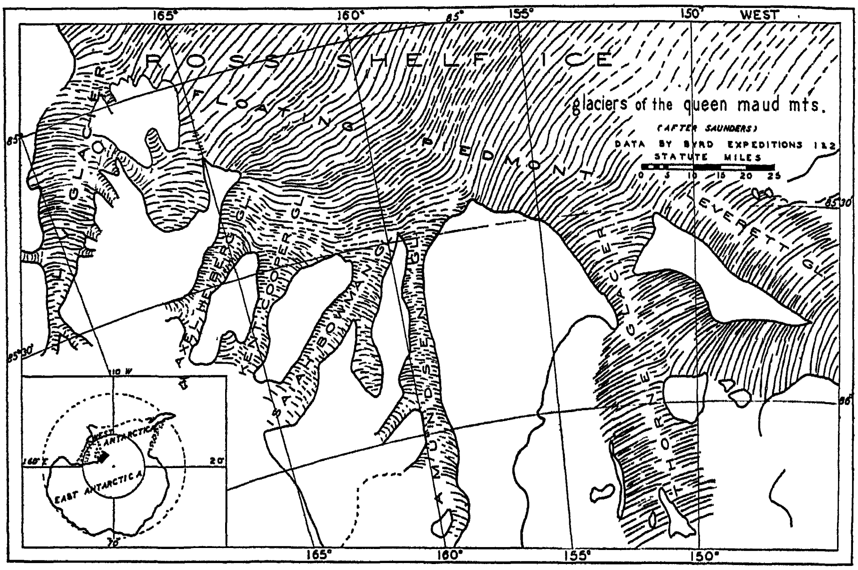

FIG. 5. Outlet glaciers of the Queen Maud Mountains.

Shelf Ice is divided into several parts by a few low nunataks. Only the western part of the channel with a width of eight miles is completely unobstructed.

None of these outlet glaciers in the Queen Maud Mountains shears its way into the shelf ice. No more did we find one spreading out to form its own individual foot. On the contrary we found a great common piedmont resulting from the welding together of all the ice tongues from Liv to Thorne Glaciers. This welding appeared to be primarily brought about by the northwestward thrust from Leverett Glacier and other and as yet unknown ice streams to the north of it.

The thrust of the ice tongues entering this piedmont normally pushes it up into a series of low open folds. These are for the most part of limited extent but below Liv Glacier low, gentle anticlines from fifteen to twenty feet high and from one-half to three-fourths of a mile apart were found about twenty miles from the mouth of the glacier. These wrinkles became more pronounced and of course closer together near the mouth of the glacier itself and according to measurements made by Thorne some of the anticlines were as much as five hundred feet higher than the synclines adjacent to them. The behavior of Axel Heiberg is unusual in that its energy as it enters the shelf ice seems to be largely dissipated in one abrupt crevassed monoclinal slope.

One of the most conspicuous features of all types of glaciers of the Queen Maud Mountains is the complete lack or scarcity of debris on top or within the ice. We found no morainal accumulations on the main part of any of the glaciers and only a few had developed weak lateral moraines (Pl. II, Fig. 1) which disappeared below the Ross Shelf Ice before they had hardly left the confining walls of their glacial valleys. Glacier tables, dirt cones, and other features which are typical of mountain or valley glaciers in lower latitudes were lacking. We found one huge solitary erratic granite boulder five miles out on the piedmont in front of Thorne Glacier.

Even as the stagnant waters of a lake allow the outlet streams to leave without a load of sediment, so does the nearly stagnant mass of ice which covers the great South Polar Plateau furnish its outlet glaciers with few tools. The limited rock exposures in the Queen Maud Mountains do not promote the accumulation of much debris by frost action. Such slow moving masses of ice as the Antarctic Glaciers are weak agents in the matter of quarrying rocks from below and along the sides of the glaciers.

Due to the persistently low temperatures, rates of firnification and the eventual formation of glacial ice are undoubtedly very slow, so that the glaciers are normally covered with great thicknesses of snow. The pronounced drying and warming effects of the foehn winds which sweep down the mountain sides and particularly through the great glacial troughs make such winds important agents in dissipating the snow and ice, so that locally glacial ice is exposed. The chief areas of such exposed ice are, therefore, found rather below the glaciers than in their main masses. In places the piedmont is covered with hard

packed snow like the rest of the shelf ice; in other places it possesses an icy rippled surface and in still others there are to be seen great expanses of white, bubbly glacial ice. So great is the rate of dissipation along the piedmont zone in the summer months at least, that this zone is generally lower than the shelf ice to the north which lies beyond the effects of the fœhn winds.

Crevasses are widely developed and are common to all the glaciers and the piedmont zone below them. The outlet glaciers are characterized by heavily crevassed slopes separated by flatter stretches. In places the ice descends in veritable falls with great toppling crags and blocks across which there is no passing. Such heavily crevassed areas reveal the bluish glacial ice beneath its cover of firn and snow.

The piedmont was found to be generally crevassed particularly in the areas below the outlet glaciers. The largest and most persistent crevasses found anywhere were in the piedmont zone below Liv Glacier. Great as were the crevasses here and in glaciers such as Liv they yet did not seem to be indicative of rapid movement. Their abundance is perhaps more a reflection of the rigidity and inflexibility of the ice at the extremely low temperatures of Antarctic conditions than an indication of rapid rates of movement. Many of even the largest crevasses over large areas were filled with snow and covered with hard, even icy snow in such fashion as to indicate that they had not been disturbed for a long time. Though there was no opportunity to make quantitative studies of the rates of flow of any of the Queen Maud Glaciers, there is no reason to suppose that they move any faster, if as fast, as those of South Victoria Land much farther north. This would mean that a rate of three feet per day for these glaciers would be a very generous estimate.

There has been much speculation as to the origin of the great outlet glacier valleys with their often simple outlines. A review of the various theories together with my own observations and conclusions that they are chiefly of tectonic origin, is set forth in detail in a paper presented before the Pan Pacific Science Congress in San Francisco in August 1939. This paper is now in press.

Shelf Ice

Probably no feature of Antarctic glaciology or geology has provoked as much interest among explorers and has brought

forth such great variety of opinions as to their origin as the shelf ice masses. Because the first such area known, The Ross Shelf Ice, prevented Ross from sailing farther south, he called it a "barrier" (Pl. III, Fig. 1). The term, Barrier or barrier ice, was long used, particularly by British explorers, but the designation "shelf-ice" proposed by Nordenskjöld is much to be preferred and is now generally used. It is at once descriptive of the shape and position of such masses, that is, they all have a shelf-like shape and lie in the main over the continental shelf.

Shelf ice masses are peculiar to Antarctica and constitute the largest floating masses of ice in the world. The larger examples are notable for the flatness of their surfaces and their thinneess in comparison with their great extent. They are difficult to classify because such attributes of a glacier as nourishment, movement and wastage which are normally characteristic of different parts of the ice mass are in typical shelf ice formations in the same general mass and are in a state of almost perfect balance.

Various formative processes predominate in different areas of shelf ice and hence various theories of origin have originated. In no case can it be demonstrated that all of the processes are represented in any one mass of shelf ice. Stranded ice bergs in shallow water may in rare cases form the nucleus for the growth of shelf ice. Embayments or inter-island areas in shoal waters may furnish the protection necessary for their development and maintenance, and ice tongues afloat may provide supporting ribs between and about which the shelf ice may develop.

The Wordie Shelf Ice and the Shelf Ice of King George VI Sound.—Both of these relatively small areas of shelf ice about the Pacific region of Palmer Land clearly owe their existence chiefly to the protection afforded by the embayments in which they are formed. The Wordie Shelf Ice fills the southeastern part of Marguerite Bay and merges laterally with fringing glaciers of the mainland.

Pressure ridges one hundred feet high, tilted and stacked blocks which he calls "Stonehenge ice" and circular bowl-shaped depressions as much as a mile in diameter which he described as "ice calderas" characterize the surface of King George the Sixth Shelf Ice according to Fleming (1938, p. 511–512). Such spectacular phenomena as these are not characteristic of the main part of the larger areas of shelf ice known from

other parts of the continent. Indeed some of the features described by Fleming make this region unique.

The Nordenskjöld and Weddell or Filchner Shelf Ice.—Fringing the Atlantic coast of Palmer Land and most of its length and extending southward and eastward across the head of the Weddell Sea is one of the two largest masses of shelf ice known. The southern part of this mass is known as the Weddell or Filchner Shelf Ice and its dimensions and extent are still almost entirely unknown. The northern part is named for the great Swedish explorer, Nordenskjöld (1920, p. 150–168), who sledged across it in 1902. It was the examination of this region which led this able scientist to propose the term shelf ice and to propound one of the two principal processes by which such masses of ice are formed.

Nordenskjöld supposed that shelf ice areas such as the one which bears his name had been formed by the accumulation of snow upon a base or raft of fast ice, that is unbroken sea ice which is moored to the land. The shoal waters of this region, the numerous islands, and the irregular coast line all combine to afford the protection necessary for this process to operate, and there is no question but that it explains the major development of this particular formation. As snow accumulates upon such a sea ice base the latter is depressed and eventually is melted away from beneath so that the mature shelf ice becomes a mass of snow and firn largely saturated with sea water.

The supporting islands which promote this type of shelf ice formation are either covered with ice caps or island ice which add some nourishment to the shelf ice by outflow around their margins. Indeed such marginal extensions of the island ice may have composed a part of the original framework for the shelf ice, provided the complete glacierization of the islands preceded the formation of the fast ice base.

The Stancomb-Wills Shelf Ice.—Very little is known of this shelf ice mass or promontory as Shackleton (1920, p. 26) who discovered it described it. It took his ship, the *Endurance,* several days to round it and it appeared to him to be essentially a "great mass of ice at least fifty miles across stretching out from the coast and possibly destined to float away at some time in the future".

West Ice.—There seems to be no good reason why ice bergs

might not be stranded in polar waters and persist long enough as islands to permit the formation of fast ice between them and thus become the nucleus for shelf ice areas comparable with the Nordenskjöld Shelf Ice. The relatively small West Ice of Kaiser Wilhelm II Land originated in some such fashion according to Drygalski (1910, p. 1–44). Because of the shallowness of Pasadowsky Bay, ice bergs were stranded on the shallows and the intervening areas were partially or completely filled by drifting snow. The partial filling of these lanes would account for the uneven surface of the ice and the fact that they are ice bergs would account for the unusual blue color. Since the exposed portion of shelf ice masses is everywhere always snow or firn, they are normally dazzling white in appearance rather than blue. About this nucleus of ice bergs, areas of fast ice have developed newer areas of shelf ice to the north. According to Drygalski at the present time there is no direct connection between West Ice and the great inland ice sheet to the south of it. On the other hand Phillippi (1907–1908, p. 9–11) believes that West Ice actually forms a portion of the margin of the inland ice sheet today. Its unevenness is due to much weathering which has removed sufficient snow and firn in places to reveal the blue color of true glacial ice.

The Shackleton Shelf Ice.—Much interest derives from this little known area of shelf ice because of the striking role played by ice tongues in its present appearance and probable origin. It appears to the best example of shelf ice which owes its existence chiefly to the presence of stiffening ribs of ice projected seaward from the inland ice. Of course the presence of such important islands as David, Henderson and Masson within the shelf ice, indicate that the accumulation of fast ice about such supports has been locally important at least. But that the shelf ice is fed to a great extent from the inland ice by glacial tongues is evident from the role played by Scott and Demman Glaciers. Indeed if Terminal ice Tongue is a prolongation of Demman Glacier (a relationship which is surely not apparent from the map) as Wild believed, it represents a single rib penetrating the whole width and far beyond the shelf ice itself.

In describing the effects of entering glaciers Wild (Mawson. 1914, p. 95–96) says a "a glacier from the inland pushing out from its valleys had broken up the shelf ice on which we were

traveling to such an extent that nothing without wings could cross it''. And again he says ''the Demman Glacier moving much more rapidly than the Shackleton Shelf, tore through the latter and in doing so shattered both of its own sides and also a considerable area of the larger ice sheet. At the actual point of contact was an enormous chasm over one thousand feet wide and from three hundred to four hundred feet deep in the bottom of which crevasses appeared to go down forever''.

Ross Shelf Ice.—Greatest of all floating masses of ice is the Ross Shelf Ice which covers the head of the Ross Sea to an average depth of 350 miles. Its seaward margin or the barrier stretches from Ross Island off the coast of South Victoria Land to King Edward VII Land—a distance of five hundred miles. Its area is approximately 200,000 square miles and our present knowledge leads to the belief that it is floating throughout most of its extent. Such crevassed areas as those we crossed on our sledge journey near latitude 82° indicate that it is locally grounded by low islands or reefs. Such phenomena are, however, quite uncommon.

For its size the Ross Shelf Ice is perhaps the flattest area on earth. From the traverses made along the western margin of it on their sledge journeys toward the South Pole by Shackleton and by Scott, Wright and Priestley derive a mean value of one hundred and fifty feet as the average height above sea level. Our sledge journey across a much wider part of it between the Bay of Whales and the Queen Maud Mountains gave a mean altitude of three hundred and ninety feet (Gould, 1935, p. 1367–1393). The average of these two values gives two hundred and twenty feet as a reasonable estimate of the whole mass. Like the inland ice itself the shelf ice surface is everywhere one of snow which rarely shows even a trace of granulation. It too is generally hard-packed and roughened by the prevailing winds into sastrugi though there are occasional areas covered with soft loosely-packed snow. We crossed one such beyond latitude 82° south on our sledge journey.

Except for its marginal features, we were aware of no irregularities in the shelf ice which merit the name of relief features. The great covered Roosevelt Island which lies south of the Bay of Whales is in reality an example of island ice. Along the foot of the Queen Maud Mountains the outlet glaciers push the shelf ice up into a series of undulations or folds as pointed out earlier.

That the shelf ice is in motion is demonstrated by the fact that the latitude of its seaward margin has not changed materially in the last thirty-five years. Yet ice bergs have been discharged or broken off from much of its front during the summer months of these years. That it is in motion along the foot of the Queen Maud Mountains is demonstrated both by the pressure rolls just mentioned and the crevasse systems of the piedmont. The directional factor in the movement of the piedmont zone along the Queen Maud Mountains is the tremendous thrust from Liv Glacier and possibly other nearby but as yet unknown ice streams to the north of it. In fact it appears likely that the major contributions of glacial ice to the Ross Shelf Ice as a whole come from West Antarctica rather than from the inland ice of East Antarctica through the great Antarctic horst. The most apparent cause of motion in the shelf ice is, therefore, the thrust of these entering masses of ice. To this should also be added the thinning or expanding movement induced by differences in thicknesses, which some investigators believe to be equally important. Such differences might be induced by marginal increments from the surrounding mountains or by differences in the amount of precipitation. Certainly in a mass of ice freely flowing on the frictionless surface of the sea conditions for rapid deformation by flow would approach the ideal. The extraordinary flatness of the shelf ice surface argues for the importance of such lateral flow though Wright (1925, p. 202) believes it to be due chiefly to the existence of an effective water circulation beneath the shelf ice. He points out that downward bulges would cause increased local melting and a quick equalization of differences in thickness. Whatever the relative importance of the various factors causing flow may be, it is obvious that their effect is additive and directed toward the seaward margin of the shelf ice. The rate of movement of the northward margin is, therefore, far in excess of the rate of flow of the entering glaciers. Accurate measurements made near the western margin of the shelf ice by measuring the position of a depot left by Captain Scott in 1903 and relocated by Shackleton six and one-half years later gave a value of four feet per day. By careful measurement of the changes in the Bay of Whales between the year 1911 when Amundsen mapped it and 1929 when we surveyed it, a daily rate of five feet is revealed.

It should be pointed out that both of these rates were determined near land margins of the shelf ice and therefore represent minimum and probably not even average rates for the front as a whole. Debenham (1925, p. 216) pointed out that changes in the front between the survey of Scott in 1902 and the later one in 1911 indicated a rate of no less than twelve feet per day. Very likely accurate measurements in the unhindered parts of the shelf ice front midway between the Bay of Whales and Ross Island will yield even larger figures than these.

Origin of Ross Shelf Ice.—Except for the limited areas of white, bubbly ice exposed in the piedmont along the foot of the Queen Maud Mountains, we found the shelf ice everywhere composed of firn and snow. This statement is based upon observations of the northern margin, of sections revealed in crevasses and of tilted bergs which floated into the Bay of Whales. Of course the absence of glacial ice above sea level along the northern margin or barrier itself gives no clue as to its fundamental composition for the exposed portions of all Antarctic Glaciers which reach the sea are normally snow covered. Furthermore the distribution of snow and firn showed little proportional distribution with depth. In other words firn might be found near the top to be underlain with great thicknesses of unchanged snow before another zone of firn was visible. Though it is much less easy to draw a sharp line of demarcation between firn and snow than between firn and glacial ice, the exposed clifts of shelf ice were clearly composed of irregularly disposed masses of firn and snow showing little or no stratification. Scott regarded the snow and firn as but a superficial veneer and believed the shelf ice to be a floating piedmont—a greatly shrunken remnant of a much larger piedmont aground which existed at the culmination of glaciation. David and Priestley (1914, p. 138) elaborated Scott's idea and concluded that the Ross Shelf Ice was formed of "thicked ribbed" ice with transverse pressure ridges—the ribs being the glacial tongues projected into Ross Sea from the bordering mountains and the transverse pressure ridges being the folds produced by the thrust of these entering ice tongues. They said "it may be compared to a shield formed of a wickerwork frame covered with hide, in which the rods represent the glacial ice jetties and the smaller osiers the pressure ridges, while the drift and fallen snow represent the hide". Thus they

believed the whole to be essentially underlain by glacial ice thinning from the entering glacial ribs toward the northern margin. They assign a minor role to the accumulation of snow upon a sea ice base. In his later work with Wright, Priestley (1922, p. 167, 220) is inclined to grant a much larger role to the accumulation of snow upon a sea ice base but concludes that "the area of Ross Shelf Ice occurring in the Ross Sea margin which can definitely be assigned to this cause is negligible".

From my own observations I believe the accumulation of snow upon a sea ice base to be much more important under present conditions than have the earlier investigators. That the shelf ice began by the prolongation of outlet glaciers into the sea as floating ice tongues is hardly to be questioned but that such ice tongues exist as stiffening ribs throughout the shelf ice today is quite another matter. We found no topographic evidence of such ribs. On the contrary it appeared that entering ice tongues expended their energy quickly in overcoming the inertia of the shelf ice and pushing it up into folds. We saw no evidence of any tongue being powerful enough to shear its way bodily into the shelf ice after the manner in which Demman Glacier breaks its way into Shackleton Shelf Ice. It appears to me that the glaciers which enter the head of Ross Shelf Ice develop short rapidly thinning ribs which coalesce quickly to form relatively thin floating piedmonts rather than to form long, skeletal ribs projected far into the shelf ice. The extent to which such glacial ice becomes a permanent part of the shelf ice is dependent upon the amount of ice which enters and the rate at which the shelf ice is depleted by ablation from above and by melting from beneath. The limited catchment areas of all the glaciers which enter the southern and western margins of the shelf ice suggest that a much smaller amount of ice is added in this way than might be inferred by the actual size of the entering glaciers themselves for on every hand we had evidence that the rate of movement of these glaciers is slow.

In the summer months there is no doubt that ablation along the foot of the mountains is very great and temporarily at least exceeds the supply of new snow and ice for the piedmont zone in many places is lower than the shelf ice to the north of it. This local phenomenon is produced by the effective fœhn winds which sweep down from the Polar Plateau through the great glacial troughs.

Wright and Priestley (1922, p. 139) conclude that there is some melting from beneath the ice and that the amount is comparable with additions from above and of the same order as the probable additions through entering glaciers. Since there is no piling up of ice at the foot of the mountains lateral flow and melting from beneath together with ablation from above must be sufficiently rapid to offset any such effects. Melting is probably, therefore, not unimportant even at this great distance from the shelf ice margin. Granted then an effective circulation of sea water beneath the shelf ice it appears that not only does the sea ice raft upon which snow accumulates to form shelf ice melt away but likewise the outward moving piedmonts of glacial tongues become rapidly attenuated and melt away leaving their burden of firn and snow to soak up sea water even as do the sea ice rafts.

In the Bay of Whales and in Sulzberger Bay west of King Edward VII Land new shelf ice was clearly forming by the accumulation of snow on a sea ice base. In general it appears that in the northeastern section of the Ross Shelf Ice the scattered islands together with the irregularities of the coast have exaggerated the process of shelf ice formation as compared with other known areas. According to Wade (1937, p. 594) material additions have also been made by seaward extensions of ice caps covering the islands. Perhaps in the largely unknown region between King Edward VII Land and the Queen Maud Mountains sheets of ice move from West Antarctica bodily into the shelf ice so that there are here great areas of true glacial ice supporting the fields of snow and firn that are everywhere the visible parts of the shelf ice.

When the level of the inland ice was higher, as was certainly true at the culmination of glaciation, outlet glaciers would have been more active than now and would have supplied much greater quantities of glacial ice to the shelf ice. They may then have been actually apparent as ribs.

If present relationships between precipitation and temperature may be taken as an index, then the waning of the ice was brought about, not by rising temperatures and excessive melting, but by colder temperatures and decreased nourishment. Such cold temperatures would work to the advantage of form-

ing and preserving extensive areas of fast ice. Not only would the air temperatures be lowered but so also would those of the sea. The rates of all methods of wastage would be reduced, sufficiently perhaps to offset the declining snowfall which would be largely conserved on the fast ice areas to form new areas of shelf ice.

From the above statement it appears that optimum conditions for the formation of shelf ice by land ice extensions and by accumulation of snow on a fast ice base are not identical.

Viewed as a whole then the Ross Shelf Ice seems to be chiefly the result of two sets of causes which may have alternated in importance but which have probably never operated separately. Ice tongues and perhaps even ice sheets from the coastal margins have functioned along with areas of fast ice as basements upon which snow has accumulated to become compacted in part into firn. Progressively more rapid melting from below as the moving shelf ice nears its northern margin has long since obliterated the youthful pattern of the Ross Shelf Ice. Whatever the formative process may have been the original material has of course long since been dissipated. The present shelf ice is, therefore, a second generation product of parents which have given no identifying marks as to its genealogy.

Independent or Detached Ice Forms of the Coastal Margin

All coastal areas which are high enough so that their seaward margins, at least, are not overwhelmed by the inland ice exhibit independent glacial phenomena. It is, of course, in such rugged mountainous areas as the great Antarctic horst which rises high above the level of the inland ice that the greatest variety of such features are developed. Indeed it is doubtful if there are any phenomena of mountain glaciers anywhere in the world which are not represented here. In places the coastal mountains are still so swamped with ice that examples of highland ice or Spitzbergen ice are formed. These have been described from South Victoria Land and extensive examples were also observed by us in the Queen Maud Mountains. Likewise in high more level stretches of the broad horst, extensive ice fields are to be seen from which distributaries descend to lower levels after the manner of the plateau or Norwegian type of glacier.

Mountain Glaciers

There are all manner of modifications of the mountain glacial type from the snow drift or nivation glacier which is no more than a mass of firn, to large and complex mountain glaciers. Such terms as cliff glacier, hanging glacier, avalanche ice and reconstructed glaciers are in themselves descriptive of some of the many types. Most conspicuous of all such mountain glacier features along the Queen Maud Mountains are the true mountain glaciers with characteristic dendritic patterns. Pl. IV, Fig. 1 is a very small example of such features. In the absence of the Ross Shelf Ice such glaciers would probably develop as expanded foot glaciers below the mountains or coalesce with others to form piedmonts. Under present conditions they promptly lose their identity when they enter the shelf ice. Not only are such glaciers as this, which derive their nourishment from local firn fields, common in the coastal slopes of the Queen Maud Mountains but they are widely developed as tributaries to the much larger outlet glaciers. Pl. I of Liv. Glacier shows a number of such tributaries.

Wright and Priestley (1922, p. 155) describe as a rather uncommon type in South Victoria Land, mountain glaciers which were once tributary to much larger streams of ice. Due to the present recession and shrinking of the ice these have become detached or independent and now spread out at their lower ends to form bulb or expanded foot glaciers.

Cirques and Cirque Ice.—Among the most common and likewise most interesting of all glacial features are the cirque, corrie or cwm glaciers or ice as they are variously called. They excite particular interest here because of the fact that many are empty or but partially filled and they occur at all levels in the mountain areas whereas one normally expects to find them only in the higher levels of mountainous regions (Pl. II, Fig. 1). As a matter of fact, some of them are so low as to be completely or partially submerged either by the ice of the Ross Shelf Ice, if they are developed along the front of the mountains, or by the ice from larger glaciers if they occupy positions along their sides.

Assuming that cirques have been produced by the combined progressive action of nivation and bergschrund action, they can-

not possibly be explained as having been developed under present climatic conditions. Of course there is some bergschrund action where the cirque ice is so thin that melt water can penetrate to the rocks by way of shallow crevasses before it freezes. We found bergschrund so coarsely developed in a few places that we could not ascend mountain glaciers to reach the rocks. These were, however, exceptional cases. For the most part all types of mountain glaciers including cirque ice are generally lacking in marginal crevasses.

Priestley (1922, p. 41–47) believes that the vast majority of Antarctic cirques were formed in a somewhat different fashion. He supposes that they were preceded by the widening and the deepening of gorges on cliff faces through a combination of freeze and thaw action. The snow which accumulates in such depressions during winter would inevitably involve such frost action around their margins as the summer approaches. Provided melting continued the whole mass would then be dislodged with great suddenness and would carry much debris with it. This process alone would result in the formation of half funnel-shaped depressions which are deep enough to maintain permanent masses of ice around which bergschrund action could continue.

Priestley further believes that cirques which become submerged by the over-riding of great ice sheets have their cirque-like characteristics accentuated. Because the ice would be thickest where the walls and the base of the cirques meet, erosion would be most rapid there and the shape would, therefore, be accentuated rather than softened. He also believes that cirque shapes might be initiated "without any other predisposing factor than the occurrence of a shallow gully".

Both of these ideas seem to me quite untenable. It seems highly improbable that an over-riding mass of ice would be effective in deepening a cirque after the fashion described by Priestley. On the contrary it is more reasonable to suppose that a cirque would be reduced rather than accentuated. I have seen cirques in the Holtsenborg district of Greenland planed off by later advances of the inland ice leaving the cirque walls rough as they have been left by sapping of cirque ice rather than polished and striated as would have been the case according to Priestley's theory. A cirque filled with ice might easily be over-

ridden by superior floods of ice which would have no effect except to reduce the height of the walls. Priestley's theory assumes that the ice of the cirque would be thrown into eddies which would erode at the bottom and along the sides. If cirques could be initiated by ice floods in this manner they should be found in greater abundance in such glaciated regions as we find in North America. As is well known, however, such features do not abound.

Taylor (1922, p. 174–184) credits the formation of Antarctic cirques to the normal process of nivation and bergschrung action but believes they were formed at some earlier time when climatic conditions were much less severe than now. It is certainly true that the present climatic conditions in the Queen Maud Mountains at least are far too severe to permit the formation of such an abundance of cirques as were observed, by these normal processes which are after all but processes of frost action.

It seems to me most reasonable that Taylor's hypothesis is correct and that the key to the whole matter is held by the submerged cirques. Clearly if they were formed by normal processes, they will have been formed at higher levels and their present submergent condition is a demonstration of the fact that their formation preceded the major glacierization of the continent which of course produced the general lowering which is evident from the present over deep position of the continental shelves.

It seems reasonable to suppose, therefore, that these submerged cirques were formed early in the advancing hemicycle of glaciation when climatic conditions were much less severe than now. At the time of their formation they were higher level features than now and for the most part were probably carved out before the thickening mantle of ice behind the horst had greatly depressed the entire land mass. That the cirques as we see them today are inherited from an earlier stage of Antarctic glaciation when climatic conditions were much milder than now seems to me the only reasonable explanation.

Antarctic Ice Foot

This term is used here to include all ice forms of the coastal areas which are not at present attached to or directly nourished either by the inland ice or by mountain glaciers from the coastal

mountains. Ice foot, ice foot glacier, snow drift ice, dune glacier, fringing glaciers and piedmont are the principal names which have been generally applied in different places to ice masses that cling to the shorelines of such coasts as the shelf-ice free western shores of the Ross Sea. These features are all similar in that they are notably narrow in their seaward direction in comparison with their coastwise length. Likewise no matter how different the process by which they were formed all may have at some time or other received considerable nourishment from drifting snow. Due to the peculiar character of the great anticyclone over the inland ice, the snow is blown radially toward the margins of the continent. Naturally then the seaward exposures of all coasts not otherwise overwhelmed with ice form lees for the accumulations of such wind driven snow. The typical ice foot, therefore, is widely developed about the whole Antarctic Continent. It may be well to point out that wind is often a much more important factor than temperature in determining the locus for the accumulation of snow and thereby of glaciers.

The ice foot proper or the tidal platform ice foot is a ribbon of fast ice separated from the sea ice by tide cracks, where sea ice happens to be present. Such features may be up to seventy or eighty feet deep but are usually less than twenty. Where the coastal waters are deep they may be but a few feet wide and where there are shoal waters they may be several hundred feet wide. Both the thickness and breadth of such ice foots may be increased considerably by spray ice. Likewise an ice foot formed normally of sea ice may be weighted down over such prolonged periods by the accumulation of drift snow on its upper surface that the original structure may be melted away from beneath. Such "fringing glaciers" have been variously called snow drift ice or snow drift glaciers and dune glaciers. Of course snow drift glaciers may be formed from drifting snow at any altitude where the snow finds permanent lodgment, so this term is not particularly descriptive for ice masses occupying the ice foot position.

In places the ice foot region is occupied by narrow masses of ice which do not fit any of the above descriptions. Ferrar (1907, p. 61–62) notes fringes of shore ice which show the blue bands and granular structure of glacial ice and which is free from salt. They range from six to three hundred feet high and from ten

yards to a mile wide. It has been suggested that the retreat of an inland ice sheet or at least large valley glaciers from which they were formed and nourished has left these masses of ice stranded or beheaded. In other words they were originally piedmont glaciers in the true sense and some investigators prefer still to identify them as piedmonts though they are no longer active and show no genetic relationship to such ice streams as normally develop piedmont glaciers.

Fleming (1938, p. 510) describes a narrow belt of fringing glaciers which occupy considerable areas between the foot of the mountains and the coast along the western shores of Palmer Land. These glaciers are short in the direction of flow and end on rock at sea level in cliffs from sixty to ninety feet in height but occasionally reach two hundred feet. These glaciers are below the present snow line and are holdovers from the time when the bays and islands off the coast were filled with shelf ice which merged with the now relic fringing glaciers even as such glaciers now merge with Wordie Shelf Ice.

Perhaps in much the same sense the coastal extensions of glaciers and similar forms along the sea-exposed coast of South Victoria Land south of Cape Robertson may be thought of as inheritances from the Ross Shelf Ice. For when the Ross Shelf Ice extended much farther northward, all such features would have been incorporated within it.

Ice Forms of the Marginal Islands

Depending on their size and the extent to which they are being glacierized Antarctic islands may exhibit a variety of ice features which in their entirety are miniature replicas of the great continental ice sheet and its marginal developments. Where nourishment is insufficient to maintain complete ice cover, the valley depression may be occupied by diminutive examples of various types of mountain glaciers. When the island is completely swamped by its ice cover the ice assumes the dome-shape of a local ice cap or the island ice of Wright and Priestley (1922, p. 147). Such ice caps may push their margins into the surrounding seas to be disposed of by the winds and the waves or to be incorporated into shelf ice areas as appears to be the case along the eastern coastal regions of Palmer Land. Though the glacierized coastal islands are in warmer temperature zones then

the continent itself, they receive much more precipitation. It, therefore, happens that many such islands show a much heavier glacierization than is to be found on the Antarctic mainland itself.

Former Extent and Activity of Antarctic Glaciers

Former Thickness

The first expedition to winter in Antarctic regions, *The Belgica* (1908, p. 59–64), from 1897 to 1899 brought back the first observations demonstrating that Antarctic glaciers were formerly more extensive than now. All subsequent expeditions have corroborated this evidence from widely different parts of the margin of the continent and have added a great deal of quantitative results. To mention but a few outstanding ones, Nordenskjöld (1905, vol. 2, p. 217) discovered erratic boulders on the sides of a nunatak in Palmer Land showing that the ice was formerly one thousand feet higher than now. Drygalski (Phillippi, 1901, vol. 1, p. 51) likewise found erratic boulders on the slopes of the volcana Gaussberg in Kaiser Wilhelm II Land 1,150 feet above the present level of the ice.

Far more evidence as to the former stand of the ice is available from the McMurdo Sound region of South Victoria Land than from any other parts of the continent. After an exhaustive examination of the facts, David and Priestley (1914, p. 290) conclude "it may be easily assumed that the minimum amount of deglaciation from 85° south to near Mount Nansen amounts to approximately the order of about 1,000 feet in the case of larger ice masses like the Ross ice barrier and from two thousand to three thousand feet in the case of the outlet glaciers".

Though there is no evidence that such high portions of the Antarctic horst as the Queen Maud Mountains in the vicinity of Mount Fridtjof Nansen have ever been overtopped with ice, glaciated surfaces are abundant on the lower slopes of flanks of these mountains. Eastward from Mountain Fridtjof Nansen, the horst becomes lower and from about longitude 160° eastward the range must have been deeply overwhelmed with ice at the culmination of glaciation. Even now it is largely swamped and such glaciated mountain tops as that of Pl. IV, Fig. 2 is mute but indisputable evidence, both of the stupendous erosive power

of the ice and likewise of the fact that it must have been formerly of the order of at least eight hundred feet thicker than now.

In attempting an estimate of the amount of thinning of the great continental ice sheet itself one must not be misled by the large amounts of down wastage of the outlet glaciers. A slight rise in the level of a great reservoir of water will cause the outlets from it to rise disproportionately higher. Outlet glaciers from the reservoir of inland ice would doubtless respond in the same way to slight increases in the thickness of the main ice sheet, so that the maximum differences between the present ice levels and those of the past will be found in the valleys of the outlet glaciers. Probably the most accurate kind of measure is afforded by such evidence as that collected by Drygalski from Gaussberg. Here the inland ice was not forced into channels that caused it to achieve abnormal heights but rather it was able to move unhindered into the sea. It seems reasonable, therefore, that the lowering of the surface here gives perhaps the best single estimate of the thinning of the ice mass as a whole. In round numbers then the value of 1,000 feet may be taken as a reasonable estimate of the vertical thinning of the great continental ice sheet.

Former Area

As to the maximum horizontal or areal extent of Antarctic ice masses, evidence is much more scarce. David and Priestley (1914, p. 46) point out that at the culmination of glaciation when the Ross Shelf Ice was at least nine hundred feet thicker than now it not only rested heavily on the bottom of the Ross Sea but extended northward about two hundred and twenty miles farther than at present. At the maximum glaciation then the Ross Shelf Ice extended over a total length of some seven hundred miles. As to how far floating ice tongues or floating piedmonts may have extended into the sea about exposed parts of the continent can be only guessed. It is very doubtful if any type of extension of the inland ice could have extended for any great length of time beyond the continental shelf for here they would all encounter greatly increased depths of waters and an exhaustible supply of warm water which would have led to their rapid dissipation. Mawson (1935, p. 30) points out that under present conditions the submarine ridge which encircles much of

the continent, at distances generally from fifty to one hundred miles, is near enough to the surface so that it causes ice bergs to become stranded and to hold up the pack ice for periods of several years at a time Great outbursts of ice into the surrounding seas accompany the breaking up of these ice blockades. Though the culmination of glaciation in Antarctica probably did not coincide with the maximum extent of the great Pleistocene ice sheets of lower altitudes, the formation of the latter may have involved a sufficient lowering of sea level in Antarctic regions so that ice bergs would have become stranded for prolonged periods of time on these submerged ridges. Such a protective girdle about large parts of the continent may have afforded sufficient protection to the seas between the continental margin and the girdle so that they would have become covered with shelf ice. Such submarine ridges which Mawson believes to be great terminal moraines are represented across the mouth of the Ross Sea by the Pennell and Iselin banks. Though these latter features are too deep at present to interfere with the free movement of ice bergs across them the lowering of the sea level of 250 feet which seems a conservative estimate for the culmination of Pleistocene conditions which have enabled them to bring about the stranding of at least such bergs as would have come from the Ross Shelf Ice when it was one thousand feet thicker. The fact that these submarine ridges appear to be morainal deposits of the inland ice does not affect the above hypothesis for they would have been deposited before the culmination of glaciation in lower latitudes as will be pointed out later. Likewise if they are moraines they represent a former very great extension of the inland ice from which they were marginal deposits.

During the waning stages of a great continental ice sheet due especially to a diminishing in nourishment as is the case in Antarctica, the immediate effect would be a thinning of the ice mass as a whole. The distribution of available snow to the margins of the ice sheet by the glacial anticylone would tend to cause the eventual areal shrinkage about the margin to lag behind the down wastage from above. While the former areal extent of Antarctic ice is still conjectural that it has wasted away from above or thinned fully a thousand feet seems demonstrably true.

That the deglaciation of Antarctica continues and perhaps at an accelerated rate is evident by changes which have occurred

within the short lapse of time that various parts have been under observation. Assuming the accuracy of the chart made in 1841 of the front of the Ross Shelf Ice, this feature had retreated an average of twenty-five miles by 1902 when it was mapped by Scott. This amounts to such a very great rate of recession that the accuracy of Ross' map has been questioned. The work of successive British expeditions whose fields of observation have overlapped preceding ones, especially in the South Victoria Land region, demonstrate continued shrinking of the ice masses of the coastal regions in particular. Mawson's explorations have enabled him to revisit scenes of his earlier exploits and he too notes the increasing areas of ice free lands about the coast in the Australian sector.

Former Activity

A comparison of the rate of rock wastage due to processes of subaerial weathering and erosion with the apparent rate of denudation beneath the ice demonstrates that the present Antarctic ice masses in general have a conservative rather than a destructive effect. But that the present reduced glacial activity is typical of conditions which have obtained in Antarctica throughout its great ice age is manifestly untrue in the light of the evidence of the former stand of the ice just presented. The present deglaciation has revealed abundant and wide-spread evidence in the presence of glaciated rock surfaces of the former immense erosive activity of the ice.

Because the ice cover of Antarctica is still so complete the depositional records of former periods of activity are still hidden in part, perhaps, beneath the ice but probably more largely in the sea beyond the continental margin.

Glacial Erosion and Deposition

Stetson and Upson (1937, p. 58–66) who examined the bottom samples collected by the Second Byrd Antarctic Expedition point out that the entire continental margin of Antarctica is encircled by a zone of terrigeneous deposits. They identify the samples examined as a "marine till" and state that it has all of the characteristics of a till laid down to about 2,000 fathoms.

Taylor has suggested that the Pennell Bank which extends across the Ross Sea may be a part of a vast terminal moraine.

The same interpretation is equally applicable to the Iselin Bank to the north of Pennell. Mawson says that (1935, p. 30) "everywhere beyond the margin of the continent are to be found beneath the sea immense terminal moraines which outline the former maximum extension of the ice cape". While the submerged ridges are usually of the order of hundreds of feet high, off the Adelie Land coast, "the height of the main off-shore moraine is probably to be reckoned at something of the order of 3000 feet".

Granting Mawson's interpretation of these submerged ridges, when one compares the above dimensions with the Pleistocene deposits of the upper Mississippi Valley, whose maximum thickness does not exceed six hundred feet, it becomes apparent that Antarctic glaciation must have been much more intense and probably, therefore, much more prolonged than anywhere else in the world in Cenezoic.

Glacial Activity and Climate

The existence of glaciers is a function of the relationship between nourishment and wastage. In such areas as the coastal regions of Alaska, both are large but in Antarctica both are small with wastage of course being predominant. Because of the persistent low temperatures over the great inland ice sheet—always below freezing—the maximum amount of water vapor the air can hold is always small. Low precipitation is, therefore, everywhere the rule. At Little America on the Bay of Whales the annual snowfall was the equivalent of seven and one-half inches of water. Elsewhere under comparable conditions similar values have been found. It is only the reduced wastage due to low summer temperatures which permit such a low annual precipitation to maintain glaciers. Antarctica is not only a cold desert but it is likewise a relatively dry one.

Scott appears to have been the first to point out that a general rise in temperature would not result immediately in a decrease of glacial activity. On the contrary it would bring about increased evaporation which would be reflected in Antarctica in the form of more abundant precipitation so that glaciers would begin to readvance. It has long been recognized that the marginal islands about Antarctica show much more intense glacial activity than the mainland itself, though they are clearly in

warmer temperature zones. The excess precipitation which they receive over that of the inland ice offsets the fact that they are in regions of much higher temperatures where the wastage is naturally much greater than on the continent. Bouvet Island in latitude 55° is a conspicuous example.

It seems evident that the present frigid climate, which not only slows down glacial activity but likewise prevents more abundant precipitation, is the result not the cause of glaciation. With the lowering of temperatures in the advancing hemicycle of glaciation, permanent snow drifts would have appeared first in high levels. From such beginnings mountain glaciers would have developed and begun their progress to lower levels. In the meantime ice would have begun to accumulate on the polar plateau eventually to coalesce into great ice sheets and to begin its journey outward toward the continental margins. The growing and expanding mass of ice automatically accentuated the cooling climates which had brought it into being and through the operation of the glacial anticyclone produced drastic climatic changes in the surrounding regions. Persistent low temperatures and heavy precipitation being mutually exclusive, ice accumulation would come to a standstill and eventually the present processes of deglaciation through lack of nourishment would set in. If the climate of the earth continues to grow warmer, conditions will be reached which will cause Antarctic glaciers to readvance before they finally disappear through excessive melting. The records of such fluctuations of the ice as may have occurred due to varying temperatures are still hidden beneath the present ice or have been completely obliterated by it.

Though climatic conditions are still too vigorous for effective glacial activity, in Antarctica, they are certainly warmer for the earth as a whole then they were in Pleistocene. That the great Pleistocene ice sheets of lower latitudes and those of Antarctica should have reached their culmination at about the same time seems, therefore, to be an untenable thesis. Long before climatic conditions had become sufficiently severe to produce ice sheets of continental dimensions in North America and Europe, they must have begun to affect Antarctic ice masses adversely. The starvation processes due to intense cold which now characterize the Antarctic mainland must have had their beginning in early Pleistocene if not earlier. Certain it is that the effective

glacial erosion of Antarctica must have preceded the rest of the world of Pleistocene by a very great length of time. According to Wright and Priestley (1922, p. 183) the intercalated layers of ice and volcanic debris on the slopes of Mount Erebus on Ross Island at the northwest apex of the Ross Shelf Ice indicate that glaciation must have begun there in early Tertiary. It may, therefore, have been well under way before that in the high horst zone and on the high inland plateau so that the major erosional effects may have been produced in middle Tertiary but certainly not later than Pliocene.

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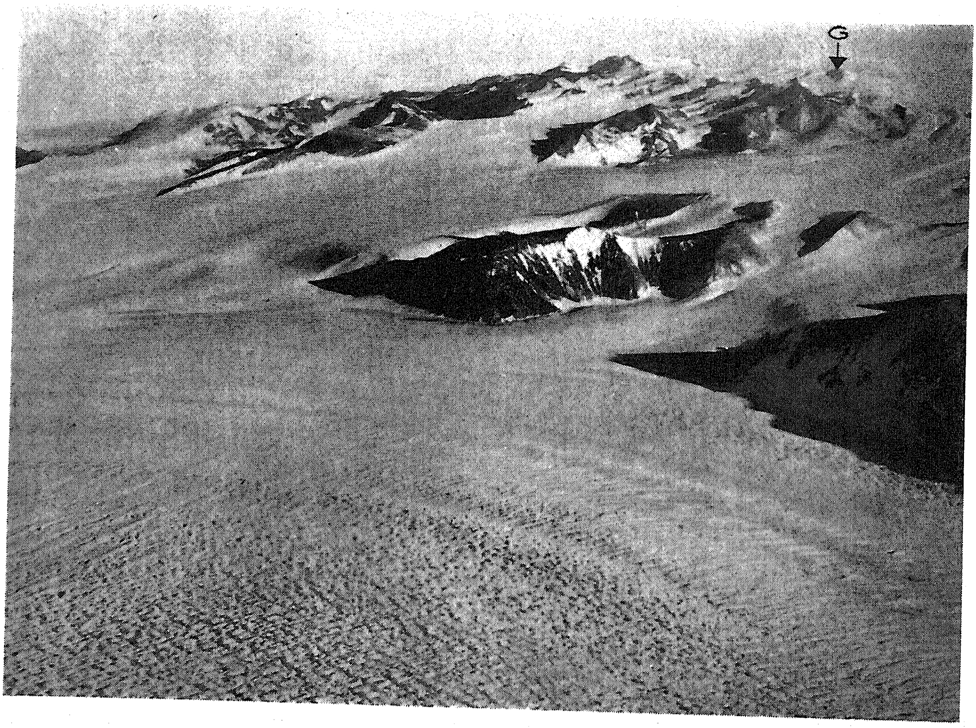
PLATE I.

GLACIERS OF THE QUEEN MAUD MOUNTAINS.

FIG. 1. Liv Glacier.

FIG. 2. Ice fields of the High Horst. (Note that G is the same feature in both figures.) Aerial photographs by A. C. McKinley.

PLATE I.



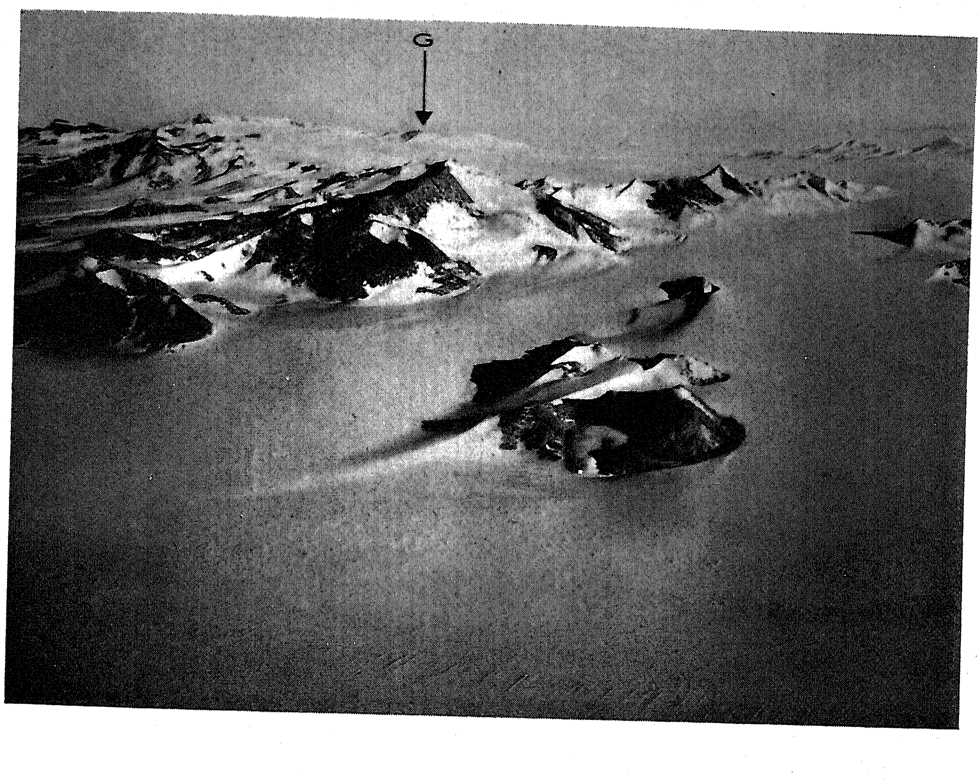


PLATE II.

SOME DEPOSITIONAL AND EROSIONAL FEATURES.

FIG. 1. Lateral moraine of Liv Glacier. Photograph by L. M. Gould.

FIG. 2. Cirques on the north face of the Queen Maud Mountains. Aerial photograph by A. C. McKinley.

PLATE II.

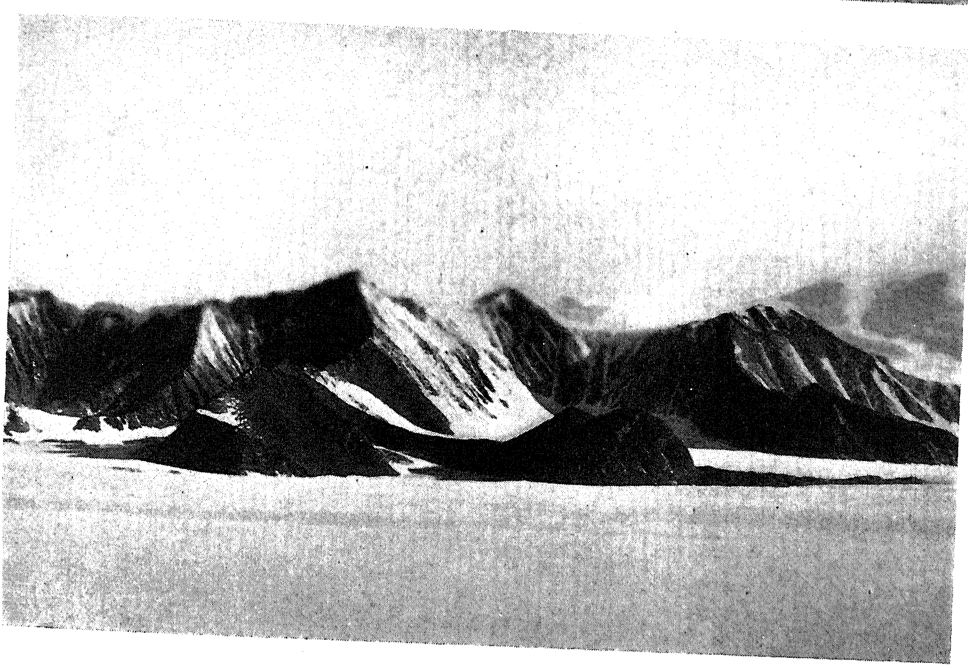


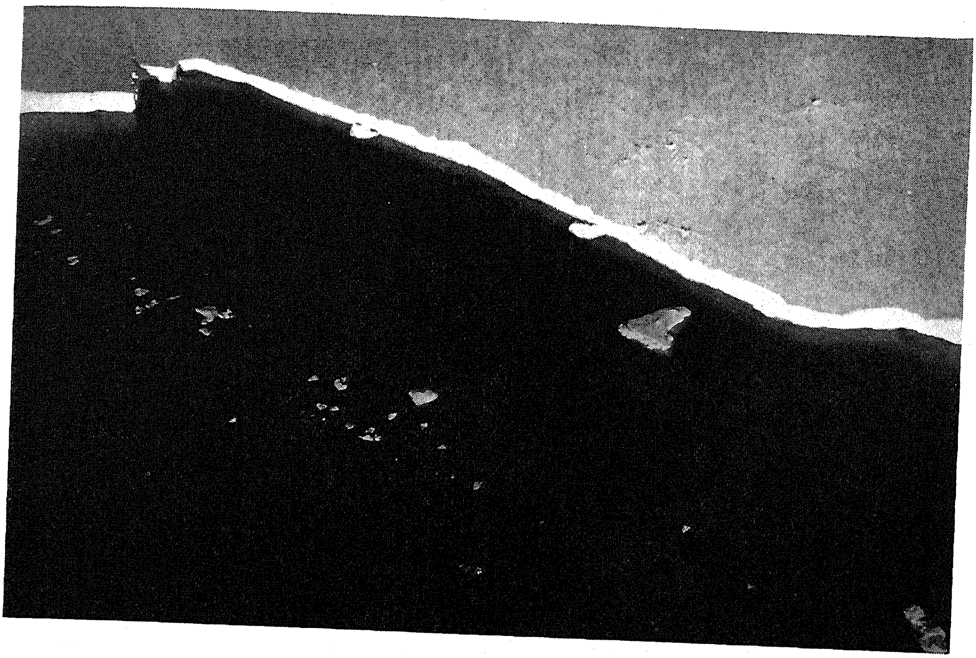
PLATE III.

Ross Shelf Ice.

Fig. 1. The northern margin or "barrier." Aerial photograph by A. C. McKinley.

Fig. 2. Anticline in young shelf ice of the Bay of Whales. Photograph by L. M. Gould.

PLATE III.



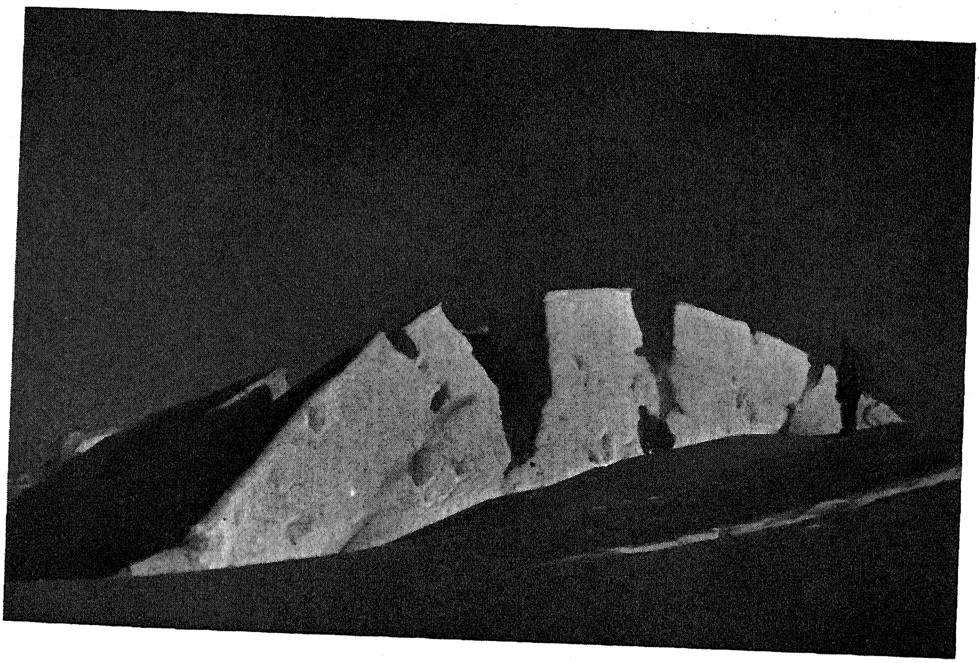


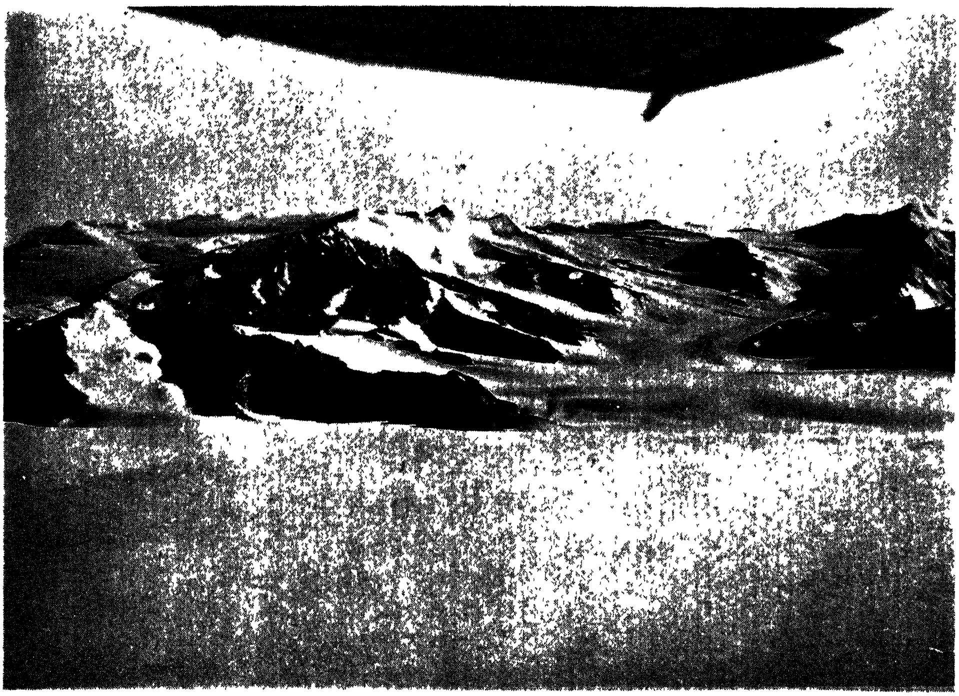
PLATE IV.

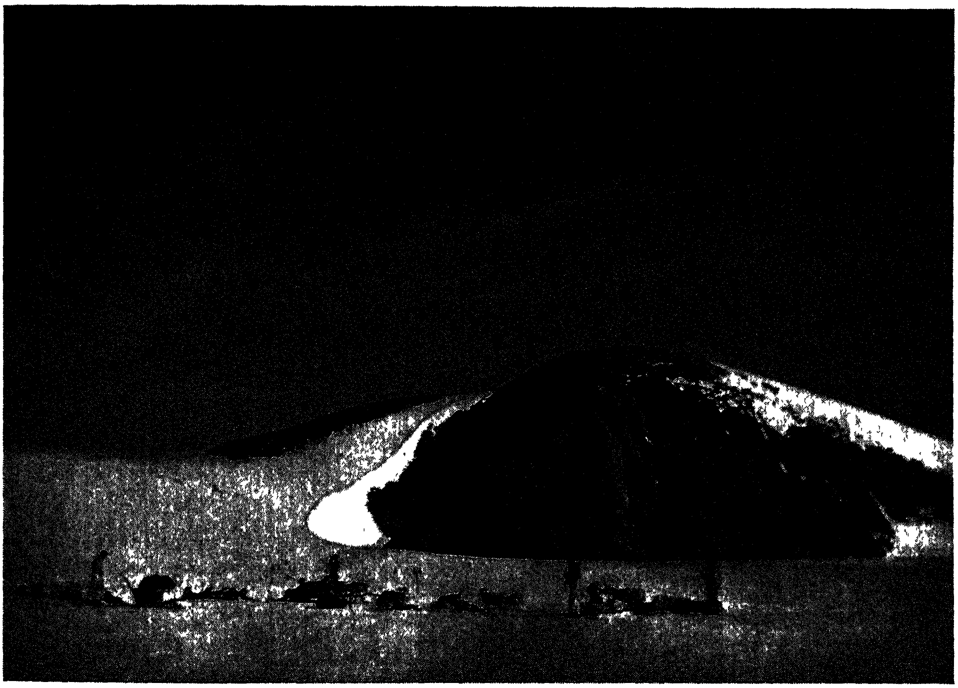
GLACIAL FEATURES OF THE FRONT OF THE QUEEN MAUD MOUNTAINS.

FIG. 1. Nameless mountain glaciers near Liv Glacier. Aerial photograph by A. C. McKinley.

FIG. 2. Glaciated mountain top near Thorne Glacier. Photograph by L. M. Gould.

PLATE IV.





THE ARCTIC VOYAGES AND THE DISCOVERIES OF DEHAVEN, KANE AND HALL

W. ELMER EKBLAW

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(Read February 24, 1940, in Symposium on American Polar Exploration)

AMERICAN exploration in the Arctic, which began with Lieutenant Edwin J. DeHaven's expedition of 1851 in search of Sir John Franklin and his ill-fated party, has extended more or less continuously down to our time, with several definite periods of intensive activity like the DeHaven-Kane-Hayes sequence in the fifties of the middle nineteenth century, and the Peary series of expeditions later, which stretched over a quarter of a century, and culminated with his attainment of the North Pole in 1909. It has been characterized throughout by daring, persistence, much mapping of newly-discovered lands, and much worthy scientific research, all in the spirit of the hardy American explorer whose centenary we celebrate by this symposium, and by recollection of his deeds and the long chain of exploits for which they have been incentive and exemplar.

For background to DeHaven's initial enterprise, it is necessary to revert farther into the history of exploration, back to the very beginning of the nineteenth century, when a young midshipman, John Franklin by name, after having fought in the bitter battle of Copenhagen, in July, 1801, obtained a berth on board the exploratory ship "Investigator" bound for the Southern Seas to survey the coasts of New Holland or Australia. After much good work and many adventures, he returned to England in 1804, hardened and experienced. He fought under Commodore Dance in the Straits of Malacca and under Lord Nelson at Trafalgar. He participated in the futile British attack upon New Orleans after the treaty close of the war of 1812–15. But young Franklin, by this time first lieutenant, still longed for exploration rather than naval service, and finding the Admiralty committed in 1818 to a program of polar research and survey,

he sought, and obtained, a commission with an expedition to Spitzbergen in search of a northeast passage to India and an attempt to attain the North Pole. Captain David Buchan was commander of the expedition and the ship "Dorothea", of 370 tons. Lieutenant John Franklin was second in command, in charge of the brig "Trent", of 250 tons. The same summer, Captain John Ross in the "Isabella" and Lieutenant W. E. Parry in the "Alexander", were sent by the Royal Navy in search of a northwest passage, by way of Davis Strait and into the waters which Baffin had explored two centuries before, and about which there had come to be so much doubt that eminent cartographers had removed from their maps all traces of Baffin's discoveries—to his temporary discredit and to their own ultimate confusion.

Though Buchan and Franklin gave up their attempt to reach the Pole, and to proceed far east from Spitzbergen, they did receive excellent training and experience in arctic navigation which proved of great value in Franklin's subsequent polar explorations. They returned to England on October 22, after an absence of almost exactly six months.

The next year the Admiralty continued its program of arctic research by organizing two expeditions, one in command of Franklin to proceed by land from the northwestern shore of Hudson Bay across the vast tract of land between that bay and the Polar Sea, to the mouth of the Coppermine River; while Lieutenant W. E. Parry, who had been second in command in the previous expedition under Captain Ross, was entrusted with the command of another expedition to seek a route to the Pacific by sea, in the vessels "Hecla" and "Griper". Parry and Franklin were to make contact, if possible, when they had achieved the Pacific.

It is not within the province of this paper to relate the vicissitudes of either Franklin's or Ross's expedition. It was 1822, after incredible hardship and suffering, before Franklin was able to return to England. Nor is it of interest to follow him through a second similar expedition that extended from 1825 to 1827.

For a period of eighteen years, that is until 1845, Franklin found no further opportunity for arctic exploration, though he did not once lose his interest in it; and then returning from the governorship of Tasmania, he was put in command of the

"Eerebus" and "Terror" for another attempt to find the northwest passage. His good friend, Captain F. R. M. Crozier, veteran arctic explorer who had served ably with Parry in 1821, 1824, and 1827, and who shared with Sir James C. Ross the distinction of having approached nearest both the North Pole and the South Pole, was placed second in command.

It was this expedition that determined the course and character of arctic exploration for more than a decade afterward. The expedition was never heard from after it was spoken by a whaler in the middle pack off the mouth of Lancaster Sound the 26th of July 1845. By 1848, when three expeditions, dispatched to seek some trace of the two ships and their complement of 129 officers and men, failed to make any contact with them or to discover any record or sign of their fate, the misgivings that had been felt for a year or two before gave way to grave alarm. Lady Franklin personally and the aroused Admiralty officially not only sent out further expeditions from England, but solicited the financial support and active aid from other countries as well. Lady Franklin appealed directly to the President of the United States and to wealthy and influential persons of the country. In eloquent letters she called upon them, and the American people, to cooperate in the enterprise of saving her husband and his party from the hazard of starvation and death.

Congress, then as now, was slow to act. It was not until Henry Grinnell, a wealthy business man of New York, offered to defray the major expenses of a searching party and to provide two substantial and adequate vessels, the "Advance" of 140 tons and the "Rescue" of 90 tons, that the government assumed responsibility for the expedition, and authorized the President to detail from the Navy the necessary officers and men to constitute the personnel, men who would willingly volunteer to undertake so hazardous and arduous a commission.

Lieutenant Edwin J. DeHaven was detailed to head the expedition and command the "Advance", and Acting Master Samuel P. Griffin was detailed to command the "Rescue". Passed Assistant Surgeon Elisha Kent Kane was made surgeon of the expedition. The total personnel of the expedition including the officers numbered 33 men. The expedition sailed from New York on May 23, 1850, and by mid-July, in company with the British yacht "Prince Albert" sent out by Lady Franklin, was

beset in the ice of the middle pack of Baffin Bay. By mid-August they had crossed the north water from Wolstenholme Sound to Lancaster Sound, and on to Barrows Strait where they met the "Prince Albert" again, only to part company with her on the 22nd of August, when she started back to England. DeHaven's expedition continued its course into Barrows Strait.

From Prince Leopold Island, DeHaven set his course northwestward to Cape Riley, and near that rugged, lofty bit of land, the "Rescue", which had been left behind in a gale in Lancaster Sound, overtook the "Advance". At this point also, three British expleditions: Captain Penny's with the "Sofia" and "Lady Franklin", the veteran Sir John Ross's with the "Felix", and Commodore Austin's with the "Resolute", a steamer, fell in with DeHaven's expedition. Continuing their explorations in company the four parties arrived at Beechy Island on the 27th of August, where they found unmistakable evidence that Sir John Franklin and his companions had made their winter headquarters during the winter 1845–46, had buried three of their dead, and sometime in April had sailed away, without leaving any record that has ever been recovered as to what their future course was to be.

From this rendezvous, the several expeditions separated, and soon afterward DeHaven's two ships were frozen in. The "Rescue" was abandoned and both crews made their headquarters for the winter in the "Advance". All winter long the ice threatened to crush the ships, but spring came without undue accident, and on May 13, the "Rescue" was reoccupied. On the 5th of June the ice began to break up, on the 10th of June both ships were free in open water, and soon after they made Godhaven on Disko Island, where they were repaired and refitted. They headed northward in Baffin Bay to about 73 degrees north and then bore westward, passing the English Whaling fleet near the Dutch Islands on the 7th and 8th of July. On the 11th they met the "Prince Albert" returned to the arctic for another Franklin search. On the 3rd of August they parted company with the "Prince Albert", and finding all possible routes toward the north and west closed by impenetrable ice, DeHaven ordered return to America. On the 30th of September, 1851, after an absence of 16 months, the "Advance" anchored in the Navy Yard at Brooklyn, and a few days later the "Rescue" joined her.

New territory mapped by the DeHaven epedition included both shores of Wellington Channel and the south coast of Grinnell Land, the northwestern peninsula of North Devon Island. The whole story of DeHaven's expedition was written by Doctor Kane, DeHaven having submitted only his official report.

After his return from DeHaven's expedition, Kane zealously sought opportunity to continue his career as an arctic explorer. His zeal was rewarded by orders in December 1852 from the Secretary of the Navy to organize and equip a second Grinnell expedition. Mr. Grinnell became one of the principal supporters of the new expedition, and placed the "Advance", which had proved its seaworthiness and staunchness in the first Grinnell expedition, at the disposal of Doctor Kane. Such scientific institutions as the Geographical Society of New York which later became the American Geographical Society, the Smithsonian Institution, and the American Philosophical Society, contributed toward the funds necessary to equip the expedition properly, and took the lead in raising funds from other sources.

Doctor Kane's party consisted of himself and eighteen carefully selected men, ten of whom were detailed by the Navy and eight selected by Doctor Kane himself. Henry Brooks was made first officer, Isaac I. Hayes, surgeon, and August Sonntag, astronomer. Later, at Upernivik in Greenland, the expedition picked up J. Carl Peterson, a Dane, as interpreter, and Hans Christian, a Greenlander, as hunter. The expedition left New York May 30, 1853, and early in September, after vainly attempting to cross the north water at Ellesmereland, established headquarters in a little bay which Kane named Rensellaer Harbor, on the north side of Inglefield Land in latitude 78 degrees 37 minutes north, longitude 70 degrees 40 minutes west. The party lived on the ship, but stored its supplies on a small island beside which the "Advance" was anchored, and also built on the island a small weather station.

As soon as spring came and Doctor Kane considered traveling feasible, parties were sent out in several directions, led by the more capable members of the staff. The eastern coast of Ellesmereland and the western coast of Greenland were mapped, chiefly northward from the latitude of Cape Sabine. Unfortunately sledging ceased as soon as the ice began to break up, and no further mapping was possible.

The ship did not break out. By August 24 the party concluded that the ship would have to stay another winter and Doctor Kane gave the men their choice of staying with it or attempting to go southward. Nine, led by Doctor Hayes, decided to sledge southward, and left the ship on August 24. The rest decided to stay with Doctor Kane and the ship. On December 12 the party which had attempted the sledge trip southward returned to the ship, fully convinced that they could not gain the Danish settlements in South Greenland. The united party abandoned the "Advance" the following spring on May 20, 1855, and by sledge and boat finally reached Upernivik in early August. September 11 it arrived at Godhaven. Soon after, Captain H. J. Hartstene with the bark "Release" and the steam brig "Arctic" which had been sent north to relieve the expedition stopped in at the port, took them aboard, and brought them back to New York October 11, 1855—and the second Grinnell expedition was ended.

The expedition mapped the outstanding features of the eastern coast of Ellesmereland from Cape Isabella to Cape Back, and sketched in an extension of the coast northward almost a degree of latitude. More accurately, and in greater detail, it mapped the western coast of Greenland from Cape Alexander to Cape Constitution and Franklin Island. By this expedition the American gateway to the Pole was first opened, because it was Doctor Kane who proposed the idea of an open polar sea beyond Kane Basin and Kennedy Channel, an idea that was later developed and emphasized by the surgeon of the expedition, Dr. Isaac I. Hayes. It was the second Grinnell expedition which brought to the attention of the world the Eskimo village of Etah which has since become famous in arctic exploration as the base to which every subsequent expedition to Smith Sound felt sure that it could go, and from which a number of other expeditions extended their explorations northward and northwestward.

Stimulated by his experiences with the second Grinnell expedition, and encouraged by the friendly support of Prof. A. D. Bache, the Chief of the United States Coast Survey, Dr. Isaac I. Hayes organized another expedition to seek "the open polar sea by the Baffin Bay-Smith Sound-Kane Basin route". He brought the subject before the American Association for the Advancement of Science at its annual meeting in Baltimore in

April 1858, and that body selected a committee of sixteen of its leading members to constitute a committee on arctic exploration. The same societies which had supported the second Grinnell expedition took the lead in supporting Hayes's expedition, and for a third time Mr. Henry Grinnell came forward to the financial aid of the project. Prof. Joseph Henry of the Smithsonian Institution became one of its strongest and staunchest advocates, and helped organize the scientific work of the party. Few expeditions have been sponsored by so many prominent institutions and so many eminent men as was Hayes's expedition.

A fore and aft schooner named the "Spring Hill", of 133 tons drawing eight feet of water, was chartered for the expedition. Her name was changed to the "United States", and she was strengthened by heavy greenheart sheathing and thick oak planking externally, and with heavy crossbeams and diagonal braces internally. For convenience in working through the ice, her rigging was changed from a fore-and-aft to a foretopsail schooner. A complement of fourteen men in addition to Doctor Hayes was chosen for the expedition. August Sonntag, who had been astronomer on the second Grinnell expedition, was placed second in command.

The expedition could not get away until July 6, but it made good time and crossed the arctic circle before the end of the month. The boatswain of the expedition, Gibson Caruthers, who had served under DeHaven in the first Grinnell expedition, died when the ship reached Upernivik and was buried in the little churchyard overlooking the sea. At Upernivik Doctor Hayes recruited three Danes: Peter Jensen, interpreter and sledge dog manager; C. E. Olswig, sailor; and Carl Christian Peterson, sailor and carpenter to take the place of Caruthers. He also engaged three Eskimo hunters and dog drivers. Continuing northward, the expedition was finally stopped by ice extending from Littleton Island southwestward to the coast of Ellesmereland, and Doctor Hayes decided to establish his winter headquarters in a little bay which he named Fort Foulke, southwestward across Foulke Fjord from Etah. After a long, dreary winter aboard ship, the party re-explored Smith Sound, Kane Basin, and Kennedy Channel, filling in details of the map made by Doctor Kane's expedition, and extending the survey 40 miles farther north, as far as Cape Lieber and Cape Union.

The "United States" broke out from the ice on the 10th of July and sailed from her winter quarters on the 14th. On the way southward Doctor Hayes completed the survey of the eastern coast of the peninsula from Cape Alexander to Granville Bay. The party reached Upernivik on the 14th of August, and on October 23 made port in Boston after an absence of 15½ months.

Of all American arctic explorers, Charles Francis Hall was probably the most enthusiastic, the most persistent—except for Peary—and certainly the most picturesque. He began his career of exploration with a determined attempt to aid in the search for Sir John Franklin. For almost ten years he devoted every spare hour to studying the problem of finding Franklin, and in February 1860 he issued an appeal to the public for support in undertaking an expedition for that purpose. Once again Mr. Henry Grinnell advanced generous funds, and the firm of Williams and Haven of New London offered to convey Hall's party and equipment free of charge to Northumberland Inlet and give it free passage home in any of its ships. On May 29, Hall with a single Eskimo companion, Kudlago, left New London in the ship "George Henry". His whole outfit included a sledge, a boat, a few surveying and weather instruments, about 1200 pounds of pemmican and meat biscuit, and a small amount of ammunition. Kudlago died before the ship met the first ice. Hall landed, joined his fortunes with the Eskimo of Frobisher Bay, and with their aid managed not only successfully to maintain himself but to explore the immediate environs of the Bay. He returned with the whaler "George Henry" in September, 1862.

On the first day of July, 1864, Hall again set out for the north in the whaler "Monticello" bound for Hudson Bay, which left him at Whale Point on the west side of Roes Welcome on the 29th of August. When he left the ship, a Mr. Charles Rudolph, a member of the crew of another whaler which had just come in, elected to join Hall in his search for Franklin. Again Hall enlisted the services of the Eskimo, and it was well that he did so because his companion, Rudolph, proved of little value.

From August, 1864, to June, 1865, Hall searched the area from Whale Point to Repulse Bay including Wager Bay, and established his permanent headquarters at Beacon Hill near the

Fort Hope of Doctor Rae. In the spring of 1866 he sledged to Cape Weynton on Colville Bay, but returned to Fort Hope for the winter. In the spring of 1867 he went to Igloolik Island near Hooper Inlet at the eastern mouth of Fury and Hecla Strait to purchase dogs, returning in due time to Fort Hope. In 1868 he explored the north end of Melville Peninsula, sledging across the tip of the peninsula by the Crozier and Brevoort rivers, and continuing southward on its western coast to Cape Parry. He carefully surveyed the environs of Fury and Hecla Strait which had never before been explored. He returned for his fifth successive winter at Fort Hope.

On the 23rd of March, 1869, he set out for King William's Land, and on the 9th of April reached Pelly Bay. He continued his journey by way of Simpson and Grinnell lakes, descending the Murchison River, crossed Inglis and Shepperd Bays, and on the 8th of May attained Todd's Island and King William's Land, where he found many relics and the bones of at least 79 of Franklin's men. He returned to Fort Hope, spent the month of July hunting, fishing, and surveying. On the 26th of August he and three Eskimo, two of whom had served him faithfully during his five years of continuous exploration, and a little girl whom the two Eskimo had adopted, were picked up by Captain Fisher of the whaler, Ansel Gibbs. On the 26th of September, 1869, they arrived at New Bedford.

In the course of his five years' exploration and faithful search for Sir John Franklin's party, Hall had not only surveyed and mapped a large area of the northern mainland of America beyond Hudson Bay, but he had determined definitely what had happened to Franklin's party, how many of them had died, how at least one of the vessels had been destroyed, and many other significant facts regarding the fate of the expedition. He brought back with him to the United States a great many relics of the Franklin expedition.

The year after his return, Congress appropriated $50,000 for another expedition to the North Pole, and commissioned Captain Hall as commander. A steamer, the "Periwinkle" of 387 tons, was heavily retimbered and her bottom double planked, caulked, and coppered and made ready for the expedition. Before her commission, she was renamed the "Polaris". A staff of ten scientists, including the famous Dr. Emil Bessels, a Heidelberg

Ph.D., a crew of fourteen persons, and the two Eskimo whom Hall had brought back with him from Hudson Bay, constituted the personnel of the expedition under Hall's command.

It left New York June 29, 1871. It encountered the first ice in August but afterward, meeting little obstruction from the ice, the expedition attained an exceptionally high northing, 82 degrees 29 minutes in Robeson Channel. There the ice beset it and carried it southward to latitude 80 degrees 38 minutes north, where in a little cove, which has been named Polaris Bay, the party made its headquarters, 200 miles farther north than Kane's famous station. After leaving Tessiusak, the northernmost Danish trading station, the 24th of August 1871, the expedition was not again heard from until the 30th of April 1873, when a portion of the party, nineteen men, women, and children, including Captain Tyson, which had been adrift on an ice floe since the preceding October 15, was rescued off the coast of the Labrador by the "Tigress" of Newfoundland under command of Captain Bartlett.

Hall died November 8, 1871, and his second in command, Master S. O. Budington, assumed charge of the party. The spring and summer of 1872, a number of sledge and boat journies were made and a considerable portion of the coast along the northern reaches of Robeson Channel was mapped. Unfortunately, the records of the surveys were not brought back by the expedition when it returned to America.

The "Polaris" drifted southward in the ice, and at a crisis when the ship faced destruction, part of the expedition attempting to salvage the supplies on the ship, drifted away on an ice floe. The ship got as far south as Littleton Island, where she was beached and abandoned by the remainder of the party under Captain Budington's command, in October 1872. The expedition built a house on the shore adjacent and with the help of Eskimo from Etah survived the winter. The "Polaris" drifted out when the land ice broke away, but soon grounded again and went to pieces. June 3, Captain Budington's party left its winter house by boat, and after many hardships was picked up by the Ravenscraig, a whaler from Kirkcaldy, Scotland, and taken to Dundee, when it returned to America, arriving in Washington on October 7. Captain Budington's party first learned from the Ravenscraig's crew of the rescue of Captain Tyson's party from the ice floe earlier in the year.

This first period of American arctic exploration, which began with DeHaven and Griffin's expedition in the "Advance" and the "Rescue", in search of Sir John Franklin, closed with Schwatka and Gilder's remarkable overland expedition, in the winter of 1879–80, from Hudson Bay to King William's Land, and return, having ascertained the final facts of the fate of Sir John Franklin's party. For three decades the search for Sir John Franklin and the attempt to recover his records had continued. American explorers engaged in the search had rather fully and thoroughly explored and mapped two important arctic areas, the northern Hudson Bay region, and the Smith Sound region. They had contributed substantially to the knowledge of the fate of Sir John Franklin's party, to arctic geography, and to technique of polar exploration.

THE DRIFT OF THE *JEANNETTE* IN THE ARCTIC SEA

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(*Read February 24, 1940, in Symposium on American Polar Exploration*)

It is a trite saying that hindsight is better than foresight, and for us today it is well to bear this in mind when we compare (to their disadvantage) the polar explorers of past centuries with those since 1900.

The *Jeannette* Arctic Expedition of 1879 stands in the minds of most of those who now recall it at all as a tragic failure, which at the cost of ship and most of the crew discovered little and got nowhere near its polar objective. Many yet think it ill-prepared, ill-conceived, and ill-lead, and destined therefore for failure from the beginning, none of which ideas fit the facts.

In the early 1870's, of the polar regions and of conditions there we knew less than is now known of conditions on Mars. The polar areas were a vast terra incognita, concerning which scientists speculated learnedly, and explorers sought to learn by going there.

But no one had penetrated north of latitude 83°. What lay beyond was a blank. It was only known that by the Atlantic route, every expedition northward had had its ship blocked solidly by impenetrable ice.

What the conditions were by the Pacific approach was still unknown, but in scientific circles, it was then argued that they were better. Two widely held beliefs of that day were responsible for this theory. One of these was that the Japanese Current, the Kuro-Si-Wo, or the "Black Tide" of Japan, which swept northward as a warm current toward the Behring Sea, and then came southward down the west coast of America to temper Alaska and the western Canadian coast to a climate far milder than corresponding latitudes around Labrador, actually split in Behring Sea into two streams, one of which swept northward through Behring Strait towards the Pole, giving there an ice-free channel far into the Arctic if not to the Pole itself.

The second belief was that Wrangel Land, north of eastern Siberia, on which no one had yet set foot, was really a large continent stretching toward and across the Pole, reappearing on the Atlantic side as Greenland, of which region as yet no northern boundary had been found.

Endlessly in Europe and America the pros and cons of these two theories were argued, with no definite conclusions possible till Lieutenant George Washington De Long of the United States Navy came along to settle the discussion by going that way to find out by inspection.

De Long persuaded James Gordon Bennett of the *New York Herald* to finance the expedition for him, to which end Bennett purchased the British ship *Pandora* of 420 tons, and in that vessel, renamed the *Jeannette,* strengthened considerably, refitted throughout for polar service, and commissioned in the United States Navy, De Long set out from San Francisco on July 8, 1879, for the North Pole via Behring Strait. Accompanying him were four officers of the regular navy, three civilians in an officer status, and twenty-three specially picked seamen.

It may here be stated that no expedition which had ever gone north before had a stronger ship, a better-equipped vessel, nor a more competent set of officers and men for the purpose, and probably few since. Every scientific instrument then available was carried, and no Arctic commander ever had a keener scent for scientific truth than De Long.

The *Jeannette,* a barque-rigged steamer, proceeded through the Pacific to Behring Sea, where in late August of 1879, De Long picked up forty dogs and a number of sledges together with two Alaskan Indians as sledge drivers, it being his hope that if at last the *Jeannette* were frozen in before she reached the Pole in spite of the Kuro-Si-Wo Current, he might winter his ship in some bay on Wrangel Land and leave her there, the while with the dogs he sledged along the firm coasts of Wrangel Land to the Pole itself.

In early September, the *Jeannette* passed through Behring Sea into the Arctic Ocean, where after a brief stop on the north coast of Siberia, she headed through the open sea for Wrangel Land. But two days steaming to the northwestward cast grave doubts upon the Kuro-Si-Wo Current theory, for neither water

temperatures nor specimens dredged up from the bottom gave the slightest indications of warmer waters thereabouts coming through Behring Straits, and to make the matter definite, the *Jeannette* shortly brought up against a continuous ice pack in latitude 71° N., with which it was evident, from the eight foot thickness of the ice, that the "Black Tide" of Japan had had no more contact than the green waters of the distant Nile.

At that point, then, the *Jeannette* Expedition exploded one polar fallacy, but there still remained the question of Wrangel Land and its extent, to offer yet some hope of sledging to the Pole. Boldly De Long put his ship into a lead in the ice-pack and for several days, with the full power of his engines driving her, butted and rammed her along through openings in the pack, endeavoring to fight his way toward the shores of Wrangel Land, dimly visible now across the polar ice far to the northwest. But on September 6, 1879, the *Jeannette* was finally solidly frozen into the pack, wholly unable by steam or sail to move thereafter, with Wrangel Land still some sixty miles off, and with tiny Herald Island perhaps fifteen miles due west of her position.

Helpless, at the whim of the polar pack, the *Jeannette* and her crew from that day on moved with the ice. Quickly they noted that they were being taken generally northwestward at the rate of two or three miles a day, and for a brief time the hope rose that they might be carried by the moving ice to the shores of Wrangel Land where the ship might be wintered in some cove while they proceeded with their sledging northward. But after a few weeks of tedious drifting, it became painfully obvious to De Long and his men that the second theory on which the expedition had based its hopes of success was quite as great a delusion as the Kuro-Si-Wo Current.

For as the *Jeannette* went northward with the ice, the explorers looking westward toward Wrangel Land quickly saw that far from extending toward the Pole, Wrangel Land was but a relatively small island, insignificant in the vast expanse of pack ice covering the polar seas, and of not the slightest value to them for sledging northward.

With this dismal discovery, all hope of real exploration vanished, for with no fixed base for the ship to work from, and nothing but a thousand miles of shifting ice between them and

the Pole to work over, sledging became an impossibility. Then and there the theoretical bases on which the expedition had been founded, collapsed, and on De Long, with his ship frozen in at 71° 30′ N., a latitude so far below that attained by other explorers in the Atlantic as to make him the laughing-stock of the world when it became known, fell a deep and abiding sense of failure, while from the *Jeannette's* wardroom, torn already by personal dissensions, all vestiges of sociability departed forever.

On September 6, 1879, the *Jeannette* began her drift with the arctic pack, a drift unprecedented till then in polar annals. But in spite of gloom, in spite of the terrors of the pack, carefully, regularly, and minutely were begun and carried through to the tragic end of the expedition the scientific observations for which the *Jeannette* was equipped.

In the sea, the salinity, the density, and the temperatures of the ocean from top to bottom were accurately measured through a hole dug in the ice near the stern, and the soundings, character of bottom, and marine specimens as shown by the dredge, duly logged. The thickness and characteristics of the ice pack were recorded, together with the varieties of old and new ice formations.

Magnetic phenomena, covering variation, dip, and strength of the earth's field, were most thoroughly investigated in a small tent laboratory set on the ice well away from the ship, and the auroral displays carefully sketched; while with a pendulum the force of gravity was measured most accurately.

Wind velocities, air temperatures, humidities, and all meteorological data were continuously recorded, and special studies made of snow crystals under low temperature conditions.

But in spite of all, as winter came on, with fierce cold reaching —50° F., the alarms and terrors of the moving pack dominated all else.

Amidst what seems to have been ice conditions encountered by no other expedition, the *Jeannette* for the months of her first year in the pack drifted aimlessly to and fro in what her crew soon came to call "the arctic doldrums" to the northward of Wrangel Island, never getting much above 74° N., never getting much below 72° N; crossing and criss-crossing endlessly her previous tracks. For weeks on end from November through January the crew lived in momentary fear of seeing their vessel

crushed in the screeching ice pack, with tumbling floebergs hurtling by them in canals in the flinty ice, threatening to crash down on their decks and squash them out like flies. Through the long arctic night, De Long and his men lived in hourly dread of sudden death, always with sledges packed and knapsacks at hand ready on a moment's notice to abandon ship should the *Jeannette* collapse under the terrific squeezing of the floes, always dismally aware that, should that happen, escape for them across the tumbling pack which had engulfed their ship would be impossible.

Spring came at last in 1880, the pack subsided, life aboard ship became once more bearable. The aimless drift, getting nowhere, continued, lighened only by the hope that summer might bring release. But summer came and went and the autumn of 1880 arrived to find the crew of the *Jeannette* no farther north than a year before, facing a second winter in the pack with all hope of release gone, doomed apparently forever to drift with the pack about that 100 miles of sea to the north of Wrangel Land till death in one form or another came to release them.

To De Long and his crew, their second winter in the arctic ice-pack was but a repetition of their first, save that, with senses somewhat dulled by long months of terror and despair, they took what came with slightly more equanimity. Their ship was damaged now and leaking. Only constant pumping with an improvised windmill to save coal, kept them afloat. Hope of discovery was totally gone, as was most of their coal, and even on De Long, a deeply religious man with an abiding faith in heavenly guidance, a gnawing sense of failure settled.

The spring of 1881, however, brought a change. For the first time, to their delight, the explorers found themselves carried well across the 74th parallel of latitude, with the drift continuing northwest, and the soundings, which had before averaged around 20 fathoms, slowly increasing. March came, to find them crossing 75° N., apparently at last "going somewheres," and all hands cheered up, and by the end of April the *Jeannette* had reached 76° 19′ N. At this point, De Long figured out that in from three to five years more if the northwest drift continued, he would spiral upward to the Pole, but the computation gave him little comfort, as he had provisions left for only another year, and coal enough left to keep from freezing hardly that long.

May came, to find the *Jeannette* nearing 77° N., when on May

16, the cry of "Land!" echoed from the crow's nest, and the *Jeannette* had made its first discovery, a small arctic island soon named in honor of their ship. A week later, in latitude 77° 16′ N., longitude 159° 33′ E., a second island, named Henrietta, was sighted, and on this the expedition's chief engineer, George Wallace Melville, made a landing after a hazardous three day journey across the pack. On Henrietta Island, Melville left two metal cylinders containing records of the expedition, one of which the Russians, reaching that spot by plane about a year ago, recovered.

Somewhat cheered by these discoveries, the crew of the *Jeannette* continued their involuntary journey with the pack, to find that now they were going steadily due west, having passed the islands which were shunting the pack northward, with the ice however increasing in liveliness, and showing some signs for the first time of breaking up when the summer of 1881 arrived. But unfortunately for these hopes, the pack split wide open on June 11, leaving the *Jeannette* for a few hours adrift for the first time in nearly two years, only to start to close again, gripping the ship between floes sixteen feet thick and heaving her over 23° to starboard.

Held thus for some hours, the stout sides and trusses of the *Jeannette* fought the ice, till finally from the pack came a tremendous squeeze which folded the *Jeannette* up like an accordion, buckling up her decks, and leaving her a complete wreck held up only by the pressing floes.

Frenziedly in this emergency her crew tossed overboard onto the pack everything they could lay hands on in the way of supplies, clothing, and boats from their crumpling ship. Late that night, the pack released its pressure somewhat and the remains of the *Jeannette* sank from sight, leaving De Long and his crew of 32 stranded on the pack some 500 miles away from the nearest point on the Siberian coast, the Lena Delta, where they might expect to find some native villages and some assistance.

Of the heroic retreat of De Long and his men across that 500 miles of ice pack and open polar sea, little here can be said. Dragging three boats and their provisions, amounting all told to some eight tons, carried on sledges over the broken ice, the men set out as human beasts of burden harnessed to the sledges. Urged on by De Long's indomitable will, under terrible condi-

tions of hardship and suffering, in three months the worn seamen managed to reach open water at Semenovski Island in the New Siberian group. There with food practically gone, in the three small boats which for ninety days had formed their chief burden in the drag over the pack, they set out to cross the last 90 miles of open arctic sea which separated them from the Lena Delta and the hoped for help.

Caught at sea on that crossing of the Arctic by a bad gale, the smallest boat, commanded by Lieutenant Chipp, swamped in the storm with the loss of her crew of eight. The other two boats (somewhat more seaworthy) under the command of De Long and Melville respectively, though separated, managed to weather the gale and, after four terrible days at sea, landed on Siberia. There Melville's boat crew, carried southward by the storm, were found by natives and saved. But De Long, landing a hundred miles away from Melville's party, at the northern mouth of the Lena where according to his charts there were native villages, found only that the charts were all wrong, and the region a desolate wilderness devoid of shelter or food. Stumbling southward in the incoming winter in late September and October, for six weeks he and his freezing men worked southward some 70 miles, finally to perish on the bleak tundra of cold and starvation.

But as fate would have it, the drift of the *Jeannette* did not end with the sinking of the ship nor the deaths soon after of most of her crew. Of the huge amount of food, stores, and clothing tossed out on the pack alongside the crushed ship, only a minor part could be dragged on the sledges by her crew, and after a careful sorting out, the remainder was abandoned on the pack when on June 18, 1881, the men of the *Jeannette* began their epic trek over the ice.

On June 18, 1884, three years to a day from that time, in latitude 60° 36′ N., longitude 46° 7′ W., being about 110 miles northwest of Cape Farewell, Greenland, three Eskimos picked up on ice floes drifting near the Greenland coast a miscellaneous collection of relics of the *Jeannette*—a torn check-book, broken boxes marked with the *Jeannette's* name, a pair of oil-skin trousers bearing the name of Louis Noros, one of the few survivors.

That startling discovery indicated a drift westward from the

point of sinking of the *Jeannette* of some 4500 miles in 1096 days; roughly of four miles a day from the position in lat. 77° 15′ N., long. 155° E., where she had foundered. Nansen, reading of it that year, conceived then and there in 1884 the basic idea for his famous voyage in the *Fram.* For if these articles, cast away on the ice north of the New Siberian Islands, ultimately emerged near southern Greenland, the same drift might well carry him in another ship starting from where the *Jeannette* had disappeared, over the same course on a great circle well toward the Pole.

From that drift of the *Jeannette* relics, grew the voyage of the *Fram.* In 1893, putting his ship in the ice near Bennett Island (which De Long had discovered on his trek across the pack) Nansen was there frozen in and the *Fram* continued the drift of the *Jeannette* to the northwest, reaching with the ice as high as 86° N., and emerging after three years on the Greenland side, safe.

Of itself the drift of the *Jeannette* and its wreckage proved the existence (later verified by Nansen) of a steady Arctic current from north of Wrangel Island toward the Pole, with a final set southerly and westerly from above Spitzbergen to the lower Greenland tip.

But in Arctic history, the real achievement of the voyage of the *Jeannette* was neither the explosion of certain fallacies geographically, the discovery of three new islands, nor the establishment of the speed and direction of the polar current mentioned above. Standing out above all else were the fortitude and dogged determination with which De Long and his men in the face of horrible privations and disaster fought on even in a hopeless situation, and in their deaths on the frozen Siberian tundra left to us an example of how men may die with their souls still unconquered. In the long roll of Arctic tragedies, for that the saga of the *Jeannette* will forever stand out.

A TEN-YEAR PROGRAM OF ARCTIC STUDY

VILHJALMUR STEFANSSON

Arctic Explorer

(*Read February 24, 1940, in Symposium on American Polar Exploration*)

THE Committee on the Wilkes Centennial and American Polar Exploration asked me to write a paper on the lands and seas discovered or first explored by the expedition, financed and administered by the Canadian Government, which I commanded during the years 1913–18. But it has not seemed to me useful to attempt presenting in a few thousand words a re-hash of what has been printed about these lands and seas in several books and many articles written by various members of that expedition, among them a 400,000-word account which I published under the name of *The Friendly Arctic.* What I shall do is to lay before this meeting a plan for long continued and systematic study of Arctic weather and sea conditions which, although referred to in *The Friendly Arctic* and in several other things which I have written, has never been advanced as a concrete program.

This plan of exploration, and of continuous study for at least a decade, is based primarily upon the experiences of the mentioned 1913–18 expedition; secondarily it rests upon the findings of the Papanin and Badygin (*Sedov*) expeditions of the Soviet Government and then upon the general advance of knowledge regarding the polar sea between the close of our expedition in 1918 and this present centenary year of the Wilkes expedition, 1940.

The proposal is that between ten and twenty observation groups, of three to five men each, be established upon the drifting floes of the polar mediterranean, the groups to use methods of living and of scientific study that are based upon, and a development of, the procedures used by the Canadian Arctic Expedition of 1913–18 and the Soviet (Papanin) expedition of 1937–38.

Paragraphs introductory to a discussion of our plan may refer to a procedure of another sort which led to the one which we here suggest. Vessels have become fast in the Arctic pack,

through accident or plan, and have drifted with it. In 1854 the *Intrepid* and *Resolute* of the Franklin search were, for instance, abandoned in Melville Sound, one of them, the *Resolute,* to be picked up undamaged the next summer in Baffin Bay after a drift of 1,000 miles or more. The *Tegetthof* of the Payer and Weyprecht Austrian expedition, which discovered Franz Josef Land, was caught in the pack to the north of Europe and drifted nearly two years until, seriously damaged, she was abandoned without loss of life.* Then came the far more notable drift of an American vessel, the *Jeannette,* commanded by De Long, when in 1879 she was caught in the pack to the east or northeast of Wrangel Island, drifting to the west and northwest for more than a year until, damaged beyond repair, she was abandoned to the northeast of the New Siberia Islands, the men retreating towards shore with eventual death of about two-thirds of the party. The losses of life were not, however, directly a part of the drift. For no one perished while on the beset ship, or on the crew's way across the pack from the ship to open water, where they launched three boats. The one commanded by Chipp was lost with all hands in a gale where no ice was in sight. The other two, commanded by De Long and Melville, reached the Lena delta where, through accidents that were on the whole favorable to Melville and unfavorable to De Long, all of the Melville crew were saved and all but two of the De Long crew lost.

These drifts in chief, but also the stories of similar ones less notable, convinced Nansen that wedging a ship in the northern pack is promising as a method of exploration and not foolhardy. His plan worked normally; the *Fram's* good fortune compared with the *Jeannette's* bad luck was somewhat as Melville's fortune had been in comparison with De Long's. The Nansen drift resulted in much information regarding winds and currents, salinities, temperatures, ocean depths, ocean bottom samples, and the like. It resulted, too, in a notable body of misinformation—a "demonstration" in which Nansen believed, and in which most of the scientific world followed him, to the effect that an ancient doctrine had been confirmed which said that life is negligible in and upon the northern sea if you are far north and far from land. Nansen claimed to have searched for animals and plants in the ocean with such diligence and with methods so

* One man died later from scurvy.

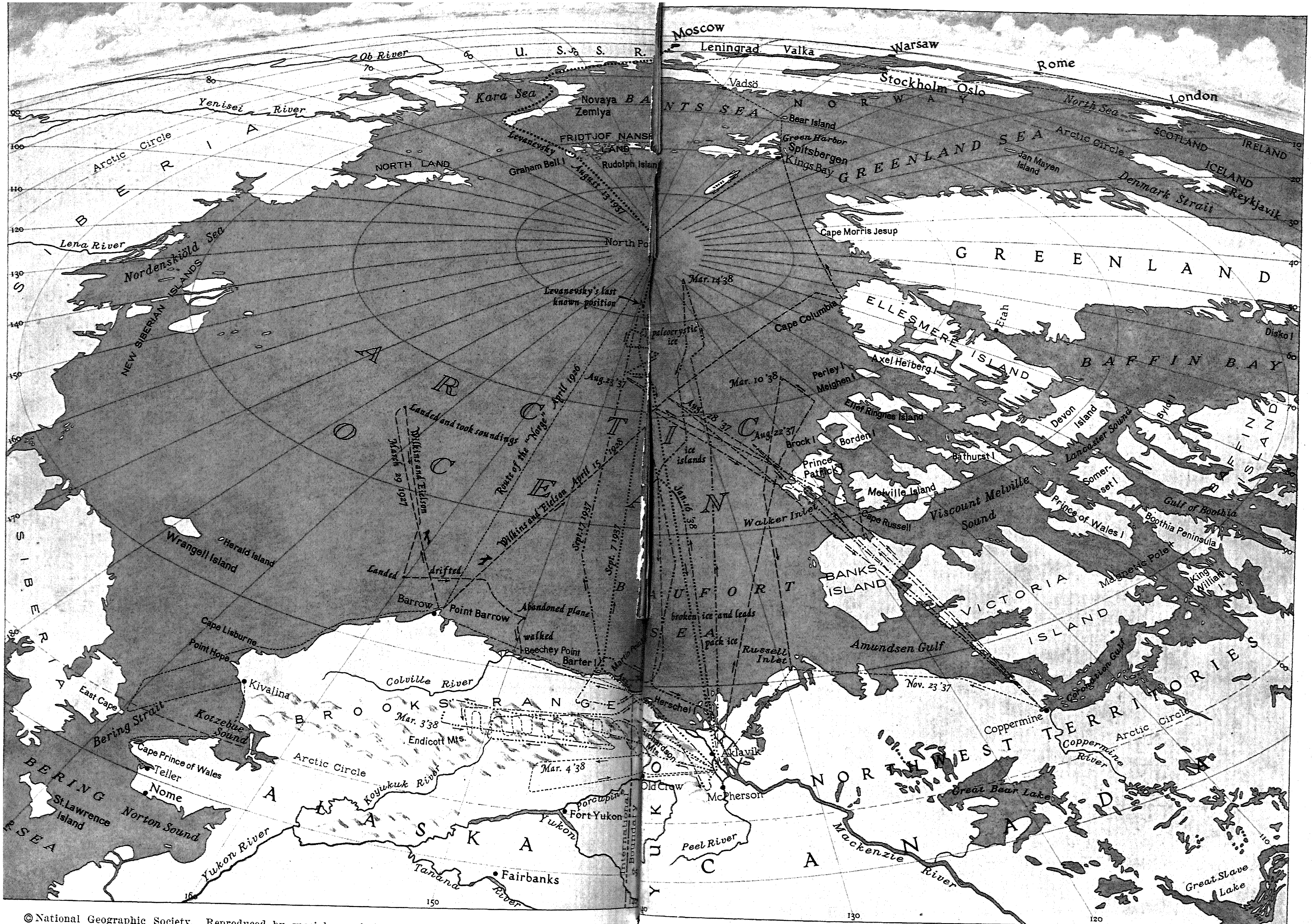
adequate that his finding "negligible" quantities proved that life was absent or scarce in the region of the drift. It was concluded by him, and agreed by most or all of the scientific world, that if the observations had been made still nearer to the Pole life would have been still scarcer or wholly absent.

Here and there throughout *The Friendly Arctic*, and in a number of shorter pieces, I have dealt with the inadequacies of Nansen's methods of observation and the illogicality of his conclusions.[1] Now I am merely trying to present a very brief statement and cannot go into the matter beyond repeating what I remember first saying in 1912, that it appeared to me the reasoning of those who thought the northern sea at high latitudes of necessity either practically or wholly lifeless was of a type more likely to appeal to a philosopher than to a fish. I did not believe that animals which in large part control their own movements would turn around just because they were getting to what they considered an excessive latitude. I felt sure that plants, and animals which control their own movements slightly or not at all, would drift with the currents and that there would be currents beneath the ice throughout the polar sea.

I did not know whether it was as important for marine life as claimed by some of the reasoners that the surface of the water shall be in direct contact with the air; but I felt sure, in any case, that no part of the sea as big as that paradise of winter fish my native lake of Winnipeg could ever be for comparably long periods as nearly hermetically sealed away from the atmosphere as are the waters of that lake when its surface congeals flush against every shore in early winter and remains that way till spring. For I did not believe that sea ice is strong enough to withstand the stresses of tides and currents so as to be permanently (*i.e.*, for several weeks or months) in larger patches than a few dozen, or at most a few hundred, square miles. I knew that cold water bears more oxygen in solution than warm; that cold waters preserve dead animals in a condition suitable for the food of other animals longer than do warm.

All of this comes back to repeating that the arguments of believers in the lifeless polar sea will appeal more to a philosopher

[1] See, for instance: "Living Off the Country as a Method of Arctic Exploration," *Geographical Review*, May, 1919; and "The Question of Living by Forage in the Arctic," *Arktis*, Heft I, 1929.



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than to a fish. Not that I want to be thought to admire a fish more than I do a philosopher—it was rather that in my travels over the polar sea I expected to be more dependent on marine creatures, from shrimps to seals, than upon the doctrinaires of any school.

During winters and summers of the years 1913–18 various parties of our expedition traveled by sledge or drifted in floe encampments upon the Arctic mediterranean, to the north of Alaska and to the west and northwest of the Canadian islands. We formed during those years the general impression that life adapted to supplying a hunting party with food, fuel, light, clothing and camp material, did not vary much, if at all, with latitude; nor with distance from shore. It did vary, however, with other conditions, such as age and mobility of the ice.

We concluded from our experience that a journey across the northern sea by sledge and living by hunting would be analogous reasonably to a journey across North America, such as that of Lewis and Clark, which depended in the main on local food supplies. There would not be any natives from whom to purchase food; to that extent the analogy breaks down. The strict similarity would be on the hunting side, where a westward journey (from, say, Portland, Maine, to Portland, Oregon) would have passed through regions where game was plenty, where game was decreasing, where game was nearly or quite absent, where game was on the increase and where, again, it was at a maximum. Thus it would be on the Arctic pack, with a difference that a given land region, such as Minnesota, would perhaps have roughly the same amount of game this year as last, while in the Beaufort Sea the regions of scarcity and of plenty would have different latitudes and longitudes in different years. However, they would have approximately similar total areas.

At sea we lived on two animals, seals and bears. The stomachs of the seals, when we cut them up, had in them nothing but shrimps in nine cases out of ten; in the tenth case there would be either fish or shrimps and fish. Polar bear stomachs never had anything in them but seal.

While our stomachs never had anything in them but seal or bear, they could have had shrimps; for we frequently saw them drifting or moving sluggishly near the surface of leads. We

could have had fish, for the ingenuity of man is not less than that of the seal—only we had such confidence in the abundance of seals and bears that we did not trouble to bring equipment for securing fish. If we had carried such equipment it would have been for scientific reasons, to determine the variety of life and not to increase or change the variety of our diets; for when you have been more than three months on an exclusive diet of any one kind of meat you are disinclined to change it for another kind—somewhat as a man who has standardized on a breakfast of orange juice, toast and coffee may be discommoded rather than pleased when he faces a dish of ham and eggs.

On the Arctic pack we were comfortable during winter. When it was cold enough, zero or colder, our snowhouses were heated by seal oil lamps, or otherwise, to where we sat daytimes in our shirt sleeves or stripped to the waist. During the night our snowhouses, with lamps extinguished, were just above freezing. We slept naked (without pajamas) in dry and comfortable blankets or sleeping bags; our clothes were always dry; our seal and bear meat always tasted good to us, and it kept us in perfect health. We worried less about the food supply when hunting on the sea than we had done in comparable long periods while hunting on land—and certainly we worried a great deal less about how to get food than do those millions of our fellow citizens who in 1940 are on the verge of home relief. I feel sure I speak for nearly or quite all of the fifteen or twenty different men who have made long journeys with us over the sea ice when I state that, except on our first journey in 1914 when our theory was being tested, we were never subject to a serious mental strain—to fear, uncertainty or other form of worry.

In a way the outstanding demonstration of the safety and comfort of a self-supporting residence upon the Arctic sea ice was through a party commanded by Storker Storkerson who took charge when I was down with typhoid. The five of them traveled between March 15 and April 16, 1918, about 200 miles north from Cross Island, Lat. 70.5° N., Long. 148° W. Then they camped on a floe, the plan being to drift a year or a year and a half. They had with them when they made this camp provisions for a few weeks but did not use these through the next six months, for they were lighter and easier to carry than fresh meat and might be needed if the party had to travel during the absence

of the sun—you can probably make your living on the northern sea even while the sun is absent if you do not have to travel; but naturally if you are on the road through the whole period of twilight each day you would not have much opportunity for hunting.

At the end of about six months and 450 miles of drifting, at distances varying between 200 and 300 miles from shore, the party decided upon returning to Alaska, for one of them had developed asthma. This was a strange disease to contract; the anomaly has not been explained, or at least there are alternative theories (*a*) that nobody understands asthma so the trouble might develop under any conditions, and (*b*) that one of the party was homesick and pretended the asthma.

In any case, the five journeyed ashore during the months of October and November when sea conditions are worst, as nearly or quite all sledge travelers of the north will agree. Much of the ice is still young and therefore breaks readily under stresses so that there is a lot of open water. Also much of it is treacherous in the sense that it is covered by heavy new snow which insulates it from the chill of the weather and keeps it mushy, so that men and sledges are likely to break through.

When I published *The Friendly Arctic* in 1921 I felt, and I still feel, that Storkerson's account of this journey made at the worst time of year shows as well in a sentence as it could in a chapter that the men of that party had become as native to the Arctic pack as any man or woman is to farm or city environment. For Storkerson's official report to me, a document otherwise of considerable length (as, for instance, in explaining why the party decided to move ashore), covers the four weeks of sledging over drifting pack in twenty-four words: "We started from a point a little over 200 miles from shore on October 9th and reached land November 8th without accident or hardship".

At least three of the five men who made up this party have said that they were comfortable, that they enjoyed it, and that they could have drifted on the floe as safely and comfortably six years as they did six months—assuming, of course, that they did not reach the open waters of the North Atlantic within the mentioned six years. From the other two men I have had no report, but their colleagues assured me that there had been no difference of opinion among the five.

All of the party agreed that the majority had felt no homesickness or desire to come ashore; each man claimed for himself that he had not been lonesome and that he did not want to come ashore until some time the next year; however, some of the men said about others in the party that evidently they were at times a bit lonesome and homesick.

Like our other sea ice traveling parties, the Storkerson drifting group lived mainly on seals. However, they could have killed practically any number of bears they desired; for as soon as the camp had been established and had begun to smell pretty strong—the barnyard-like smell where you have eighteen dogs for a long time and then the other smells from butchering a large number of big animals—these smells started bringing the polar bears in from great distances. After a sufficient tonnage of bear and seal meat had been piled up it became the practice to scare bears away.

As on all our other journeys, it was found by Storkerson that the seals lived mainly on shrimps but some of them to a slight extent on fish, and that the bears lived exclusively on seals.

When our five-year third expedition was closed and our results began to be published, it was a common feeling among scientists, led, so far as distinction was concerned, by Fridtjof Nansen, that we had succeeded in living by hunting because we were not very far north—this in spite of what cannot be denied in view of the literature, that previous to our journey the section to the west and northwest of Prince Patrick and Banks Islands had been described as covered with older and heavier ice than any other part of the northern sea which has been described. It is clear from the descriptions of that area, when they are compared with what Peary found between "the big lead" and the North Pole, that the average age, therefore the average thickness, of the Beaufort Sea ice at a distance of 100 miles or more north of Alaska is far greater than the average ice thickness for several hundred miles before reaching the Pole from the Greenland side. That the Beaufort floes are the heaviest known will be reenforced by studying the descriptions of De Long, Nansen, Abruzzi, and the rest of the travelers who have covered the sea in other regions.

Nansen, then, thought our stories true for the region which we had covered but felt sure we would have gone beyond the limits

of seals of numbers sufficient to feed men and dogs if we had gone somewhat farther north. Roald Amundsen was less gullible, or more frank; for in lectures, interviews and books he said that we were plain liars. For instance, in the chapter "On Stefansson and Others" of his autobiography, *My Life as an Explorer*, you will find, among other things:

". . . it is quite certain that one or another who is in search of adventure and new experiences up in the North will be led astray by this prattle about the 'hospitality' there, and that he will actually attempt to do what Stefansson declares that he has done, namely, to venture into these regions equipped only with a rifle and a little ammunition. If they do this it is certain death. A more unreasonable distortion of conditions in the North has never been set forth than that a skilful marksman 'can live off the land.' Stefansson has never done it, although he says he has. Furthermore, I am willing to stake my reputation as a Polar explorer, and will wager everything I own, that if Stefansson were to attempt it, he would be dead within eight days, counted from the start, provided that this test takes place on the Polar ice, which is constantly adrift over the open sea." [2]

However, there were explorers of distinction quite equal to Nansen's who were willing to believe that seals might be living in numbers anywhere in the polar sea and that they might be secured by a competent hunter if he looked for them with sufficient diligence. Peary was the first to express that view publicly, which he did from the platform of the National Geographic Society, Washington, in January, 1919. He had himself noted at least one seal in a lead about 285 miles from the North Pole. His account of this in his book *The North Pole* shows, what he also said verbally, that he was at the time surprised, thinking the seal astray, far out of his proper orbit. Now Peary was willing to concede, upon the evidence we had secured in the Beaufort Sea, that very likely the seal of 85° 53′ N. Lat. had been on his native heath and that others might be found residing at other latitudes, even to 90° N.

However, the mere willingness of Peary and several others

[2] Perhaps because of the libel laws, the section quoted does not appear in this phraseology in either the London or New York edition of Amundsen's *My Life as an Explorer*. We have used a translation made by Isaac Anderson (at one time, and perhaps still, translator of Scandinavian material for the New York *Times*). His version of what Amundsen said about Stefansson in the chapter "On Stefansson and Others" has been published only in *Explorers Club Tales* (J. Allan Dunn, Editor), New York, 1936, and London, 1937, where it appears as the first chapter of the volume.

to take this view did not weight the scale heavily with a majority of the scientists, who continued to hold one or another position from Amundsen's to Nansen's. It was the Papanin expedition which finally settled the dispute. On May 26, 1937, they were delivered by four airplanes with their ten tons of food and scientific and other equipment at a point a few miles beyond the North Pole, looked at from the European side—one reason for which is that the drift was known to be in the direction of the Atlantic so that the camp, if pitched on the Alaska side, would likely drift hard by the actual Pole on its way towards open water. The first morning at the Pole there were singing birds; within a few days the party had been visited by several species of birds. The drifting camp was still in the immediate polar vicinity when a mother bear arrived with cubs so young that they must have been born in that district upon a migrating floe. When the party had time to visit a lead some distance away, they noticed shrimps and later seals. When the traps for animal and plant life were lowered they brought up samples to reveal a life gradient from the surface to the bottom of the polar sea that was not dissimilar to what you find in certain well known portions of the North Atlantic.

It could have been said of Nansen, although not without contradicting his own reasoning, that he had made effective observations and demonstrated the absence of life in the seventies and eighties of north latitude; but that we now had the anomaly (from the old theoretic point of view) of Papanin's discovering more life farther north. That position, if taken, would have been made untenable by the *Sedov* drift which, between October, 1937, and January, 1940, followed a course often parallel to but usually some distance north of the *Fram's,* occasionally right in the Nansen vicinity, for the plankton observations of Badygin resembled those of Papanin.

So Nansen had "demonstrated" the absence or scarcity of life, first by careless or inadequate methods and technique of observation and then by drawing the false conclusion that life was absent or scarce where he had found none or little—instead of the true conclusion that the life might be there but that he had failed to observe it.

This is not, of course, a general criticism of Nansen's methods and logic; it is specific and refers solely to the immediate

question under discussion. There have been few, if any, better all-around scientists that Nansen among those thought of as polar explorers. He was a man of varied abilities, distinguished as philanthropist, diplomat, statesman, writer, biologist and as oceanographer. Few have ever had so small a percentage of detractors among those who knew them well. What we are saying is merely that Nansen had at least one blind spot: he was so hipped on the ancient doctrine of the lifelessness, or the "practical" lifelessness, of the northern sea that he failed to take adequate steps for carrying out on the *Fram's* voyage even those observations which he did make; failed to make a great many which a man with less ingenuity could have made; and then went through the motions of reasoning to the conclusion which he (and nearly everybody else) had arrived at in advance.

We know, then, from the drift of various ships that crews and scientists may be fairly comfortable and reasonably safe in them for many years, and that even in the days before radio and airplanes they could usually save themselves through their own exertions if their ships had to be abandoned. We know from the Papanin drift that you can be as safe as on a ship, and almost as comfortable, if you live in a camp the materials for which have been brought with you by airplane—living also on food so brought. We know from the experience of our 1913–18 expedition that by using a snowhouse, instead of a ship or padded hut, you can be as safe as in either, and more comfortable than anybody has reported himself to be when drifting in a ship, or than the Papanin group were in their camp, to judge by their own narrative. We know from the average health of twenty men for five years (which was excellent but for the mentioned one case of asthma) that you feel better when living on the game you secure from day to day than on provisions which have been brought along—notably you reduce to zero the chance of scurvy and of every other deficiency trouble.

You also find, through comparing the various records of the three methods, that people are in better spirits when living at sea by hunting than when drifting in a ship or drifting in a camp that is supplied with groceries. This is, no doubt, for a combination of reasons—that hunting is an occupation conducive to good spirits, that you tend to be happy or at least content

when you are active, and that such exuberant health as comes with a hunter's life is itself a large factor in producing cheerfulness.

We know from the Papanin example that even the biggest land planes can descend with safety upon the Arctic pack in any of the northerly latitudes; for the expedition led by Professor Schmidt, which delivered the Papanin group at the North Pole, made between latitudes 85° N. and 90° N. around twenty different landings with four-engined airplanes, nearly the largest then in existence, every descent followed by a successful take-off. And we know that if in 1937 four planes were able to deliver ten tons of pay-load at 500 miles from where they took off, we can do at least that well in 1940 or in any year thereafter.

The nature, shape and size of the polar sea are such that you can establish observation camps covering practically or quite the whole of it without any single flight being more difficult than those of Professor Schmidt from Rudolf Island to the Papanin campsite at the Pole.

It has been the experience in the Soviet Union that for every berth on a polar expedition there are 30 or 40 applicants. In the United States, Peary and the rest of us had similar experiences, even with the comparatively limited publicity of those days. Byrd, with modern publicity, is said to turn away literally thousands. There are, of course, freaks who apply in all countries; but it is equally true that some of the ablest and soundest young men volunteer quite as readily as do the freaks. There would be no difficulty in any country about securing twenty suitable candidates for every position on any sane kind of polar expedition, even if there were available several hundred jobs in northern exploratory work.

It has been said by well meaning and kind people that living by hunting on the northern sea has worked on my expeditions because I am an exceptional hunter. Those who study our record, and particularly the members of our expeditions, will not feel that way about it. To be a good hunter is, true enough, a skilled occupation; but it is one that does not require as much training or native ability as, for instance, surgery or engineering. A healthy college graduate of normal physique and good eye sight, with brains to graduate near or above the middle of his class, will have ten or a hundred times as good a chance to

excel me in hunting as he has for getting his name into *Who's Who* through rising towards the top in any of the crowded occupations such as journalism, fiction writing, law, or biology.

Doctors are not considered trained nowadays in less than half a dozen years, subsequent to university graduation, that are spent in medical school and then in some sort of advanced study. There are not, of course, in icecraft, any more than in woodcraft or plainscraft, schools that resemble medical colleges and hospitals. But in my opinion two years of apprenticeship are as nearly adequate for success in northern hunting as six years of medical school and hospital training are in modern surgery. A high school graduate of Boy Scout proclivities will be, inside six months, as well qualified for membership in a self-supporting residence on the drifting pack as a medical graduate with two years of surgical interneship would be to hold a job on an ambulance unit in a war.

From what I know of the Arctic sea and have heard about war, I would judge that being an ambulance surgeon would average something between twenty and a hundred times as dangerous as being a member of the staff of a drifting, self-supporting, Arctic sea observation station.

The suggestion is, then, that knowledge which we have justifies us in planning to establish, within the next year or two, at least five—preferably ten, fifteen or twenty—observation stations upon the ice of the northern sea. The staff of each should be from three to five. They should be placed at their first destinations some time between the first of January and the last of March, for those are the best flying months and the freighting should be done by airplane.

Apart from scientific paraphernalia the equipment of the drifting observation groups would be in the main for camp-life and for self-support, with no more food supplies than are required for the initial stage, while the party is getting its bearings. How compact and light such equipment may well be derives from our experience of the 1913–18 expedition, that one sledge could carry in a load three or four feet high and ten or twelve feet long, at a weight between 200 and 300 lbs., everything necessary to self-support in comfort for three men and six dogs for two years. When you compare this with the ten tons of food

and equipment delivered by Schmidt for the use of the Papanin drifting party, four men and one dog, you realize how much the task could have been simplified without interfering with the prospects of even such notable success as Papanin attained. And his was, in my opinion, one of the most significant and fruitful polar expeditions of modern times.

For only one of the floating observatories do I suggest a permanent location, at the geographic North Pole.

We used to think, and I was one of those who said so, that the drift of floes would average at the Pole between half a mile and a mile per day, which would mean that a station wanting to keep approximately at the North Pole would have to move camp every week or two, when they got five or ten miles away. But the Papanin observations would indicate that, at least in some years, the drift is more like two or three miles a day, which means that you would have to move every few days, traveling each time back approximately to the Pole. This station I would think to be one of the safest that could be located anywhere upon the northern sea. Papanin was in danger only after he had drifted far south; you would have everything comparatively safe and simple, as he did, in the slack waters, two miles deep and 400 miles from land, which are the immediate vicinity of the Pole.

It would not seem advisable, or at least it would be comparatively risky, to establish camps between the North Pole and Iceland, or the North Pole and Spitsbergen. There should be one drift station about halfway between the Rudolf Island observation center, which the Soviet Government maintains, and the North Pole. In a like manner there should be one or two observation stations between the North Pole and the Soviet establishments which now function on Northern Land, at one-third and two-thirds of the distance to the Pole; similarly between the New Siberia and Bennett Island observatories on one side and the North Pole on the other. North of the Wrangel Island Soviet station there should be two or three drift camps, so as to divide into thirds or quarters that meridian which connects Wrangel with the Pole. North from Barrow there should be a minimum of two stations, with two others north of some such place as Cross Island, the more southerly of them in approximately the position where Storkerson drifted in 1918.

A station between Cape Morris Jesup and the North Pole

would have to be watched carefully; for it would be swept into the Papanin course by the eastward drift which Peary observed, particularly north of the Big Lead. North from Cape Columbia, the danger would be less, or less imminent. There should be, of course, land observation stations at Morris Jesup and Columbia; and then the sea station we speak of, about half way from these capes to the Pole.

There should be a station on Borden Island and two stations on its meridian, perhaps at latitudes 84° and 87°. These stations will be particularly interesting from the ocean current side, for the drift may bifurcate so that the camp nearer the Pole would drift east, southeast and south toward the North Atlantic, while the one nearer Borden might either drift that way or else south and southwest toward Alaska.

Because of the high probability that there is a large and sluggish eddy to the northwest and west of Borden and Prince Patrick, and because of the resulting known thickness of the ice and slowness of its movement, there should be more of a cluster of drift stations in this region than anywhere else.

We have emphasized that what we propose is not particularly dangerous work, the hazards surely not greater than those of many common occupations, such as taxi driving or coal mining; but even so a greater safety might be a factor in our judgment on where most stations should be placed, and safety is greatest in this region. The sea to the northwest of Borden is the home of the "paleocrystic" ice, the ice that looks like land and behaves almost like it. On this heavy ice would stand with maximum security the windmills which, likely enough, will supply many of our stations with electric power and light, as they did Papanin; on the same land-like ice might be depots of gasoline and kerosene, which, although not necessities, are much more important in our scheme of work than food supplies—such depots would be safer in this eddy than anywhere else, though the danger to them is nowhere very great so long as a station remains more than 200 miles from land and does not approach too closely the open waters of the North Atlantic.

But the chief argument for having in this region a cluster of stations is that no one knows which way they would drift. They might drift away from each other, more or less radially, northeast, north, northwest, west and southwest—in one or more of

those directions. So we might perhaps erect stations 150 miles northwest from Cape Malloch, Borden Island, 200 miles northwest from Cape Andreasen, Prince Patrick Island, and 250 miles west from Cape Alfred, Banks Island; with shore stations perhaps at Capes Malloch, Alfred and Kellett.

One of these eddy camps will probably establish the record for the longest drift of all the stations we have suggested; for we think that one or more would drift southwest, south, southwest again, and finally west and northwest through the Storkerson region, thereafter following the routes of the *Karluk, Jeannette* and *Fram.*

If there proves to be a genuine eddy to the northwest of Prince Patrick and Borden Islands the floes bearing the stations might circulate for a number of years in that general region, though eventually escaping. If that escape proves to begin with a southwest and south drift parallel to the coasts of Prince Patrick and Banks Islands, then the station might expect still further years of drifting comparable to Storkerson's where he made good only about 100 miles northwesterly while his floe was zigzagging and waltzing a total of 450 miles. This would mean that, after getting away from the eddy, such a floe camp would need five to seven or eight years for reaching the dangerous stretches to the north of the Atlantic where it would have to be picked up by airplane or ship.

We consider, from the experience of our 1913–18 expedition, that if a drift station is manned by volunteers who think in advance that they are going to enjoy it, then about two-thirds of them actually will enjoy it. Nevertheless, it may seem advantageous to replace even contented members of a given staff after three years, except in special cases. Volunteers who find themselves mistaken in having thought they were going to like a hunter's life on the pack ice should no doubt be replaced within two or three months after they, or their local commander, arrives at such a conclusion.

Special cases may arise where discontent and homesickness likely will constitute a serious (or at least a more serious) problem. This justifies a digression.

Some pages back we spoke of a living-by-hunting journey north-south across the ice-covered section of the polar mediterranean as being comparable to an east-west journey by susten-

ance hunters across North America in the Lewis and Clark period. Now we consider in connection with problems of discontent and homesickness those drifting stations which, for meteorological, oceanographic and other reasons, may eventually be placed within a sea desert, perhaps even near the center of one of them. As background for what we have to say we use a statement on ice deserts and their relation to traveling parties which was published by this writer as the last three paragraphs of an article "The Region of Maximum Inaccessibility in the Arctic" by the *Geographical Review* of September, 1920:

"But there are undoubtedly in the Arctic certain 'ice deserts.' These are regions similar to the Sargasso Sea. In them pressure due to winds or currents operating from a distance heaps the ice up, and it may even remain in an eddy for years. We found one such region north of Prince Patrick Island. Seals were not absent but they were comparatively rare, and they became more numerous again when we got farther north. On coming to such an ice desert the traveler who depends on some method similar to ours, where the main reliance for food and fuel is upon seals, will find that he is face to face with a problem similar to that of a traveler who, in crossing an unknown continent in tropical or temperate regions, finds himself gradually entering a desert produced by lack of rain. Such a traveler overland would have to depend upon his judgment. He might avoid the desert by skirting it; he might turn back, giving up his journey for the time being; or he might make a dash across, hoping that his resources would take him to the farther side of the hostile area. Just such a problem one would have to face in ice travel on coming to a region where an eddy existed and where massed ice had evidently persisted for years.

"That is, it would be a problem to a party trying to live by forage. To a company using the Peary pemmican-and-relay system it would constitute no problem at all. They would care about the smoothness, stability, freedom from leads of the ice, and under these heads they might find the conditions excellent—they probably would. For the presence or absence of seals beneath their feet they would care not at all.

"But for those who count on getting their food and fuel as they go, these ice deserts are the one source of gravest concern. We cannot tell in advance for certain where we shall find them, for we can reason only on the basis of what we know and in the Arctic there are still many things unknown. When we come to the edge of such deserts we can guess only very roughly at their extent or in what direction the diameter will be least. They are, in the system of 'living by forage,' the most serious source of danger, although the mere understanding of their existence lessens the danger."

A first consideration about ice deserts, as mentally pictured by us of the 1913–18 expedition from experience with them and from our general views of the northern sea, is that (as mentioned in our earlier comparison with a Lewis and Clark journey) they are moving deserts—meaning that if your camp is the center of a scarcity area of fifty-mile radius on December 1 at 80° N., 140° W., it will still be the center of an area of like dimensions and of like scarcity March 1 if the camp has meantime drifted to Lat. 78° or 82°, to Long. 125° or 135°. A seal, unobtrusively residing in a breathing hole eight miles from you at one of these latitudes and longitudes, will still be residing approximately eight miles from you and at the same or a near-by breathing hole when you get to another latitude and longitude. (This is, of course, one of the points which Nansen ignored when he made his extreme statements about scarcity of life in the polar sea—he speaks as if a drift of a thousand miles with a ship embedded in a large and reasonably permanent floe were the exploration equivalent of a sledge journey across a thousand miles of floe ice.)

In our 1920 quotation we say above that an ice desert presents no difficulty, and perhaps even an advantage, to the Peary method of sledging over sea ice with dependence wholly upon supplies brought along. For like reasons an ice desert has features rather of advantage than of disadvantage for the Papanin method of drifting, where again all supplies came from outside, although freighted by airplane.

It may, then, prove necessary to supply by airplane one or more of the camps we have suggested; for when strategically placed by desiderata of the scientific program they may prove badly located for maintenance as an exclusive hunting camp. Likely, however, food and fuel would have to be delivered only in sufficient quantity to last from the establishment of the camp to the beginning of the summer thaws and rains. For the greater breaking of the ice will then give both seals and bears increased mobility, and it will probably not be long until you have a chance to kill and butcher enough of them in the neighborhood of your camp to create a fairly strong smell that will sweep the ice in various directions as the breezes vary, which brings in polar bears that travel up the scent—from distances of certainly more than ten miles; some believe from as much as twenty miles.

However, it may prove that the summer does not bring in food supplies enough to carry you through the next winter, if you happen to be located near the center of one of the larger ice deserts. Then you will have to freight in most of the supplies for that station by airplane. However, this will not be as much of a problem as the one solved in 1937 by the Schmidt-Papanin establishment, for they brought 20,000 pounds 500 miles. By using snowhouses in place of their padded hut, by depending at least in some part on fresh meat, and through various economies that are suggested by the experience of the Soviet expedition, it ought to be possible to reduce the freighting to less than half of the ten-ton problem.

The greater difficulty with an ice desert location served by freighting planes will be the comparative monotony of life. There will be decreased variety and thrill of experience when the men who stand watch-and-watch for the meteorological shifts listen for the dinner bell instead of being on a keen lookout for prowling bears. Nor will the arrival of a supply plane once every two or three months take the place of the interest and activity that come from hunting seals through a wide area of neighboring ice fields, particularly when using *auktok* and *mauttok* techniques, for they occupy more time, require more skill and are generally better for pastime than the hunting of seals in open leads.[3]

It may be, then, that the personnel of those drifting camps which have a minimum of hunting opportunity, and which need to be supplied largely by airplane, should be renewed more often than every third year, as we have suggested—perhaps each year, or every other year.

It will be a rule in the establishment of the observatories that none shall be placed in an initial position that is less than 200 miles from land; or in any position such that from our present (imperfect) knowledge of the currents we would think the camp likely to be swept to closer than 100 miles from any shore within the first six months or year. For such sledge travel as that of Peary north of Ellesmere-Greenland, and ours north of Alaska and northwest from the Canadian islands, will indicate that ice pressures, and the crushing of ice, with greater inconvenience

[3] For description of the *auktok* and *mauttok* techniques, see *The Friendly Arctic*, pages 301–310.

and some increase of danger, are worst near shore, and comparatively bad for a hundred miles from shore; while at distances of more than 150 miles from land ice movements are relatively sluggish and pressures small. There is, of course, one halfway exception to this rule, north of the Atlantic. There it remains true that pressures are mild; but it is not true there that movements are sluggish, for it is evident from the Papanin record that speeding up of the drift is fairly consistent as the pack approaches "Gulf Stream" waters when coming from the approximate direction of the Pole.

What novelty there is in our proposal for drifting scientific observation stations that are to be largely self supporting may call for a few supplementary paragraphs on how life would be in the drifting camps.

There would be one season of moderate discomfort, the summer. This will be longest in camps that are nearest the margin of the ice, where security will also be least. We are assuming that any station which drifts too near a margin will be picked up and taken home through such technique as the Soviet Government used at the termination of the Papanin and Badygin expeditions.

What the summer discomforts of a drifting camp are may be seen from the Papanin accounts and from descriptions of summer travel over pack ice, for instance in my book *The Friendly Arctic.* It is perhaps easiest to get a picture of summer in a hunting camp by reading Storkerson's account, printed in the same book, of how a party of five spent that season in 1918 on their mentioned 450-mile, six-month drift 200 and 300 miles north of Alaska.

The discomfort from thaw weather and rain is less when you are in residence than when you are traveling, for you can keep your things dry more easily.

There is moderate discomfort in a drift camp during spring and autumn. Again, the particulars will be seen through reading the descriptions and narratives of my mentioned book. The period of slight discomfort following the cessation of the rains in the autumn, and the other before the start of the rains in spring, are caused through the necessity of living in tents. However, double tent covers would be used, preferably on the air-space style which, I believe, was first developed by the Shackle-

ton party in the South and which was used on my third expedition.

From the autumn when temperatures begin running as low as ten above zero, and thence through the winter until the same temperatures come in the spring, life will be as comfortable in a snowhouse drifting camp as in a hotel room—with the exception of short warm weather spells, when the thermometer goes higher than 20° above, which spells may occur at any latitude in any month of winter.

When temperatures are 30° and 40° below zero, snowhouse interiors will be maintained at 50° or 60° above, even when the buildings are unlined. If they are lined, the temperatures will, of course, be higher—70° or 80°. For maintaining this warmth, the expenditure of fuel is in a sense immaterial; for when you kill enough seals to provide yourself with all the lean you need, there is always a superabundance of fat. This would remain true even if each camp were to have a dog-team as well as a staff of men to feed.

There is among the public, although not among scientists, a misapprehension that the "long winter darkness" is very dark. Commander Louis Bernacchi said in connection with the first Scott expedition, for which he was astronomer, that there is no pitch darkness in polar regions, north or south, even on a thickly overcast night with no moon in the sky. This means that two dark-clad men can note each other's movements at distances of several hundred feet, if there is no fog or falling snow. As Nansen pointed out from his drift in the *Fram* there is no fog at high Arctic sea latitudes from December to March inclusive, and there are few thickly clouded days. Even on December 22 noon daylight will be wholly wanting only from the station which is to remain at the Pole and from those others which are drifting north of 83°. However, the main lights of midwinter at all latitudes north of 75° will be the moon, aurora and stars, each being approximately twice as effective, because of reflection from the nearly uniform snow covering of the landscape, as if the surface were black or green. With the use of 2½ power telescope sights on rifles the shooting even of seals can be done throughout the winter by natural light, while polar bears visiting the camp can be shot in the ordinary way.

The flying of Wilkins when searching for Levanevsky from

late August, 1937, to early March, 1938, showed that any camp located anywhere on the drifting pack can be reached by airplane at any time. For, remember, he flew 10,000 miles over the polar sea in autumn, which is recognized as the worst time of year, and then another 10,000 miles in midwinter (between December and March), some flights of nearly 3,000 miles being done without benefit of daylight except for a few hours of twilight; and all of them without accident in the field. (There was one slight accident, the damage of a propeller, but this occurred at the home base, the northerly Canadian airport of Aklavik in the Mackenzie delta.)

Naturally the floating camps would all use radio. It would be necessary to have on shore, in connection with our plan of sea drifting, at most six air base stations for supply and for "mercy flying" (as, for instance, if a member of one of the drifting parties needed an appendicitis operation). Two of these bases should be on Soviet territory, two in Canada, one in Alaska and one in Greenland. For the plan, naturally, calls for international cooperation.

PEARY'S TRANSECTIONS OF NORTH GREENLAND, 1892–1895

HUGH J. LEE

Peary's Companion in Greenland, 1895

(Read February 24, 1940, in Symposium on American Polar Exploration)

THE late Admiral Robert Edwin Peary, who was the first man to reach the North Pole, was one of the pioneers in the exploration of the great inland ice cap of North Greenland, following up and extending, as he did, the work begun by that previous American Arctic explorer, Dr. I. I. Hayes, who was the first man to penetrate beyond the coastal fringe of the ice cap, in October, 1860, while on a voyage in search of the supposed "open polar sea".

Peary was the first man to cross Greenland north of the Arctic circle, not only crossing it, but returning over the great ice to his headquarters, making it a double journey. He was the only man who, having crossed and recrossed that frozen Sahara, showed the fortitude to undertake and execute a second double transection of that great ice field. It was on this second crossing that I had the honor of accompanying him.

Even to this day, more than thirty years after Peary reached the Pole, we read in newspapers and magazines articles by irresponsible writers, casting, or trying to cast, doubt upon Peary's accomplishment. All persons familiar with polar exploration know that he did reach the long-sought point, the North Pole, on April 6, 1909; and those not so familiar might well take the record of the meeting of the Royal Geographical Society in London in May, 1910, when the Great Gold Medal of that society was presented to Peary, as sufficient authority for believing in him.

Major Darwin, then president of the society, in presenting the medal for "Arctic Exploration from 1886 to 1909", said: "I stand here tonight as the representative of the Royal Geographical Society, and armed with the full authority of its council, to welcome you, Commander Peary, as the first and only human

being who has ever led a party of his fellow creatures to a pole of the earth".

On that occasion, after Peary had delivered his address, Admiral Sir George Nares, veteran British Arctic explorer, moved a vote of thanks, and, in seconding the motion, Admiral Sir Lewis Beaumont, also a distinguished British Arctic explorer, said:

"You have heard from him how the splendid success which at last crowned his efforts was only won by a combination of twenty years' experience and the long and patient training of the Eskimos to do his will; but I would like to remind you that in his first great journey, to which I have just alluded, (Peary's first crossing of Greenland) he had with him only one European, and no Eskimo; and that the fame of this, his apprenticeship in Arctic sledging, has only receded from view because of his more brilliant later achievement."

In connection with this special Great Gold Medal to Peary in 1910, it might be pertinent to recall that it was designed by Lady Scott, wife of Sir Robert Falcon Scott, who was, two years later, to lose his life in the frozen wastes of the Antarctic, after having reached the South Pole.

Although Greenland had a thousand years of history before Peary began his exploration of it, it had never been penetrated beyond its mountainous coastal fringe of ice-free mainland and islands, until the latter part of the last century. The native Greenlanders peopled it with supernatural beings, and were afraid to go into the interior, although their hunting excursions took them to the very rim of the great ice sheet.

According to Crantz (Crantz's *History of Greenland,* English translation, London, 1767, pages 21–25) Lars Dalager, a Danish factor, on August 28, 1751, with several Greenlanders, starting from a point south of Fredrickshaab, in north latitude about 60½ degrees, attempted to make a trip over the ice cap to the east coast.

On September 5 he and his party reached the summit of a mountain or nunatak within the margin of the ice cap, from which he had an extensive view to the eastward, where he saw the "spacious field of ice" stretching across the country.

He said, in summing up the results of his trip, that the surface of the ice cap could be traversed without danger, but it would very likely be impossible to make the journey to the east coast

because no one could carry sufficient provisions for the length of time it would take, and he considered it impossible for a living creature to draw breath in such intolerable cold, especially as one must camp for many successive nights on the plains of ice.

The next serious attempt to penetrate to the interior of Greenland, of which I have found a record, was that of Dr. I. I. Hayes, who started from Port Foulke in latitude about 78¼ on October 22, 1860, a week after the sun had sunk from sight for the winter in that latitude.

He was accompanied by five men, hauling sledges, and in three days' march from the foot of Brother John's glacier at the head of Foulke fiord, he reached a point 70 miles from the coast and 5,000 feet above the level of the sea. (*The Open Polar Sea,* by Dr. I. I. Hayes, New York, 1867, pages 130–135.)

At their farthest camp, which was in the immediate vicinity of Peary's line of march on both of his transections of Greenland over thirty years later, the temperature fell to 34 degrees below zero, accompanied by a gale. They broke camp, and with the wind behind them made their way back to their ship in two days, where they found the coldest weather had been 12 degrees below zero, or 22 degrees higher than at their camp on the inland ice.

Ten years later Professor A. E. Nordenskiold and Dr. Svend Berggren made a successful attempt to cross the margin of the Greenland ice cap at a point a little south of Christianshaab in latitude about 68¼ degrees. Almost at the beginning of their journey, their two Greenlanders left them, and they continued inland without them. They reached a point 2,200 feet above sea level and 30 miles from the edge of the ice cap. The traveling was very difficult, and they were impeded by crevasses, water courses and lakes, but the weather was clear and mild. They could see nothing but the ice cap, sloping upward to the east (*The Arctic Voyages of Adolf Eric Nordenskiold,* London, 1879, pages 156–197).

According to Greenland legend there was believed to be an ice-free region in the interior of the country, and Professor Nordenskiold thought there were many reasons to believe that the inland ice merely formed a continuous frame around a land free of ice, perhaps even wooded in the southern part, which might be of great economic importance to the rest of Greenland.

Nordenskiold made a second expedition into the interior of Greenland in 1883, starting on July 7 from approximately the same point as in 1870. In fourteen days of very toilsome travel, through half-melted snow, waist deep, he penetrated to a point 78 miles from the margin of the ice. Here two Laplanders, who accompanied the expedition, were sent overhead on skis. They returned in 57 hours and said they had reached a point 150 miles from the coast and 5,000 feet high.

It was at this point, while but little of the true nature of the ice cap was known, that Peary entered the picture. He had read Nordenskiold's account of his trips onto the ice cap, and, of course, Dr. Hayes' account, and he determined to make a more extensive exploration.

He decided to make first a reconnoissance to determine the kind of equipment best adapted to travel there, and in 1886 he went to Godhaven, capital of the northern Inspectorate of Greenland, as a passenger on a whaling ship. From there, with a Dane, Christian Maigaard, and a number of natives, he went to the mainland at the head of Disco bay, a point in latitude about 69 degrees, north.

When it came to starting actually on the ice cap, the natives refused to go, just as had those of Nordenskiold in 1870, but, with his one companion, and pulling hand sledges, Peary began the ascent of the ice on June 28, and continued his eastward march, with several interruptions, due to weather, until July 19, when he found himself 110 miles from the margin of the ice and 7,500 feet above the level of the sea. For some distance they were able to use snow shoes, for the surface, except within a comparatively short distance of the land, was covered with dry snow.

Thus Peary, on his first trip into the interior of Greenland, attained a greater elevation than any previous explorer, and penetrated a greater distance than any other white man. He attained for the first time the real interior plateau of unchanging snow, and determined the ruling characteristics of the inland ice from its border to the interior.

He learned that the surface of the ice cap offers what he termed an "imperial highway" to the east coast. He then proposed a journey from Whale sound to the northern terminus of Greenland, or to the intersection of the ice cap with the east coast; or, as an alternative, one from approximately his former

starting point in Disco bay to Cape Dan on the east coast, in approximately latitude 66 degrees.

In commenting on this first ice cap journey of Peary, Fridtjof Nansen, the Norwegian explorer, said : "No previous explorer had, with such simple equipment and at so little expense, achieved such excellent results as this first Peary expedition, which may, with justice, be said to have been admirably planned and admirably executed" (*Fridtjof Nansen,* 1861–1893, by W. C. Brogger and Nordahl Rolfson, English translation, London, 1896).

When Nansen learned of Peary's successful reconnoissance in 1886, and of his plan to continue his exploration of that country by crossing the inland ice, he hastened to perfect his own plans for crossing Greenland. Peary, recalled to duty in Nicaragua, was unable to return to Greenland immediately, and thus Nansen was the first to cross Greenland, making the trip in 1888 in latitude about 64 degrees, north, over two degrees south of the Arctic circle, where the distance was much less than Peary's proposed route.

It was on August 16 when Nansen and his companions began the ascent from a point on the east coast. Laboriously plodding upward, dragging their sledges, they finally reached an elevation of between 8,000 and 9,000 feet, and experienced temperatures as low as 49 degrees below zero. From August 21 until September 24, when they reached land on the west coast, they found no drinking water, and consequently experienced a burning thirst. They spent the winter at New Herrnhut, and returned to Europe the next spring.

Peary then decided to undertake the exploration of the northern part of the ice cap, and, with a small party of assitants, and accompanied by Mrs. Peary, he reached McCormack bay in the Smith sound region in August, 1891, and established his headquarters on the south shore, not far from Cape Cleveland.

Here the winter was spent preparing for the ice cap journey in the spring, winning the friendship of the Eskimos, acquiring experience in dog team sledging and in camping in frigid weather. Peary was handicapped at the outset by a fractured leg, but he supervised the work of the expedition, although he could take no active part in it until the fracture had healed.

During the early spring the bulk of the supplies for the in-

land ice trip had been hauled up the steeper slopes at the head of McCormack bay, and he left his headquarters the first week in May, accompanied by three men, and with twenty dogs. The climb was steep and storms delayed, so it was not until the 15th of the month that the region of the great Humboldt glacier was reached.

From this point he set his course northeast, true, at an elevation of 5,000 feet above the sea, and soon reached the summit of the divide between the Humboldt glacier and Whale sound. He was then in approximately the spot where Dr. Hayes had camped thirty-two years before.

His northeast course just cleared the ice bluffs at the head of the great glacier, but, fearing others ahead, he deflected his course five miles to the eastward, and then resumed his march in the northeasterly direction. The surface was slightly down grade, dipping into the depression at the head of the glacier. A fierce storm delayed them for 48 hours, but the pounding of the wind had hardened the surface, so that the traveling was easier.

On May 24, when 130 miles from McCormack bay, the supporting party of two men left to return to headquarters, while Peary and one companion, Eivind Astrup, continued, and on May 31 they looked down into the basin of the Petermann glacier. Keeping to the northward in foggy weather, they were not able to see just where they were going, and they approached too near the mountains of the coast, and thus became entangled in the rough ice and crevasses of the Sherard Osborne glacier system.

They had to make a long detour to the eastward and southward, and were two weeks getting back to the smooth, unbroken ice cap of the interior, and had lost several miles of their northing in extricating themselves from the trap. They continued their march northeastward until June 27, when they sighted mountains directly ahead. The northwest entrance to a fiord came into view, the course of which they could trace to the southeast, extending behind the mountains in the north and northeast.

He changed his course to the east, and soon sighted land in that direction, so he veered to the southeast and continued until July 1, when he reached a moraine of broken rock 3,500 feet above sea level. They had reached a point on the northeast coast of Greenland at the head of a fiord or bay, which Peary

called Independence bay, from the fact that he first looked down upon it on July 4.

On this mass of mountainous land or nunatak, which was 300 or 400 miles in extent, they killed several musk oxen and sighted many more. They remained on that land a week, making observations and hunting, and they erected a cairn and left a record on a steep bluff, which Peary named Navy cliff. This cairn is in north latitude 81 degrees, 37 minutes and 5 seconds, and in west longitude 34 degrees and 5 minutes.

It took two days to get from Navy cliff back to their moraine camp, and the traveling over the naked, broken rock was extremely hard. Peary called it the worst traveling he had found in Greenland. On July 7 they turned their backs upon this new land and began the long journey back to McCormack bay. They set a course more to the eastward than their outward one, with a view to avoiding all the rough ice and crevases at the heads of glaciers, which had impeded them before; thus their return march was much shorter than their northward one.

However, they found harder sledding, for they were in a much higher elevation for about two weeks of the return journey, being from 8,000 to 9,000 feet above sea level, where the snow was soft and light, allowing the sledge runners to cut in. When they approached the latitude of the Humboldt glacier, they were down to an elevation of from 5,000 to 6,000, and the surface of the snow was much harder, having been scored by the winds, and their progress was much more rapid.

On August 6, when but a few miles from McCormack bay, both men in excellent physical condition, with ample food supplies and the dogs in good condition, they met Professor Angelo Heilprin and a relief party, which had started out to look for them, and the reunion was a happy one. The ship was in port, ready to take the expedition home, and it sailed southward on August 24.

Peary, in his first crossing of Greenland, had made a sledge journey which was unique in respect to the distance traveled, about 1,200 miles, with but one companion, and without a cache from beginning to end. The results of his first transection of Greenland were the delineation of the northern limits of the main ice cap, the determination of the rapid convergence of the shores of Greenland north of the 78th parallel, and the observation of the relief of a large area of the inland ice.

Peary, on this trip, was the first to observe the outward movement of the air over the Greenland ice cap. Whenever he was going up a slope, the wind was found to be always in his face, but in going down a slope, it was found to be at his back. Thus he observed that the flow of the air along the surface resembles that of a liquid flowing down a slope under the influence of gravity (*Exploring About the North Pole of the Winds,* William Herbert Hobbs, New York, 1930). Thus Peary was the first observer of what is now technically described as the Greenland glacial anticyclone.

Peary now began to turn his attention to attaining the North Pole, which became the great objective of his future exploratory work in the north. He had found an apparently abundant supply of musk oxen at Independence bay, and he believed the land there could be made an advanced base, only about 500 geographical miles from the Pole.

He planned to return to Independence bay in the spring of 1894 with seven companions. While one group would remain on the land there, hunting and storing up meat for use on the return trip to Whale sound, another group would travel northward over the sea ice, toward the Pole, while a third group would explore and map the eastern coast of Greenland.

On August 3, 1893, Peary arrived in Bowdoin bay, on the north shore of Inglefield gulf, at a site selected by him the previous year, with a party of eleven men, besides himself and wife and Mrs. Cross, a nurse and cook. No sooner had a house been built for the winter quarters than the work of advancing supplies into the interior, for use in crossing the ice cap next spring, was started.

The work was pushed vigorously all through September and October, and even into November, long after the sun had left for the winter, and in spite of an almost continuous series of severe storms they succeeded in depositing the entire cache of supplies 26½ miles from the margin of the ice, and 5,000 feet above sea level.

During this fall campaign on the ice cap, a very interesting event occurred at Anniversary lodge, as the headquarters house was called, in the birth on September 12 of a daughter to Lieutenant and Mrs. Peary, who was to become famous as the Snow Baby.

It required fortitude indeed to plan thus for another expedition over the ice cap, knowing it as he did. Extreme cold weather may be pleasant if there be no wind, but the wind, said Peary, is never quiescent on the great ice. Day and night it is sweeping down, sometimes with greater and sometimes with less velocity, bearing with it a burden of snow. During gentle breezes this snow is of almost implacable fineness, and extends but a foot or two above the surface. As the wind increases in force, the particles become coarser, and the depth of the layer of the flying snow increases to several hundred feet.

But to quote Peary: "The character of the great ice is such as to make a powerful impression upon even the most prosaic mind. When I think of it, I rarely recall the hunger, the cold, the killing work, the disappointments I have experienced upon it."

Let me quote a remark of Christiansen, one of Nansen's men, to Diedrichson, a companion, as they were struggling up the ice cap in much lower latitudes than those traversed by Peary: "Good Lord, to think of people being so cruel to themselves as to go in for this sort of thing."

The winter of 1893–94 was passed at the lodge preparing for the start on the long journey on March 1, a few days after the reappearance of the sunk which would give light enough to make fair days' marches. Peary and Astrup, on their journey of 1892, had suffered tortures from body lice. Their furs had been obtained from Eskimos and Eskimos women had made the garments, and, while they were thoroughly cleaned, some of the nits remained in the furs, and hatched out and multiplied. The difficulty of removing underwear in the frigid temperatures made it impossible to escape them.

In an attempt to lessen this annoyance, the clothing for use in the spring of 1894 was made early in the winter, and hung out on the frame of the weather observatory, with the thought that temperatures in the 50's below zero would banish from the garments what, during the World War, came to be known as "cooties".

The start was made on March 6, and in the bitter cold but slow progress was made up the steep slopes, so that it was March 12 before we finally left Cache camp and headed northeastward, Peary and seven men. My left foot was frozen, and

Astrup was ill, just as he had been on the ice cap the previous autumn, so that on March 14, the Lieutenant and Clark took Astrup and me back to the lodge, leaving but six in the advance party.

A terrible storm swept over the ice cap on March 21 and 22, the temperature ranging down to 62½ degrees below zero, and the average velocity of the wind for 34 hours was 48 miles an hour. Davidson's heel was badly frozen, so that he had to be sent back. Dr. Vincent accompanied him so he would be at the lodge to care for all the disabled men.

This left but four for the advance party, and they struggled on until April 10. Another storm had delayed them, and they were but 124 miles from the moraine, and about 5,500 feet above sea level. The dog disease, with the cold and storms, had so decimated the teams, and the condition of the men and the remaining dogs was such that Peary decided to give up the trip for that year, and try it again in 1895, starting a month later to avoid the March storms.

Peary and his three men were back at the lodge on April 20, their faces completely covered with great scabs from frost bites. Entriken's and Clark's feet were badly frozen, all were snow blind, and all were absolutely covered with "cooties", in spite of the freezing to which their clothing had been submitted before the start. They had suffered the tortures of the damned.

On August 26 all the members of the party except Peary, Henson and Lee sailed on the *Falcon* for the states. All of us had signed a contract that Peary should be the sole judge of the time to be devoted to the expedition, but when Peary called for volunteers to remain another year, none of the others volunteered.

In July I visited the cache at Equinoctial camp, as we called the place where the big storm had overtaken the party in March, to obtain some supplies for use at the house, and I found the eight foot bamboo pole which marked the site covered with snow except a few inches at the top. I restored the cache to the surface and reset the pole, and from this circumstance we had some indication of the large accumulation of snow there within a short time.

In October we failed to locate any of the nearer caches, and Peary realized that the big pemmican and biscuit cache 128

miles from the house might be lost, so through the fall and winter we gathered in all the walrus and reindeer meat possible, planning to start on April 1 with full loads of that. I was seriously ill of stomach trouble during March, but recovered so the date of departure did not have to be postponed.

On March 28 I wrote in my diary: "I dread the ice cap journey, but I shall do my best to make a success of the trip across and beyond." One of the annoyances of previous trips would be missing this time, for we had discovered, much to our joy, that the liberal use of camphor in our clothing would keep the "cooties" away.

The start was made on scheduled time, April 1. We were accompanied by a party of four Eskimos with their dog teams for the first week, every sledge loaded to capacity with walrus meat for dog food and reindeer meat for ourselves. Our plan was to rest a couple of days at the big cache, 124 miles from the moraine, feed the dogs to their fill on walrus meat, and cache the remainder there for the return, proceeding with full loads of pemmican instead of raw meat.

Good progress was made, the winds of March having hardened the surface, so that at the end of six days we camped in the vicinity of the cache. In my diary entry for April 7, I read: "Today we did not move camp. The day was spent in search for the pemmican cache. We did not find it, however, and have decided it would be a waste of time to search more. Lieutenant Peary offered to turn back if Matt and I objected to going on without pemmican, but we are willing to take a chance on making it with deer meat for ourselves and walrus meat for the dogs."

On the 8th the Eskimo supporting party turned back, and Peary and his two men continued their march, northeast true. During the following week I was quite ill, a recurrence of the stomach ailment, resulting in delays, so that but 76 miles were covered. The next week we did better, making 95 miles, but the dogs were tiring and growing thin, and we, ourselves, were suffering with hunger, as the raw meat rations we were able to afford dogs and men were insufficient in the cruel cold, and with the strenuous work.

In order to get on, we had to feed the dogs double rations for a few days, and the next week we succeeded in covering 107

miles. The average elevation for that week was 7,670 feet above the sea, and the maximum, 7,865. This was the summit of the divide between the east and west coasts, and, as we continued our course, we were very materially helped by the down grade. On April 30 we made 23 miles, and the next day, May 1, 27 miles.

The dogs were rapidly playing out, and we had but about a dozen left of the forty-one with which we left the lodge. As we advanced down grade, we realized there was no need to haul all our supplies down the slope, as we would need some for the home journey, so we made a cache. When we camped on May 6, we were not far from land, somewhat west of the Independence bay land that Peary had visited in 1892.

While I remained in camp nursing a frozen toe, which had been causing me trouble for about ten days, Peary and Matt, with a hand sledge, visited the land to hunt for game. They returned on May 9, having found none. Our dog food was practically all used up, and Peary offered to start back at once, saying he did not believe he had the moral right to take the lives of his two men into his hands and proceed. However, both men volunteered to go on to the Independence bay land, in the hope of replenishing their supplies with game.

We then returned to our cache, something over 20 miles back, and there we left a sledge, all our deer meat, and what biscuit we figured would be scant rations for the return across the ice cap. In two days we then covered the 36 miles to the moraine where Peary and Astrup had landed on July 1, 1892, reaching that point on May 13.

On this land six musk oxen and four calves were secured, but we hunted in vain for more, and in the meantime had found that it was impossible to get down onto Independence bay without going back onto the ice cap and seeking another route, because of the precipitous cliffs along the coast. We realized that with the scanty supply of game we had obtained, there was no chance of making northing on the sea ice, and the only thing to do was start back across the ice cap.

We visited the cairn on Navy cliff and left a record of our visit, and then worked our way back to the camp on the moraine, hunting for musk oxen all the way. Traveling on this land was the hardest work imaginable, with broken sledges and aching backs and muscles all the time.

On June 1 we started up the slope of the ice cap again, and in two marches reached our cache, from which point we set our course southwest true. In the first week we had covered 155 miles on our way home, pushing forward to the utmost, for we know that unless we made long marches our food would not last. The next week I was ill again for several days, and on June 9 was unable to travel, but we managed to cover 130 miles, although we had but four dogs left.

In the third week we traveled 135 miles, being delayed some by bad weather. Our meat was all gone and we had but a few biscuit left, and very little oil for making tea. The next two marches we made 40 miles, delayed by stormy weather and in making observations, the first on the return trip. They showed that we were about 20 miles nearer home than our dead reckoning had placed us.

We stopped to rest 20 or 25 miles from the home moraine, and abandoning our sledge and camping equipment, struggled on accompanied by our one surviving dog, first one and then another ahead, as we stopped from time to time to stretch out on the snow and rest. On June 25, Peary and Matt reached the land ahead of me, and, as I came down the slope from the moraine to the old familiar trail, I found them lying on the ground beside a small stream, sipping water, the first we had had since April 1, except that melted by the expenditure of precious fuel.

We had crossed from moraine to moraine in 25 marches averaging 20.1 miles each. We had been fortunate as to weather, for one of those protracted storms would have delayed us, making it impossible for us to win our race against starvation. Faces covered with great scabs from frost bites, blear eyed from the glare of the sun, filthy beyond description, for when one has no water to drink, he will not use any for bathing. Frederick G. Jackson, the British explorer of Franz Josef's land, is the only Arctic explorer of whom I ever heard, who stripped to the waist and bathed every morning, while in the field in extreme temperatures.

Mr. Stefansson tells us of the "Friendly Arctic", but there is no friendliness in the ice cap of Greenland.

Peary had now convinced himself that the North Pole could not be reached via the ice cap, as the distance is too great to sledge supplies from Whale sound, and he next turned his atten-

tion to utilizing Fort Conger, the old Greeley headquarters in Lady Franklin bay, as an advanced base. In sledging his supplies there in the winter of 1898–99, he froze his feet and lost several toes, but he persevered, and, with that as a base in the spring of 1900, attempted a journey toward the Pole from the northern extremity of Greenland.

He left Fort Conger on April 15, crossed Robeson channel and laboriously hewed a road for his sledges along the ice foot and over the heaped-up pressure ice along the northwest coast of Greenland, past the farthest north of Lockwood and Brainard, and on May 13, he reached the most northerly land, which he called Cape Morris K. Jessup.

From there he headed straight north over the sea ice for three days and reached open water in latitude 83 degrees, 50 minutes, and returned to land on May 17. He then proceeded eastward and southward to a point in latitude 83 on the east coast, from which the land trended in the direction of Independence bay, and then, retracing his steps, reached Fort Conger on June 10.

The Greenland route, he had demonstrated, was not the way to reach the Pole, as the fields of ice are constantly sweeping around the northern end of Greenland into the southerly current of the East Greenland sea, and thereafter he devoted his efforts to the sector north of Grant Land, where he finally achieved success.

PEARY'S EXTENDED EXPLORATION OF ARCTIC LANDS CULMINATING IN THE ATTAINMENT OF THE NORTH POLE

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PEARY's extended Arctic explorations have been covered so well by Mr. Hugh J. Lee, his companion during the crossing of Greenland in 1895, that I shall confine my paper to his progress toward the Pole, beginning with the work done aboard the Peary Arctic Club Steamer *Roosevelt.*

This was a new type of Arctic ship and Peary had a lot to do with her designing. For many years he had been going north in Newfoundland sealers which had been built in Scotland and Norway. Naturally he could see the good and bad points in their construction and made many improvements in the Roosevelt which made her more efficient for the work in hand.

The *Roosevelt's* keel was laid October 15, 1904, and she was launched March 23, 1905. Peary wrote: "When it came to christening the ship by whose aid it was hoped to fight our way toward the most inaccessible spot on earth, the name of Roosevelt seemed to be the one and inevitable choice. It held up as ideals before the expedition those very qualities of strength, insistence, persistence, and triumph over obstacles, which have made the twenty-sixth President of the United States so great".

The *Roosevelt* left New York on her first trip north on July 16, 1905, and I was her captain. My uncles Harry, John and Samuel had previously been Peary's captains and I had always contemplated succeeding these men when they retired. So when Peary asked me I gladly accepted. My cousin Moses Bartlett went along as mate. Professor Ross G. Marvin was Peary's secretary and Doctor Louie J. Wolf was surgeon. Matt Henson, Peary's colored assistant, who was an expert dog-driver and sledge builder, went along also. The crew was composed of Newfoundland sealers and fishermen.

Soon after leaving North Sydney leaks developed in our Almy boilers and we had to depend on the Scotch boiler but we finally arrived at Etah, having found no ice in Melville Bay. From here, heavily laden with an extra supply of coal transferred from the auxiliary steamer *Eric,* dogs, walrus meat and Eskimos, we fought our way through the ice-encumbered channels of Smith Sound, Kane Basin, Kennedy and Robeson Channels. In a bight on the Bache Peninsula, between Victoria and Albert Heads we left a supply of provisions. Off Cape Lupton, in an ice rafter, we twisted the back of the rudder and broke the collar that kept the rudder from slipping down on the toe of the rudder post but temporary repairs enabled us to get around Cape Sumner and tie up to the fast ice in Newman Bay.

Peary and I climbed to the summit of Cape Brevoort to get some idea of the ice conditions ahead. It did not look any too good but we put the ship in the ice and crossed our fingers. It was a hard fought battle trying to make the Grant Land shore but the Roosevelt was new and strong and we had a good gang in the boiler room. Her twisting qualities and great horsepower finally got us around Cape Union to Cape Sheridan and two miles beyond Floeberg Beach, the winter quarters of H.M.S. *Alert* in 1875–6.

We made many attempts to get inside the heavy ice ridge, even using dynamite, but were unsuccessful. So we had to take our chances and make our winter quarters astride the hinge of the ice-foot. Because of the danger from fire or the possible destruction of the ship from the rafting of the ice we landed our boxed provisions from which we constructed a house, lining the walls with snow-blocks.

When the sun left us in October we carried on our sledging trips through the moonlight nights and thus accustomed ourselves to sledge traveling and Eskimo ways. We soon caught on to their technique and lived off fresh muskox, seal, caribou, polar-bear and frozen trout from Lake Hazen, which kept us healthy and free from scurvy. Thus the New Year found us in good spirits and eager for the assault on the Pole.

The route to the Pole led along the north coast of Grant Land to Point Moss, twenty miles beyond Cape Hecla. From this point the trail across the Polar pack was to be as straight as possible. The plan of assault was to fix an advance base some

250 or 300 miles out from Point Moss and divide the distance into 50 mile sections. Separate divisions of men, each in charge of a white leader and comprising two or three Eskimos with dog teams and sledges, would start at intervals for this advance base. The pioneer party had to select the best route so had the pick of the dogs and Eskimos and light sledges. At the north terminus of each section the leader would transfer his load of provisions to the division ahead and return for a new load from the division next behind. Any excess provisions were cached along the route. Peary was to stay in the rear and come up the line for his final dash for the Pole. Owing to high wind conditions which tore up the ice the system did not work but I believe it could have been carried out if the weather had been favorable.

Acting in accordance with this plan I left Cape Hecla about the middle of February and went on to Point Moss. Dr. Wolf's and Marvin's party with Peary followed. On the 26th we were all at Point Moss and Henson started out over the Polar Sea ice on the 28th with the pioneer party and three light sledges for four marches. I followed the next day. Then came Clark and his party; then Dr. Wolf, Marvin, Ryan and Peary, who left on March 6th. Heavily rafted ice was encountered and we had to use pick-axes continuously. After Henson had made four marches I took charge of the pioneer party. It was a case of many long, weary hours and we did not get very far. Peary caught up with us on March 26th. In my division was Henson and Clark. We had much open water to contend with and Peary finally decided to send Clark and me back to the land for more provisions. The latitude by observation was 84.48° North and the approximate longitude 74° West.

We made good time going back over the outward trail. Spending one day on shore repairing and loading sledges we left with full loads to join Peary. In the meantime Peary kept working northward, meeting much open water and we experienced the same conditions inshore. Then came the fierce gale out of the west which proved so disastrous to our plans. It shifted the ice with Peary and his party seventy miles to the eastward. Marvin and Clark were nearer shore so their drift was not so great. My division and the one in charge of Dr. Wolf had gone back to the land for more supplies; also Ryan's party.

After the storm we began to work north again but it was a

hopeless job. I stayed out on the Polar ice until June 5th and, returning, picked up Dr. Wolf's party. Ryan's division had followed our trail out. After taking all the provisions he could spare I sent him back to the ship.

When the gale had blown itself out Peary located himself in latitude 85.12° North and longitude approximately 62° West. From here he made his farthest north to latitude 87.06° on April 21st. Both Marvin and he reached the ship around June 1st. Their many narrow escapes from drowning and starvation would take many pages to tell.

Peary was back on the ship only a short time and then left with his three Eskimos, Koolootingwah, Egingwah and Ooblooyah with six sledges and thirty-nine dogs to explore the glacial fringe of Grant Land. Perhaps he felt that this might be his last Arctic trip and it was part of his plan to close the gap in the map of Western Grant Land between the farthest west made by Aldrich in 1876 and that made by Sverdrup who came up from the south-east in 1900.

On June 28th he climbed a peak, towering 1600 feet, on Axel Heiberg Land which he named Cape Thomas Hubbard after the President of the Peary Arctic Club. It was from this peak that he thought he saw what he later named Crocker Land. Returning to the winter quarters of the *Roosevelt* he found that we had broken the ship out of the ice at Cape Sheridan and had worked our way down to Cape Union. Here we were driven on shore by ice pressure and lost our rudder and two blades of the propeller, also tearing away the skeg and doing other damage to the ship.

It was a very difficult job working the ship out of the ice with a jury-rudder but we finally got her down into open water and headed south. From here on the trip to New York developed into a classic of arctic adventures. During many hard fall gales the ship was unmanageable among the icebergs and blinding snow in her crippled condition but we finally got her through and reached New York on Christmas Day. We had left our winter quarters in July.

Peary left on his eighth and last expedition toward the North Pole July 6, 1908. The *Roosevelt* left her pier in New York and steamed up the Sound to Oyster Bay where President Theodore Roosevelt came aboard and bade Peary Godspeed on what

turned out to be his final and successful venture. I had command again and the crew was composed of hardy Newfoundlanders. Ross Marvin was with us once more as Chief Expedition Assistant and George Wardwell was Chief Engineer. Matt Henson went with us again as he had been with Peary on all his trips. The new members were Donald B. MacMillan, George Borup and Dr. J. W. Goodsell, who was our surgeon.

We reached Cape York on August 1st and met the Eskimos, who were delighted to see Peary again. At Etah we coaled from our auxiliary ship *Eric* and took aboard 49 Eskimos and 246 dogs. We already had 70 tons of whale-meat which we had picked up on the Labrador coast. We added 50 walrus carcasses to the lot so we had quite a load.

Steaming northward we encountered better luck than the year we went north on our former voyage and after passing through Smith Sound, Kane Basin, Kennedy and Robeson Channels reached Cape Sheridan on September 5th, our former winter quarters. The ice in Robeson Channel gave us some trouble but we didn't spare the *Roosevelt*—and she could take it. Peary left the ice navigation to me and I knew what he wanted. I did my best which was all that lay in my power to help him in every way I could.

We were fortunate in being able to put the ship in a good berth safe from harm. The worst that could happen would be fire and to safeguard the expedition we put the supplies ashore which were in tin boxes, and these we put into wooden boxes. The wood and tin we could use very effectually later on. If the ship should be destroyed we could walk home, and it would not interfere with the sledge work nor seriously cripple the expedition. We planned to spend the winter in the box houses which we constructed and make the dash for the Pole in the spring. Hunting parties were sent out and a lot of meat obtained before darkness came upon us. We sledged supplies up to Cape Columbia and did tidal work. As the winter settled down upon us many of the dogs began to perish and this began to look serious but luckily our walrus meat saved the day.

At length the winter passed, seemingly very quickly. Having been kept in the field on sledge journeys we were all in good shape physically and ready for the hard work ahead. On the night of February 14th Peary had a talk with the Eskimos and

outlined his plans. He told them of the gifts they would receive if they were faithful. He did this for the sheer love of seeing them show their deep feelings and respect for his word and skill in bringing them back alive so many times. Writing of the essential points in successful Polar work, Peary gave the following résumé:

"To drive a ship through the ice to the farthest possible northern land base from which she can be driven back again the following year.

"To do enough hunting during the fall and winter to keep the party healthily supplied with fresh meat.

"To have dogs enough to allow for the loss of sixty per cent of them by death or otherwise.

"To have the confidence of a large number of Eskimos, earned by square dealing and generous gifts in the past, so that they will follow the leader to any point he may specify.

"To have an intelligent and willing body of civilized assistants to lead the various divisions of Eskimos—men whose authority the Eskimos will accept when delegated by the leader.

"To transport beforehand to the point where the expedition leaves the land for the sledge journey, sufficient food, fuel, clothing, stoves (oil or alcohol) and other mechanical equipment to get the main party to the Pole and back and the various divisions to their farthest north and back.

"The have an ample supply of the best kind of sledges.

"To have a sufficient number of divisions, or relay parties, each under the leadership of a competent assistant, to send back at appropriate and carefully calculated stages along the upward journey.

"To have every item of equipment of the quality best suited to the purpose, thoroughly tested and of the lightest possible weight.

"To know, by long experience, the best way to cross wide leads of open water.

"To return by the same route followed on the upward march, using the beaten trail and the already constructed igloos to save time and strength that would have been expended in constructing new igloos and in trail-breaking.

"To know exactly to what extent each man and dog may be worked without injury.

"To know the physical and mental capabilities of every assistant and Eskimo.

"Last but not least, to have the absolute confidence of every member of the party, white, black, or brown, so that every order of the leader will be implicitly obeyed."

It was planned that my division was to pioneer the trail toward the Pole, and keep one day ahead of the main party. I had

three Eskimos, three sledges and dogs and five days supplies. Borup's division was composed of three Eskimos and four sledges with dogs, carrying standard loads. Our daily rations consisted of one pound of pemmican, one pound of ship's biscuits, a half pound of compressed tea, four ounces of condensed milk and for fuel six ounces of liquid alcohol. We ate two meals a day. The dogs had a pound of pemmican each per day but sometimes we gave them double rations.

Peary left Cape Columbia April 28th with all the other divisions, viz., the Doctor, Marvin, Borup, MacMillan and his own party. Two marches out they ran into open water which held them up. They were now beyond the British record of 83.20° North lat. and a sounding by Marvin gave 96 fathoms.

Borup caught up with me when we were five marches out from the land and I sent him back, telling him that he would surely meet the Commander if he kept to the marked trail. However he missed Peary in the rafters and when Peary picked up the trail on the north side of the lead he saw that Borup had gone by him so he sent Marvin and the Eskimo Kyootah back over the trail to the land. It was a fortunate move as Marvin, with his past experience and knack of handling Eskimos, would be able to bring them out again if they showed signs of nervousness. It turned out that he had to use quite a bit of coaxing with some of them.

On March 4th I was held up by open water and the next day Peary came in with the main party. Thus we all faced the Big Lead which the westerly wind kept open. I made a sounding and got 110 fathoms. We were now about 45 miles north of Cape Columbia and on the edge of the Continental Shelf.

Five days of inaction followed while we watched the lead grow wider. Peary and I said little to each other. Silence was better. I kept walking along the edge of the lead studying its movements. Often I climbed to the tops of the rafted ice and looked to the south for signs of Marvin and Borup. They were to bring the alcohol we needed for our fuel. Peary had devised an alcohol cooker for the 1906 expedition which we were using with great success.

Once in a while Peary and I talked about what we would do if the lead should close before Marvin contacted us. Two of the Eskimos began to get nervous so Peary sent them back with a

note for Marvin but the lead closed shortly after they had left and we went on, leaving a note for Marvin telling him how vital it was for him to come on and overtake us. We also told him not to camp on the south side of the lead. The ice was in motion and we encountered many more leads but got across them. The thermometer registered 53° F. below zero which kept the leads closed with leathery salt-water ice which is easier to cross on compared to fresh-water ice which is crisp and breaks easily. The little brandy we had in case of accidents froze solid and our petroleum turned white and viscid. Our dogs were covered in a white cloud of their own breath.

Just as we had finished building our igloos on March 13th we heard the Eskimos yelling "Kling-mik-sue!" (Dogs are coming). Later Seegloo of Borup's party came in with a note from Marvin saying that they would be at our camp the next day. Here the Doctor was informed that he was to return, the first party to go back. How we all slept that night! We now felt that we had gotten the break. On the evening of the 14th Marvin came in and we were certainly glad to see him. He made a sounding and got 820 fathoms. I was delighted for I knew then that we were across the Big Lead. Our latitude was about 84° North.

On March 15th Marvin's and my divisions left camp early and MacMillan left for Cape Columbia, the second supporting party to return. We plodded along toward the north day after day with the ice twisting and changing, now rough, now smooth. Five marches from where MacMillan returned Borup was sent back. This was at latitude 85.23° North. Peary regretted to see the young Yale athlete go but he had frozen his feet and, notwithstanding all his enthusiasm, Peary did not want him to be subjected to further risks amongst the treacheries of the ice. I, too, was sorry to see him go for he was a great big, fine, two-fisted lad.

On and on we went but the going began to get better and our spirits rose. The returning sun and thoughts of the prize ahead gave us renewed vigor. Each division now comprised three men instead of four. Continuous daylight permitted a modification of the previous arrangements. From now on Henson's party traveled with mine and Marvin and his men came along with the Commander. Our two parties were in touch with each other every twenty-four hours. This relieved the Eskimos of all ap-

prehension. It also reduced by fifty per cent the chances of separation from open leads.

When we reached 86° North, Marvin made a sounding getting 310 fathoms. I was very much surprised at this. Fifteen miles further he got 700 fathoms and no bottom—and the wire parted. On the 22nd I made a good march of fifteen miles. It was hard going at first through rough ice. The Eskimos complained a lot but they got relief by breaking the sledges and delaying our march. I kept my temper, however, as we were all working hard and keyed up and not getting much rest. I realized what a stake we were striving for and that the Eskimos could not be expected to understand it all. I could walk easily three miles an hour on snow-shoes and many times I found myself almost 20 miles ahead of them. Then I would return and find them repairing a sledge. This meant a rest for them. I knew, however, that Peary would understand when he came up. That was the way with this great leader.

No observations had been made recently as the sun was so low but we could estimate fairly well our daily marches. Our previous experience on the Polar Pack had taught us how to do this. Peary had done it on the Ice Cap and I had learned to judge distances while seal hunting on the ice off Newfoundland. In thick weather there I had often nothing to guide me but dead-reckoning. When Marvin came up he took an observation which gave 85.48° North lat.

On the 25th of March we built our igloos and turned in very tired and sleepy. Peary came in around ten o'clock and we got up and made hot tea and ate some biscuits. I put all hands to work repairing sledges and Marvin made another observation which gave us 86.38° North lat. We were now beyond the Italian record. At four P.M. I got away again but before I left I had a long chat with Marvin. I did not realize that this was to be our last talk together. We knew each other very well as we had been shipmates before and it was I who had persuaded Peary to take him north again. He was a splendid fellow and had a good position as teacher of Civil Engineering at the Mercersburg Academy. We shook hands and I said: "Take care of yourself and be sure not to sleep on young ice".

I had made up my mind that if it was humanly possible I would get to 88° North. The light was bad but the floes over

which we were traveling were very level. The sledges were heavy and the dogs had eaten extra pemmican but the Eskimos were very loath to step up the speed. I was just rearing to go. The sun was obscured and a haze spread over the sky. The shadowless light ice and the sky appeared to meet but this did not bother me as all I thought about was to get to 88° North. During the last quarter of the march that day we ran into open water but we got around the leads and went into camp and built our igloos. I hated to do it but the Eskimos were not feeling very keen about going on.

After a five hour sleep I turned all hands out and we were just getting away when Peary came in. The sun was shining gloriously and the sky was a marvellous blue. I wore snow-glasses so did not mind the glare on the snow. Next day's journey, March 27th, was a hard one. We ran into much rafted ice and later heavy rubble. The light snow did not hold us up and we often sank into places that seemed bottomless. I saw the tracks of two white-foxes 240 nautical miles from the land and observed a seal in the water. We built our igloos amongst a heap of rubble ice and turned in but it seemed only a short while before Peary came in. My Eskimos said that they wanted more sleep so I told Peary how they felt and he said it was all right. Somehow I couldn't sleep myself as that 88 mark kept rising up before me. Finally the Eskimos were satisfied and we got away. Peary had lightened the loads on the sledges while we were resting and this made things much easier. We made wonderful time and I fully intended to travel as long as I could get the Eskimos to go. They told me they would go as long as I would and I believe that we would have made fifty miles on that march if open water had not stopped us. I was asleep when Peary's division arrived. He built his igloos a half-mile away so as not to disturb us. Shortly after this the ice began moving and I got up and had breakfast. Going outside I felt that everything was moving and I saw open leads all around. Peary and his men were adrift and the air was filled with vapor. The dense clouds of condensation seemed as black as a prairie fire. I made a sounding of 1260 fathoms. The next day, March 29th, we were still held up by open water but around 7 A.M. on the 30th the ice became quiet and the fog began to dry up. We were soon away and all that day we traveled together, crossing one lead of four

or five miles on young ice. After making 20 miles we built igloos and turned in. I knew the next march was to be my last and I hoped that all would be well.

The next day the going was good and the weather thick. A strong northerly wind was blowing which was better than if it had been blowing from the east or west as there would not be any lateral changes in the ice. But it would rob me of distance. I knew, however, that it would close up the leads to the south and help Peary on his return trip. Once in a while on that march Peary would walk along with me. I always wore snow shoes which made it so much easier and comfortable; Peary walked well and did much walking. He was a man of steel but he knew that I was anxious to make a long march and left me alone. I began to realize, however, that the strong north wind had hindered me from getting to 88° North. The thick weather bothered me a lot, too. I did hope that it would clear as I wanted to make at least 25 or 30 miles. We finally built our igloos after we had gone 20 miles. Just as we had finished them the sun came out.

Peary now told the two Eskimo lads, Keshingwah and Karko, that they were to go back with me the next day. The wind continued to blow strongly from the north while we slept. Peary turned us out after five hours of sleep. After breakfast I went outside the igloo and Peary and I talked together. He said: "Bartlett, I am afraid the north wind has robbed you of the 88th degree". I said: "Yes sir, I think so". Then he suggested that I walk a few miles beyond as our meridian altitude could not be obtained until noon and it still lacked several hours of that time. So I put on my snow-shoes and left. The wind was still strong from the north. The snow was blowing over the level surface of the floes about six or eight inches high. On and on I went but the wind slowed me down. I knew that I had to be back for the noon observation so I turned around after going about seven miles. I first knelt down and thanked God for having given me the health and strength to reach a spot so far north and I asked Him to help Peary to reach the Pole, and to return safely to the ship. I then made the sign of a Master Mason on the snow and laid near it a Newfoundland flag.

With the wind at my back I reached camp a half-hour before noon. During the time I was making the observation, Ootah kept hanging around. When he saw me leave the lean-to of

snow blocks that Peary had the Eskimos build as a shelter for me while I worked he came up to me and said in Eskimo: "How many sleeps before North Pole reached?" I told him we were now about as far from the Pole as we were from the place where Marvin turned back. He said: "Ice good, no open water; we can do it in one sleep". His idea was that by feeding the dogs well before leaving this place and by riding on the sledges and going light it could be done. They were all heart and soul in the next get-a-way for the Pole.

Handing my calculations to Peary he looked at them and said that he was sorry that I had not made 88° North. Had I asked him then I could have gone on. He would have taken me but I did not want to go. I realized then as now that he knew where I could serve him best, and I am fully satisfied. To me it was a glorious adventure to have served such a heroic and capable leader.

So from 87.47° North lat., on April 1st, 1909, I started back with two Eskimos, one sledge and eighteen dogs. From here to the land it was big travel and small sleep. Many long hours we marched, trying to knit together the old trail. Sometimes we were successful and sometimes we lost it for awhile but we kept on going. On one occasion we traveled 48 hours without sleeping. I fell in the water once when it was 35 degrees below zero F. but my bear-skin trousers and muskox kamiks saved me from freezing. Luckily I did not go all the way in. By rolling in the snow and beating the moisture out of my fur garments with a stick we always carried for that purpose I got fairly dry.

When we were a hundred and twenty miles from land we saw the two peaks of the Cooper Key Mountains behind Cape Columbia rising on the southern horizon; and this encouraged us a lot. We reached the land on April 16th and arrived at the ship a few days later. For a week we did nothing but sleep, eat and sleep again.

Peary reached the Pole on April 6, 1909. His return to the land was accomplished in rapid time with light sledges and good weather conditions. When he arrived at the ship I told him that Marvin had been drowned. It was a sad loss to us all.

During the months of May and June we made many trips along the coast and went inland collecting eggs and birds. Some

went to Cape Morris K. Jessup for tidal observations. Others to Fort Conger and Cape Columbia, where cairns were built.

We left our winter quarters at Cape Sheridan on July 18th and on August 8th we were out of the ice off Cape Sabine. Stopping at Nerke we reached Etah on the 17th where we picked up Boson Murphy, Harry Whitney, a sportsman who had spent the winter there as a guest of the expedition, and Billy Pritchard, our messroom boy who had been guarding our cache of supplies and is now steward aboard my schooner *Morrissey*. We gave all the supplies that were left over to the Eskimos and spent several days securing about 50 tons of walrus meat for them.

We left Cape York August 26th and arrived in New York late in September.

INDEX